

# Moved to Opportunity:

## The Long-Run Effect of Public Housing Demolition on Labor Market Outcomes of Children

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### Abstract

This paper provides new evidence on the effects of moving out of disadvantaged neighborhoods on the long-run economic outcomes of children. My empirical strategy is based on public housing demolitions in Chicago which forced households to relocate to private market housing using vouchers. Specifically, I compare adult outcomes of children displaced by demolition to their peers who lived in nearby public housing that was not demolished. Displaced children are 9 percent more likely to be employed and earn 16 percent more as adults. These results contrast with the Moving to Opportunity (MTO) relocation study, which detected effects only for children who were young when their families moved. To explore this discrepancy, this paper also examines a housing voucher lottery program (similar to MTO) conducted in Chicago. I find no measurable impact on labor market outcomes for children in households that won vouchers. The contrast between the lottery and demolition estimates remains even after re-weighting the demolition sample to adjust for differences in observed characteristics. Overall, this evidence suggests lottery volunteers are negatively selected on the magnitude of their children's gains from relocation. This implies that moving from disadvantaged neighborhoods may have substantially larger impact on children than what is suggested by results from voucher experiments where parents elect to participate.

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# 1 Introduction

Over the past three decades, the number of U.S. children living in high-poverty neighborhoods has grown by nearly 80 percent to reach 20 million (Bishaw, 2014; Census, 1990). This increase has renewed concern over the long-run consequences of growing-up in disadvantaged areas. Theory suggests that a child’s odds of success are reduced if they live in impoverished neighborhoods where most adults are unemployed and peers engage in criminal activity (Wilson, 1987; Sampson and Groves, 1989; Massey and Denton, 1993).

Yet, quantifying the causal impact of neighborhoods on children has proven difficult largely because unobserved factors influence both selection into neighborhoods and individual outcomes. The best evidence to date comes from the Moving to Opportunity (MTO) experiment which randomly allocated housing vouchers to a sample of families living in low-income public housing. Recent analysis of MTO detected a positive impact on adult labor market outcomes for children who were young when their families moved and no detectable effect for older children (Chetty et al., 2016). While MTO provides important and credible estimates of neighborhood effects, some researchers have expressed concern that findings based on a group of motivated volunteers may not generalize.<sup>1</sup> For example, Durlauf (2004) writes: “[O]ne cannot extrapolate the [MTO] findings to the broader population of the poor.”

This paper provides new evidence on the impact of neighborhood conditions on a general population of disadvantaged children by exploiting public housing demolitions as a natural experiment. During the 1990s, the Chicago Housing Authority (CHA) began reducing its stock of public housing by selecting buildings with poor maintenance for demolition while leaving nearby buildings untouched. Residents of buildings selected for demolition received housing vouchers and were forced to relocate.

Public housing demolition in Chicago provides plausibly exogenous variation in the neighborhoods where displaced children grow up. My research design compares adult labor market outcomes of children from displaced households to their non-displaced peers that lived in the same public housing development. Because these two groups of children and their households were similar before

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<sup>1</sup>The MTO researchers have been careful in discussing the issue of external validity. Ludwig et al. (2001) write: “We hasten to note that because participation in the MTO program is voluntary, our estimates of the effects of relocation may be different from the effects of relocating a randomly selected group of families from poor areas.”

the demolition, any differences in later-life outcomes can be attributed to neighborhood relocation (Jacob, 2004).

Novel administrative data from Illinois makes it possible to match social assistance records to pre-demolition addresses of public housing to create a sample of displaced and non-displaced children and their households. In addition, the address information in assistance records allows me to verify that displaced households relocated to less disadvantaged neighborhoods. Three years after demolition, displaced households lived in neighborhoods with 20 percent lower poverty and 33 percent less violent crime relative to non-displaced households.<sup>2</sup>

Using employment data linked to the social assistance records, I find that displaced children grow up to have notably better labor market outcomes. Displaced children are 9 percent (4 percentage points) more likely to be employed as adults relative to their non-displaced peers. Further, displaced children have \$602 higher annual earnings – an increase of 16 percent relative to their non-displaced counterparts. The trajectory of these benefits is relatively stable across the range of adult ages that I observe for my sample of children.<sup>3</sup>

The estimates in this paper contrast with previous findings in the neighborhood effects literature.<sup>4</sup> Unlike the recent extended analysis of MTO that revealed earnings benefits for the subgroup of children who were young (below age 13) when their families moved, this paper detects a positive impact regardless of the age when a child’s family moves.<sup>5</sup> Moreover, these benefits accrue to displaced children whose family received standard vouchers, whereas the MTO study finds effects only for children receiving vouchers that were restricted for use in low poverty areas.

To understand these differences in results, I analyze a housing voucher lottery that occurred in Chicago shortly after the demolitions began. Similar to MTO, authorities in Chicago randomly allocated vouchers to households that were sufficiently motivated to sign up for this program.

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<sup>2</sup>This change in neighborhood conditions is similar to the pattern for MTO volunteers that were randomly selected to be part of the unrestricted (“Section-8”) housing voucher treatment group.

<sup>3</sup>In the last year of my administrative data, individuals in my sample were between ages 21 and 32 and experienced demolition when they were 7 to 18 years old. The results are not sensitive to including even younger children.

<sup>4</sup>This paper also relates to previous work on the impact of Chicago’s public housing demolitions. I extend Jacob (2004) which studies the short-run impact of public housing demolition on schooling outcomes.

<sup>5</sup>Note that Sanbonmatsu et al. (2011) also analyzed children who participated in MTO finding no detectable impact on labor market outcomes. The more recent MTO analysis by Chetty et al. (2016) differs from this previous work by focusing on younger (below age 13) children who were not old enough to have completed their education at the time that data was collected for Sanbonmatsu et al. (2011). In other major work on neighborhood effects, Oreopoulos (2003) found no improvement in labor market outcomes for adults that grew-up in very different public housing projects in Toronto.

While families that won the lottery moved to lower poverty neighborhoods, the effects of winning a voucher on labor market outcomes of children are small and not statistically significant.<sup>6</sup> The contrast between the effects in the lottery and demolition samples remains even after re-weighting the demolition sample to adjust for the fact that lottery households are relatively more advantaged.

Comparing the lottery and demolition results suggests that children who move through voluntary mobility programs have lower gains to relocation. This “reverse Roy” pattern of negative selection parallels education studies which find that children who would benefit most from attending charters or other high-quality schools are the least likely to apply (Walters, 2014; Hastings et al., 2006). In addition, these results are consistent with recent analysis that suggests children with the largest achievement gains from Head Start are the least likely to enroll (Kline and Walters, 2015).

I conclude that there are significant benefits to relocating children (of any age) from public housing. Back-of-the-envelope calculations suggest that a child who moves out of public housing due to demolition has about \$45,000 higher total lifetime earnings (\$12,000 in present value). The increased tax revenue associated with this earnings gain exceeds the (average) cost of relocating public housing residents. This suggests that efforts to improve long-run outcomes of disadvantaged children will yield net gains for government budgets.

## 2 History of Public Housing Demolition in Chicago

During the 1990s, the Chicago Housing Authority (CHA) had the third largest public housing inventory in the U.S., providing services to nearly five percent of the city’s population (Popkin et al., 2000; Jacob, 2004). Seventeen housing developments (also known as “projects”) spanned the city providing homes specifically for families with children. Each project consisted of a collection of apartment buildings built in close proximity. Many of these buildings were large high-rise structures providing 75 to 150 units of housing.

Low-income households were eligible to live in public housing if their income was at or below 50 percent of Chicago’s median income. Because public housing is not an entitlement, eligible families typically spent years on waiting lists and usually accepted the first public housing unit that was

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<sup>6</sup>To compare the effects on adult labor market participation in the lottery and demolition samples, I test that the difference in point estimates is zero, and the resulting  $p$ -value is 0.13. Results for labor market earnings are much less precise in the lottery sample, and a test of the difference in point estimates across samples has a  $p$ -value of 0.43.

offered to them.<sup>7</sup> The vast majority of public housing residents during this period in Chicago were African-American, and a large share were single-parent, female-headed households.

The demolition of public housing in Chicago during the 1990s began as a reaction to serious housing management problems.<sup>8</sup> By the end of the 1980s, chronic infrastructure problems plagued much of the public housing stock which had been built poorly during the 1950s and 1960s. Few city officials believed that the CHA could effectively address these maintenance issues after a series of scandals revealed that housing authorities had mismanaged public funds. With this in mind, authorities laid plans to replace project-based housing assistance with vouchers and gradually eliminate public housing through building demolition.<sup>9</sup> Although the city wanted to eventually eliminate much of the housing stock, funding limitations implied that only a small number of demolitions occurred in the 1990s.

The first demolitions in Chicago stemmed from a variety of events and circumstances that were sometimes unforeseen (Jacob, 2004).<sup>10</sup> For example, in January 1999 pipes burst in several high-rise buildings in the Robert Taylor projects causing flooding that shut down heating systems. Residents were forced to evacuate four buildings, and the CHA subsequently closed these buildings for demolition (Garza, 1999a). Similarly, harsh winter weather damaged several buildings in the Henry Horner Homes project, prompting the CHA to close these buildings for demolition (Garza, 1999b). When not reacting to specific crises, the CHA generally sought to close buildings that had the most severe maintenance issues.<sup>11</sup>

When the CHA selected a building for demolition, the authority provided Section 8 housing

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<sup>7</sup>For example, over 30,000 households were on the CHA public housing waiting list during the mid-1990s (Jacob, 2004). Households must wait as their request for housing assistance rises to the top of the queue. Once at the top of the list, a household that is unsatisfied with their offer can reject their assignment, but they must then return to the bottom of the wait list. Further, due to the same high demand for services, there is also little opportunity to transfer between housing units after entering the public housing system.

<sup>8</sup>For a detailed history of public housing in Chicago, see Popkin et al. (2000).

<sup>9</sup>A number of federal housing policy reforms also facilitated public housing demolition in Chicago. Specifically, the creation of the HOPE VI program in 1993 helped provide funding for demolition. The CHA was one of the largest recipients of HOPE VI funding, receiving nearly \$160 million during the 1990s.

<sup>10</sup>Note that the analysis in this paper only pertains to building demolitions that occurred from 1995-1998 which preceded public housing demolitions under the Chicago Housing Authority's "Plan for Transformation". This focus on demolitions during the 1990s allows me to study long-run outcomes and also corresponds to the period studied in Jacob (2004).

<sup>11</sup>An additional motivation for building closure was related to criminal activity. For example, snipers located on the roof of a Cabrini Green building shot 7-year-old Dantrell Davis in 1992. The building from which the shots were fired was permanently closed after the shooting and later demolished in 1996 (Hawes, 1992). I exclude the projects and the buildings where these crime-based closures occurred from the analysis in this paper.

vouchers to displaced residents which allowed recipients to rent housing on the private market.<sup>12</sup> Alternatively, residents of affected buildings were also given the option of applying to transfer to another unit in their current project or transfer to another unit in a different CHA project. In the case that a resident selected the voucher offer, the CHA paid for all moving expenses. Note that the CHA provided few support services such as housing counseling during the period of my study.<sup>13</sup>

Typically, the voucher subsidy was equal to the difference between the gross rent or the local Fair Market Rent (FMR) and the family's required rent contribution (30 percent of adjusted income). The FMR was equal to the 40<sup>th</sup> percentile of the local private-market rent distribution. Families that obtained a housing voucher were able to keep the voucher as long as they remained eligible for assistance. Finally, the transition to vouchers from public housing should *not* mechanically affect income of assisted households because the program and rent rules for vouchers and project-based assistance were similar.<sup>14</sup>

### 3 Expected Effects of Demolition on Children

The expected effect of public housing demolition is related to the relocation decision of affected households. One possibility is that displaced households used their vouchers to move to lower poverty neighborhoods. In this case, theory suggests that children may benefit because relatively affluent adults serve as role models that shape norms and social identity (Wilson, 1987; Akerlof and Kranton, 2000). Living in a less disadvantaged neighborhood can also affect a child's long-run outcomes by providing exposure to higher-income peers that can provide job information and referrals.<sup>15</sup> Relatedly, lower poverty areas may provide displaced parents with better access to job-finding networks, which implies that they may be more likely to work and invest in goods that promote child development.

While these mechanisms suggest children should benefit from moving to low-poverty neighbor-

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<sup>12</sup>Prior to the beginning of Chicago's public housing demolitions, low-income households had no ability to access housing vouchers because the CHA stopped allowing requests in 1985 (Jacob and Ludwig, 2012).

<sup>13</sup>Later initiatives by the CHA included providing relocation support to residents who were displaced by building demolitions that occurred during the 2000s (Popkin et al., 2012).

<sup>14</sup>For the interested reader, a full list of program rules for housing vouchers is contained in Appendix Section A3.

<sup>15</sup>In addition, living in a lower poverty areas may reduce a child's exposure to peers who commit crime. This is particularly relevant in the context of Chicago where many poor neighborhoods also have very high rates of crime. Damm and Dustmann (2014) provide evidence that a child's long-run outcomes are causally affected by the share of youth criminals living in their neighborhood.

hoods, existing empirical studies provide mixed support for this prediction. For example, analysis of Chicago’s Gautreaux program found that low-income children who moved to the suburbs had much better outcomes than their peers whose families moved within the city (Rosenbaum, 1995).<sup>16</sup> However, Oreopoulos (2003) did not detect any impact of living in a lower poverty area for children living in public housing in Toronto. In addition, Chetty et al. (2016) find no evidence of benefits for children who were older when their family moved to a low-poverty area through MTO. In relation to the present paper, the MTO results are the most relevant comparison given its similar focus on distressed, high-crime public housing in major U.S. cities.<sup>17</sup> As mentioned previously, the results from MTO may differ from the demolition context because MTO families volunteered to participate.

Another way in which demolition and relocation may affect children is through changes in the quality of schooling. A variety of studies provide credible evidence that attending schools with better teachers and smaller classes generates notable gains, especially in terms of long-run labor market outcomes (Chetty et al., 2011; Fredriksson et al., 2013; Fryer and Katz, 2013; Chetty et al., 2014). Yet, previous studies of housing assistance programs suggest that families typically *do not* use vouchers to relocate to areas that give their children access to better schools. In the MTO evaluation, there was little impact on child school quality although households moved to areas with notably lower poverty (Sanbonmatsu et al., 2006). Moreover, the most relevant evidence for the demolition context comes from Jacob (2004), who studied the short-run impact of public housing demolition on education outcomes. He finds that there is little difference in school quality for displaced and non-displaced children after relocating due to demolition.<sup>18</sup>

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<sup>16</sup>The Gautreaux program provided housing vouchers to a limited set of low-income families in Chicago in the late 1970s. As part of this voucher program, counselors recommended apartments in lower-poverty areas of Chicago or the surrounding suburbs. Researchers studying the Gautreaux program argue that this process resulted in a quasi-random assignment of households to new neighborhoods. In support of this argument, Popkin et al. (1993) report that 95 percent of program participants accepted the first apartment offered. However, Shroder and Orr (2012) argue that it is unclear whether the Gautreaux setting approximated random assignment because there are statistically significant differences in observed characteristics between the set of participants that moved to the suburbs or within Chicago.

<sup>17</sup>There are notable differences between public housing neighborhoods in the U.S. and Canada (Oreopoulos, 2003, 2008, 2012). For instance, Oreopoulos (2012) notes that “[a]lthough large public housing projects in Toronto are unattractive, they do not exhibit nearly the same degree of crime and racial segregation that occur in high-poverty neighborhoods in the United States.” This suggests that the effects of public housing in Toronto may not apply to the U.S. context.

<sup>18</sup>As will be discussed in Section 7, Jacob (2004) also finds few detectable effects on student outcomes such as test scores and grades. This suggests that an impact on long-run child outcomes is not due to changes in schooling achievement.

Finally, displaced children may have different outcomes even if their households did not relocate to less disadvantaged neighborhoods. One idea proposed by sociologists posits that the physical design and density of public housing projects fosters criminal and other negative behavior (Newman, 1973). Hence, relocation to less-dense private market housing can impact child development irrespective of changes in exposure to neighborhood poverty.

## 4 Data Sources and Sample Construction

The data that I use to test whether demolition has an impact on long-run outcomes of children is drawn from multiple administrative sources. Specifically, I combine building records from the Chicago Housing Authority (CHA) and social assistance (i.e., TANF/AFDC, Food Stamp and Medicaid) case files (1989-2009) from the Illinois Department of Human Services to create a sample of children that lived in public housing and were affected by demolition during the 1990s. I obtain additional information on long-run outcomes by merging this sample of children with unemployment insurance wage records (1995-2009) from the Illinois Department of Employment Security. In addition, I also obtain measures of baseline (prior to demolition) characteristics by linking the sample to arrest records from the Illinois State Police.<sup>19</sup> The employment and police data are linked to the social assistance case records using identifiers created by Chapin Hall at the University of Chicago, a research institute and leader in administrative-data linking. In the next sections, I describe my sample and the data construction in more detail.

### 4.1 Sample of Public Housing Buildings

My analysis focuses on a subset of public housing projects and buildings listed in CHA building inventory records from the 1990s. Specifically, I examine non-senior-citizen projects that experienced building closure and demolitions from 1995-1998 which were part of the initial wave of housing demolitions associated with HOPE VI grants.<sup>20</sup> I also restrict attention to high-rise buildings (defined as having 75 units or more).<sup>21</sup> Finally, I exclude projects where documented evidence

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<sup>19</sup>I also use the police data for supplementary analysis of adolescent criminal behavior.

<sup>20</sup>Note that I exclude high-rise projects that did not have any buildings closed due to demolition. This is because my empirical specification includes project fixed effects to account for systematic differences *across* projects. Hence, projects that did not experience demolition would not contribute to the identification of the impact of demolition.

<sup>21</sup>In general, low- and mid-rise buildings did not experience the same type of sudden and abrupt demolition process as observed in high-rises.



suggests that building demolition was correlated with unobserved tenant characteristics. Specifically, I exclude the Cabrini Green and Henry Horner projects.<sup>22</sup>

The final sample contains 53 high-rise buildings located in seven projects. I obtain the date when a building was closed from Jacob (2004) which determined the year of closure by examining CHA administrative data on building occupancy supplemented by qualitative sources.<sup>23</sup> Based on this information, there were 20 demolished (treated) and 33 stable (control) buildings during my study period. Note that stable buildings are defined as those that did not close during the 1995-2002 period.

## 4.2 Linking Households to the Public Housing System

To create my main analysis sample, I exploit the fact that social assistance records provide the exact street address for welfare recipients. Specifically, I link welfare recipients who have a street address that matches a building in my public housing project sample in the year prior to notification of demolition. Note that by focusing on addresses in the assistance data in the year *before* demolition, the sample definition is unrelated to any impact that demolition has on public assistance participation. Overall, the assistance data contains 5,676 adult recipients that live in public housing in the year before demolition. Since the sample of public housing buildings contains 7,770 individual apartments, this suggests that my assistance sample covers at least 73 percent of the households living in the demolition sample of buildings (assuming there are no vacant apartments). The high coverage rate is not surprising given that the disadvantaged status of the Chicago public housing population implies that many residents will receive some form of public assistance.<sup>24</sup>

Finally, I focus on children aged 7 to 18 in the year that building demolition takes place at their project. With this sample, I observe adult (age > 18) outcomes for at least three years and at most 14 years for each child. Moreover, this age restriction allows me to compare my results directly to the analysis of children in the final impact evaluation from MTO.<sup>25</sup> The final data

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<sup>22</sup>As highlighted by Jacob (2004), the housing authority chose to demolish buildings at the Cabrini-Green Homes that were associated with gang activity and crime. I 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). This non-standard process was due to an earlier lawsuit initiated by the Henry Horner residents.

<sup>23</sup>Appendix Section A4 provides further details on the process for determining the year of demolition.

<sup>24</sup>Only 15 percent of households living in Chicago public housing had an employed member, and the average CHA household income was \$6,936 per year (Popkin et al., 2000).

<sup>25</sup>In Appendix Table A8, I show that my main results are not sensitive to changing the sample definition to include even younger children who will be just entering the labor market in the final years covered by my employment data.

contains 5,250 children from 2,767 households. Using this set of children from project-based public housing, I create a panel at the person-year level which covers the period from demolition to 2009, the last year of my administrative data on labor market and welfare outcomes. The number of observations per individual is determined by the demolition date. For example, residents of projects that had demolitions in 1995 will have 14 observations in their panel. I merge this panel with the administrative data on labor market outcomes, social assistance receipt and criminal arrests.

## 5 Empirical Approach

### 5.1 Estimating the Reduced-Form Effect of Demolition

As in [Jacob \(2004\)](#), I study the impact of demolition by exploiting the fact that the CHA selected a limited number of buildings for demolition within each public housing project during the 1990s. Hence, my empirical strategy compares children who lived in buildings selected for demolition to their counterparts living in non-demolished buildings.<sup>26</sup> The former is the treatment group which is displaced from public housing. To the extent that displaced and non-displaced individuals are randomly assigned within the same project, subsequent differences in outcomes can be attributed to the demolition and relocation.

I use the following linear model to study the impact of demolition on outcome  $y$  for children,

$$Y_{it} = \alpha + \beta D_{b(i)} + X_i' \theta + \psi_{p(i)} + \delta_t + \epsilon_{it}, \quad (1)$$

where  $i$  is an individual and  $t$  represents years. The indexes  $b(i)$  and  $p(i)$  are the building and project for individual  $i$ . The terms  $\delta_t$  and  $\psi_{p(i)}$  are year and project fixed effects, respectively. The vector  $X_i$  is a set of control variables (e.g., age) included to improve precision. The dummy variable  $D_{b(i)}$  takes a value of one if an individual lived in a building slated for demolition. Hence,  $\beta$  represents the net impact of demolition on children's outcomes.

In addition, I also estimate two augmented versions of Equation 1. First, I estimate a model

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<sup>26</sup>For example, in the Robert Taylor Homes project, building #1 was slated for demolition in 1995 while other high-rises in Robert Taylor were left untouched at that time. Residents of this latter group of stable buildings can be used as a comparison group that holds constant characteristics specific to residents at Robert Taylor Homes.

which includes interactions for gender.<sup>27</sup> This analysis is motivated by a large body of previous empirical work that documents significant heterogeneity by gender. For example, many of the benefits detected in the MTO evaluations were found for girls but not for boys (Kling et al., 2005). In addition, Anderson (2008) analyzes data from several education interventions finding that all benefits accrued to girls, with no statistically significant long-term benefits for boys. Second, I also explore how treatment effects vary with age by estimating a model which interacts an individual’s age in a given year and the treatment indicator. Hence, this specification compares displaced (treated) and their non-displaced peers at each age of measurement observed in my sample.

Estimates of  $\beta$  in Equation 1 have a causal interpretation if the CHA’s selection of buildings for demolition was unrelated to resident characteristics. The historical evidence suggests that this condition is plausible because building maintenance issues were the main concern when selecting buildings for demolition. Moreover, there should be little difference between residents living in demolished and non-demolished buildings because the tenant allocation process restricts the ability of households to sort into different buildings. Recall that most families spend years on the public housing waiting list and accept the first unit that becomes available. Finally, in the next section, I provide empirical support for this assumption that building demolition is unrelated to resident characteristics by showing that my sample of displaced and non-displaced residents have similar observed characteristics in the year prior to demolition.

## 5.2 Comparing Treated and Control Individuals in the Year Prior to Demolition

The validity of my research design depends on whether the selection of buildings for demolition was uncorrelated with characteristics of children living in public housing. To provide support for this assumption, I exploit the comprehensive nature of my administrative data to compare children living in buildings marked for demolition (treated) and stable (control) buildings. Specifically, I examine crime and demographic characteristics measured in the (baseline) year prior to demolition.

Table 1 compares children living in treated and control buildings by estimating a regression model where the dependent variable is a child characteristic measured in the year before demolition

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<sup>27</sup>Formally, the augmented model I estimate is:

$$Y_{it} = \beta_g G_i D_{b(i)} + \beta_b (1 - G_i) D_{b(i)} + X_i' \theta + \psi_{p(i)} + \delta_t + \epsilon_{it}$$

where  $G$  is a dummy variable that takes the value of one if individual  $i$  is female. In this equation,  $\beta_b$  is the average difference in outcomes between males in the treatment and control group while  $\beta_g$  is the average difference for females.

and the key independent variable is an indicator for living in a treated building. Column (1) of the table shows means for various outcomes for all non-displaced children living in stable public housing buildings. The second column reports the mean difference between control and treated individuals from the regression model. If selection of buildings was uncorrelated with child characteristics, we expect that the mean difference would equal zero. The table shows that the mean difference is never statistically different from zero for all measures of past criminal activity and demographics in my sample.<sup>28</sup> In addition, Table 1 shows there is no difference in schooling outcomes at baseline by reproducing the balance test results from [Jacob \(2004\)](#), who studied the impact of demolition on schooling outcomes.<sup>29</sup> Columns (3) through (6) similarly show no detectable difference between displaced and non-displaced youth by gender subgroup.<sup>30</sup>

I also examine whether there are detectable differences in the baseline characteristics of adult (age > 18) residents of public housing. One important limitation in this analysis is that the data only includes adults living in public housing who are observed in the social assistance case files. Table 1 examines this sample of adults in Columns (7) and (8) in terms of demographic, criminal and labor market characteristics. Similar to the exercise for youth, Column (7) reports the mean for non-displaced adults measured in the year before demolition. Column (8) reports the difference between displaced and non-displaced adults as calculated from a regression model. Reassuringly, adults living in buildings marked for demolition do not appear statistically different in terms of past criminal activity or labor market activity. Adults in treated buildings are one year older, but the magnitude of this difference is small relative to the mean adult age.

### 5.3 Testing for Attrition and Spatial Spillovers

Administrative data allows me to avoid many concerns over sample attrition and missing data that are problematic in other studies. If an individual works in the state of Illinois in any quarter from 1995 to 2009, I observe earnings as reported to the Illinois unemployment insurance (UI)

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<sup>28</sup>Note that juvenile arrests come from the Illinois State Police (ISP) data. Prior to 1998, the arrest data for juveniles in the ISP data is limited to serious felonies. After this date, revisions in the Illinois Juvenile Court Act allowed for the submission of juvenile misdemeanor arrests into the ISP database, resulting in more complete coverage of juvenile criminal activity.

<sup>29</sup>The sample of treated and control public housing buildings used in this paper matches the sample used in [Jacob \(2004\)](#) except I exclude buildings from the Henry Horner project where there was a distinct process for public housing demolition. All of my results are robust to including Henry Horner in my analysis.

<sup>30</sup>For schooling outcomes, there are no gender specific balance tests because [Jacob \(2004\)](#) did not provide balance tests for schooling outcomes separately by gender.

program. At the same time, one may be concerned that my estimates are biased if children displaced by demolition are more likely to move out of state. In this case, my administrative data would suffer from a missing data problem: an individual who moves out of state will have zero earnings in the Illinois data even if they are working in their new state of residence.

To address concerns over attrition, I follow [Grogger \(2013\)](#) and use terminal runs of zeros to measure permanent out-of-jurisdiction attrition. Intuitively, the idea is that attrition has a distinctive pattern: when an individual moves out of state, all of their subsequent entries in administrative panel data from their original location are zeros. As detailed in Appendix Section A1, I construct this measure of attrition based on terminal zeros and test for imbalance across treatment and control groups. This analysis reveals no evidence that children displaced by demolition are any more likely to attrit from the administrative data than non-displaced children.

A final concern for my empirical results are spatial spillovers stemming from demolition. In other words, the control group of non-displaced children could be affected by the demolition of neighboring buildings and the relocation of their peers. This would bias my estimates upward if non-displaced children are worse off due to demolition. To test for the existence of spillovers, I augment Equation 1 with additional indicators for living in a stable building that is adjacent to a demolition building,

$$Y_{it} = \alpha + \beta' D_{b(i)} + \pi N_{b(i)} + X_i' \theta + \psi_{p(i)} + \delta_t + \epsilon_{it}, \quad (2)$$

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. This specification tests for spillovers on stable buildings under the assumption that social interactions between buildings within a project decrease with distance. Appendix Table A2 presents the results of this specification for labor market and welfare outcomes. I find no evidence of this type of spillover given that the analysis fails to reject the null  $\pi = 0$  across outcomes.

I also test for the existence of general spillovers at the project level. For instance, a concern is that demolition reduced social cohesion in all remaining (control) buildings at the project and this

could affect long-run child outcomes.<sup>31</sup> To address this, I conduct an event-study of Census-tract-level homicides as a proxy for social conditions at and around public housing projects.<sup>32</sup> Formally, I use the following specification:<sup>33</sup>

$$H_{it} = \mu_i + \sum_{j=-4}^{-1} \pi_j \mathbf{1}(t - t^* = j) + \sum_{j=1}^{10} \tau_j \mathbf{1}(t - t^* = j) + \delta_t + \epsilon_{st} \quad (3)$$

where the dependent variable  $H_{it}$  is homicide rate for Census tract  $i$  at year  $t$ . The terms  $\mu_i$  and  $\delta_t$  are tract and year fixed effects, respectively. In the notation,  $t^*$  is the year  $t$  in which a particular tract experiences a public housing demolition directly (or there is a demolition within one mile of the tract). The dummy variables  $\mathbf{1}(t - t^* = j)$  indicates that an observation in year  $t$  is  $j$ -periods before or after demolition occurs. For example, the dummy variable  $\mathbf{1}(-1 = j)$  indicates that the observation  $y$  is one year before the policy is implemented.

I restrict the estimation sample to include tracts that (1) that contained public housing with at least one building demolition or (2) are within one mile of a public housing demolition site. The data contain observations that are at most four years before demolition and up to 10 years after a demolition. I choose four pre-periods because the bulk of the demolitions I consider occur in 1995 and my homicide data start in 1991. Note that year effects  $\delta_t$  are identified using data from locations that have not yet or already have had a demolition. The Figure A1 plots the coefficients  $\pi_j$  and  $\tau_j$  along with the 95-percent confidence interval. The results show that there is no detectable impact of demolition on the tract homicide rates following public housing demolition.

## 6 Main Results

Since the expected effects of demolition depend on relocation choices, I begin by showing that displaced households with children moved to less disadvantaged neighborhoods compared to their non-displaced peers. Next, I study labor market outcomes, finding that there is a large positive impact on long-run employment and annual earnings. Yet, there is no detectable impact of demolition

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<sup>31</sup>Previous research on the demolitions in Chicago finds that there is no evidence of this type of negative spillover affecting non-displaced residents. [Aliprantis and Hartley \(2015\)](#) estimate that there is a *decrease* in violent crime after a project experiences building demolition. Yet, in contrast to the present study, they study the impact of demolition on neighborhood-level crime over a longer time period and include demolitions that occurred after 1998.

<sup>32</sup>I focus on homicides because tract-level data on property or other forms of criminal activity are unavailable for Chicago for my period of study from 1995-1998.

<sup>33</sup>See [Jacobson et al. \(1993\)](#) for further discussion of event-study specifications.

on any measure of public assistance utilization.

## 6.1 The Impact of Demolition on Household Location

Housing vouchers increase housing location choice (relative to project-based housing assistance) and can potentially reduce low-income household exposure to poverty and other forms of neighborhood disadvantage. Increased choice may be particularly important for families on housing assistance since public housing projects tend to be located in the most distressed areas of cities. For example, the public housing buildings in my Chicago sample reside in Census tracts where the poverty rate – defined as the fraction of persons below the federal poverty line – was about 78 percent. To put this figure in perspective, Census tracts with 40 percent or more households falling below the poverty line are typically classified as extreme poverty tracts (Coulton et al., 1996). According to 2000 Census data, only 12.4 percent of the U.S. population had income below the poverty line (Bishaw, 2014).

I test whether displaced public housing residents move to better quality neighborhoods using address information from social assistance case records. The primary concern with this analysis of post-demolition location is that address data is only available if some member of a household received assistance such as AFDC/TANF, Foodstamps or Medicaid. Hence, this analysis may be biased if demolition has an impact on participation in social assistance programs.<sup>34</sup> In subsequent Section 6.3, I find that there is no detectable difference in the probability that displaced children are on social assistance in the years following demolition.

Table 2 shows that displaced (treated) households are less likely to live in public housing and moved to better quality neighborhoods relative to their non-displaced (control) peers. Column (2) shows that three years after demolition treated households with children are about 80 percent less likely to reside in public housing and live in Census tracts that average a 20 percent lower poverty rate relative to control households. In addition, their neighborhoods have less crime: treated households lived in neighborhoods with about 23 (about 33 percent) fewer violent crimes per 10,000 residents. Overall, these statistics on neighborhood relocation are similar to those reported in Jacob (2004), who relied on a different administrative source (specifically, he uses data

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<sup>34</sup>If demolition reduces the likelihood that a household uses social assistance, they will not have an active record in the social assistance data which implies that I will not observe their address history. The direction of this bias for the mobility analysis then depends on what kind of neighborhood these households selected.

from the Chicago Public Schools) to examine changes in children’s location.<sup>35</sup>

Figure 1 presents densities of neighborhood (tract) poverty rates to help characterize the cumulative impact of demolition on neighborhood quality.<sup>36</sup> Specifically, the poverty rates in the figure are duration-weighted averages over all the locations at which a household lived at since demolition. Separate densities are presented for displaced (solid) and non-displaced (dashed) households. This figure indicates that a large share of displaced residents relocated and lived in neighborhoods with notably lower poverty rates relative to residents of stable buildings. Nearly 44 percent of treated households lived in neighborhoods with poverty rates less than 40 percent (the threshold for classification as an extreme poverty neighborhood). Overall, the change in neighborhood condition in my sample is similar to the pattern for MTO volunteers that were randomly selected to be part of the unrestricted (“Section-8”) treatment group (Kling et al., 2005; Sanbonmatsu et al., 2011).

The difference in neighborhood quality also shrinks over time. Column (4) of Table 2 shows that there is no detectable difference in neighborhood quality eight years after the demolition. This occurs because the vast majority of the control group moves out of public housing by this time.<sup>37</sup> To further explore the impact on neighborhood quality over time, Figure 2 plots the difference in the (tract) neighborhood poverty rate in each post demolition year. Unsurprisingly, the difference in neighborhood poverty is largest in the first year after demolition: displaced households live in neighborhoods with about 28 percentage points lower poverty relative to non-displaced households. After this point, the difference in poverty rate begins to attenuate to nearly 13 percentage points by year three. Eventually there is no detectable difference in neighborhood poverty eight years after demolition when the youngest children in my sample (age 7 at baseline) may be leaving their parents’ household.

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<sup>35</sup>Moreover, the similarity between these results and Jacob (2004) should provide further reassurance against concern over the impact of demolition on the address histories that I construct from social assistance records. This is because Jacob (2004) analyzed relocation outcomes using Chicago Public School data where there is no concern over differential attrition due to the impact of demolition on use of social assistance.

<sup>36</sup>This analysis uses the tract poverty rate associated with a household’s address in a given year which is obtained from social assistance case records. When a household does not have an active case in a given year, no address is observed. For the duration weighting, I only consider years in which an address (and poverty rate) is observed for a given household.

<sup>37</sup>Recall that the set of control (non-demolished) buildings are defined as those that did not close during the 1995-2002 period. This implies that much of the movement by control households out of public housing (even in the long-run) was not due to later waves of demolition.



## 6.2 Adult Labor Market Outcomes of Children

Table 3 examines the impact of demolition on children’s adult labor market outcomes by presenting results from Equation 1. The point estimate reported in Column (2) show that children (age 7 to 18 at baseline) whose households were displaced have higher labor-force participation and earnings during their adult working years (age > 18). On average, children who were displaced are 4 percentage points (9 percent) more likely to be employed and have about \$600 higher annual earnings.<sup>38</sup> While I do not directly measure hours worked, Table 3 also shows that the probability of earning more than \$14,000 – the equivalent of working full time (35 hours a week) at \$8 per hour for 50 weeks – increases by 1.3 percentage points (13 percent). Overall, these results show that demolition and subsequent relocation is strongly associated with better adult labor-market outcomes for children.

Table 4 presents estimates from a modified version of Equation 1 which allows treatment effects to vary by gender. The point estimates show that the positive impact detected for the full sample is driven mainly by girls. Relative to non-displaced peers, girls are 6.6 percentage points (13 percent) more likely to be employed and have \$806 (18 percent) higher annual earnings. The corresponding effects for boys are less precisely estimated although the point estimates for all outcomes are positive.<sup>39</sup>

I also explore how labor market activity evolves as my sample ages by interacting an individual’s age in a given year (hereafter “age of measurement”) and the treatment indicator.<sup>40</sup> Figure 3 plots each point estimate for the estimated labor market treatment effect at each age for adult employment and earnings. Note that age of measurement on the  $x$ -axis ranges from 19 to 32 because the oldest child in my sample is age 32 in the last year of my sample. Despite larger standard errors at older ages, Panel (a) shows that the positive treatment effect for employment is

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<sup>38</sup>Recall that these results are from analysis of a panel of employment and earnings based on IDES data. If an individual is not present in the IDES data, their earnings in that year are zero. All monetary values are in 2012 dollars.

<sup>39</sup>Note that I cannot reject the equality of treatment effects for male and female earnings ( $p$ -value = 0.18). However, I can reject the hypothesis of the equality of treatment effect estimates for male and female labor participation ( $p$ -value = 0.07).

<sup>40</sup>Note that the treatment effect for older ages (e.g. age 32) are only identified using children who were relatively old at the time that displacement occurred. Again, this is because children who were displaced when they were younger have just entered adulthood by the end of my sample. For example, the youngest child in my sample is age 7 at the time of demolition and age 21 in 2009, which is the last year in my administrative data on employment outcomes.

relatively stable as the sample ages. Panel (b) shows that the earnings treatment effect is relatively stable although there is a slight positive trend and the point estimates are approximately \$5,000 after age 28.

Figure 4 examines the evolution of treatment effects separately for very young (age < 13 at baseline) and older children (age 13 to 18 at baseline). Note that I restrict the age of measurement because younger children are age 26 in the final year available in my administrative data. Overall, this analysis reveals two important findings. First, the treatment effects for older children (in red) are always positive (although not precisely measured) and shows little trend over time. Second, the analysis of younger children reveals that there is a slight increase in the size of the treatment effect point estimates at older ages. This pattern is consistent with a recent long-run evaluation of MTO evaluation by [Chetty et al. \(2016\)](#) which reveals that treatment effects for MTO children younger than age 13 are positive starting in their mid-twenties. However, [Chetty et al. \(2016\)](#) tend to find negative treatment effect point estimates for MTO children that are older at baseline (age 13 to 18), whereas I find that the point estimates for the effects on older children are always positive.<sup>41</sup>

I also examine additional types of heterogeneity in the labor market response to demolition. Panels (a) and (b) in Appendix Table A4 present results for labor market participation and earnings, respectively. Each row of the table reports descriptive statistics and results specific to a given subgroup based on baseline characteristics. The results for younger (age < 13) and older (age 13 to 18) children do not show any heterogeneity based on age; however, this average difference masks the notable heterogeneity of treatment effects by age of measurement shown in Figure 4. The additional subgroup results paint a mixed picture of how treatment effects are related to baseline measures of household disadvantage. On the one hand, children from (more disadvantaged) households with no working adults have large treatment effects, and there are no detectable effects for children from households that have at least one working adult. On the other hand, children from (more disadvantaged) households with adults who have at least one prior arrest have no detectable treatment effects, while there are large treatment effects for children from households in which no adult has a past criminal arrest.<sup>42</sup>

Finally, Figure 5 and Appendix Table A3 further explore heterogeneity in the impact of demoli-

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<sup>41</sup>For more on this comparison with recent re-analysis of MTO, see Figure 1 and Table 3 of [Chetty et al. \(2016\)](#).

<sup>42</sup>At the same time, it is worth noting that I cannot reject the equality of treatment effects across these different subgroups.

tion by estimating quantile treatment effects. Note that the lower bound of the  $x$ -axis on the figure is restricted to the 60<sup>th</sup> percentile because a large fraction of earnings are equal to zero.<sup>43</sup> This analysis examines the treatment effect for particular percentiles of the earnings distribution and *does not* measure the effect for any particular individual in the sample. The pattern of the point estimates show a notable degree of heterogeneity in the earnings response with the treatment effects generally increasing for higher quantiles. At the 85<sup>th</sup> percentile, the quantile treatment effect is about \$2,100, which is a 22 percentage point increase relative to the same percentile in the control distribution.

### 6.3 Adult Welfare Receipt of Children

Demolition and neighborhood relocation may also affect welfare receipt through many of the same mechanisms that link neighborhood conditions and labor-market outcomes. For example, [Bertrand et al. \(2000\)](#) examine U.S. Census data and find that use of social services and public assistance is affected by the usage rate of neighbors in one's social network (measured by language spoken). In this section, I test whether there is any impact of displacement due to public housing demolition on public assistance receipt (TANF/AFDC, foodstamps or Medicaid).

Table 5 presents results from Equation 1 in column (2), which shows no detectable impact of demolition and relocation on utilization of AFDC/TANF, foodstamps or Medicaid services across years. The point estimates are generally very small (less than or equal to 0.01), and the 95-percent confidence intervals generally rule out effects larger than negative or positive 3 percentage points. Columns (4) and (6) present results by gender and show little detectable heterogeneity.

This lack of effects may seem initially surprising given that the positive treatment effect on labor market activity should reduce reliance on social assistance. However, the intensity of disadvantage in this sample of children means that even sizable gains in labor-market activity are not sufficient to reduce eligibility for social assistance. For example, the mean annual earnings for non-displaced (control) in my sample is about \$3,700, and the reduced form impact of demolition is about \$600 which implies that the average displaced (treated) households will still be below the maximum annual income limits for foodstamps (\$25,000), TANF (\$7,000) and Medicaid (\$26,000).<sup>44</sup> Even at

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<sup>43</sup>Specifically, the descriptive statistics in Panel A of Appendix Table A3 show that 48 and 67 percent of annual earnings for males and female, respectively, are zero.

<sup>44</sup>Social service eligibility depends on both income and family size. The maximum income allowance described in

the 90th percentile of the earnings distribution, treated individuals still qualify for both foodstamps and Medicaid assistance.<sup>45</sup> Hence, the detected treatment effects of labor market participation and earnings would not reduce social program eligibility for the vast majority of individuals in my sample.

## 7 Mediating Mechanisms

Why does demolition have such a large impact on the adult labor market outcomes of children? In addition to the mechanisms described in Section 3, displaced parents may be more likely to work and use the additional household income to invest in child development (Black et al., 2014). To test for this parental channel, Table 6 explores whether there is any impact of relocation due to demolition on labor market outcomes of parents.<sup>46</sup> Column (2) shows that the point estimates are consistently small and the effects are never statistically different from zero. For example, the reduced form effect on labor market participation represents less than a one percent effect ( $=0.004/0.489$ ).<sup>47</sup> Overall, these results are consistent with previous analyses of MTO which found no detectable impact of vouchers for adults (Sanbonmatsu et al., 2011).

Another possibility is that living in a less disadvantaged neighborhood with less crime could possibly affect teenage criminal behavior. Indeed, Damm and Dustmann (2014) show that children who live in areas with a higher share of youth criminals are more likely to commit crime when they grow older. In this way, moving to lower poverty neighborhoods may boost labor market outcomes by affecting the likelihood of committing crime and being arrested. I test this hypothesis by examining the impact of demolition on teenage arrests using data from the Illinois State Police. Table 7 presents results that suggest that the positive impact of relocation due to demolition does not arise from reductions in adolescent criminal behavior. Column (2) reports results from estimating Equation 1 where the dependent variable is the number of arrests in each post-demolition year and the sample is restricted to years in which the individual is between 13 and 18 years old. The point estimate for the impact on total arrests is negative, but this effect is not precisely

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the text applies to a family size of three. Details on Illinois TANF, foodstamp and Medicaid eligibility come from DHS (2015), DHS (2014) and Brooks et al. (2015), respectively.

<sup>45</sup>For descriptive statistics on the quantiles of the earnings distribution, see Appendix Table A3.

<sup>46</sup>A parent in my sample is defined as any adult (age > 18 at baseline) who lives in a household with a child.

<sup>47</sup>The LATE estimates (not reported) are similarly small in magnitude (and imprecise).

measured ( $p = 0.37$ ). Looking at the impact by type of arrest reveals notable heterogeneity in that the impact on property arrests is actually positive. Specifically, children displaced by demolition had 16 percent ( $=0.008/0.048$ ) *more* property crime arrests than their non-displaced peers. While the increase in property arrests during ages of peak criminal activity may be somewhat surprising given the effects on labor market outcomes, supplemental analysis in Appendix Table A9 shows that there is a positive impact (reduction in arrests) on long-run crime. This analysis examines arrests over the *entire* sample of post-demolition years for children. The point estimate shows that youth who relocate have 14 percent fewer arrests for violent crimes.<sup>48</sup>

Finally, as discussed in Section 3, demolition has the potential to change long-run child outcomes by affecting school quality or student achievement. Previous research by [Jacob \(2004\)](#) sheds light on this issue by providing a short-run analysis of the impact of demolition using data from the Chicago Public Schools.<sup>49</sup> Interestingly, he finds that displaced families are not enrolled in better schools after demolition, and there is no detectable impact on test scores or grades. Yet, there is a 9 percent increase in the probability of dropping out of high-school for older children in his sample. This suggests that the labor market benefits for older (age > 13) found in this paper are not due to improvements in schooling.

## 8 Multiple Comparisons

One particular concern for my results is how to manage the risk of both false positives and false negatives given that my analysis considers labor supply and additional outcomes such as social assistance usage and youth crime. I follow current best practices to adjust per-comparison  $p$ -values to account for multiple outcomes ([Jacob et al., 2015](#); [Anderson, 2008](#)). To start, I specify a limited set of outcomes for my main, confirmatory analysis. Based on the preceding analysis, I focus on four outcomes: (1) labor market participation; (2) annual earnings; (3) use of social assistance (i.e., AFDC/TANF, Foodstamps or Medicaid); and (4) total number of criminal arrests. Next, I use a two-step procedure from [Benjamini et al. \(2006\)](#) to calculate adjusted  $p$ -values that control for the false discovery rate (FDR), which is the proportion of rejections that are false positives (Type I

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<sup>48</sup>Interestingly, [Kling et al. \(2005\)](#) show that there was an increase in property crime for boys whose household moved to a low-poverty neighborhood through the MTO program.

<sup>49</sup>[Jacob \(2004\)](#) has data on student outcomes up to five years following demolition and relocation.

errors).

Reassuringly, the results in Appendix Table A5 show that the main conclusions of my demolition analysis do not change based on examination of adjusted  $p$ -values to account for testing multiple outcomes. For convenience, Columns (1) and (2) repeat the results from Tables 3, 5 and 7. Column (3) reports the standard per-comparison (pairwise)  $p$ -values associated with each of the four outcomes. Again, these results show that I can reject the null hypothesis of no effect on labor market participation and earnings at the 1 percent level. Similarly, the adjusted  $p$ -values in Column (3) show that I can still reject the null of no effects even after accounting for the fact that my analysis of the impact of demolition considered four different outcomes.

## 9 Reconciling Estimates Across Studies

The positive impact of public housing demolition on labor market outcomes of children seems at odds with the results from the MTO evaluation, which is one of the most credible studies of neighborhood effects. This section begins with a description of the MTO study and reviews its findings. Next, I provide a heterogeneous treatment effects framework which explains the difference between the parameters identified in the present study and MTO. The framework highlights that the effects from MTO may be different from the average treatment effect because a relatively small fraction of families in MTO's target population volunteered for the experiment. Estimates from public housing demolition represent effects for a more general population because families had no ability to control whether demolition affected them. In short, volunteers for MTO may differ from the sample of families from demolished housing. The final part of this section provides evidence in support of this interpretation by examining a housing voucher lottery held in Chicago just after the housing demolitions began.

### 9.1 The MTO Evaluation and Comparing Estimates

The MTO evaluation sought to relocate families living in public housing projects located in extremely poor neighborhoods using housing vouchers. The program operated from 1994 to 1998 and recruited 4,600 households into the experiment across five major U.S. cities. The program randomly assigned each family to one of three groups: (1) a control group that received no vouchers

through MTO; (2) a treatment group that received housing vouchers that could be used to subsidize private market housing; and (3) a treatment group that received housing vouchers that could be used only to lease private market housing in Census tracts with poverty rates below 10 percent (Sanbonmatsu et al., 2011).

Using this random assignment, the MTO studies report intent-to-treat (ITT) effects of receiving a housing voucher offer and treatment-on-the-treated (TOT) effects. As explained in Kling et al. (2005), the latter provide an estimate of the causal effects of vouchers among those who used a voucher to move. Specifically, the TOT estimate is equal to the ITT estimate divided by the fraction of the treatment group that successfully used an MTO voucher.

Figures 6 and 7 show that there is notable contrast between estimates of the TOT effects for each MTO treatment arm and the reduced form effect of public housing demolition. The outcomes of interest are labor market participation and annual earnings for all children who were between ages 7 and 18 at baseline in each study.<sup>50</sup> For each outcome, the MTO estimates are negative and fall outside of the 95-percent confidence interval around the demolition estimate. Recall that households displaced by demolition received housing vouchers so that the demolition estimate can be interpreted as an ITT effect of vouchers. In addition, the ITT estimate from the demolition sample can be interpreted as a TOT estimate if all households displaced by demolition used their voucher and none of the non-displaced households used a voucher.

## 9.2 A Framework for Interpreting Estimates Across Studies

Why do estimates of the impact of demolition differ from what one might have expected based on the MTO study? While families in both contexts received housing vouchers, the demolition experiment is different because use of vouchers was effectively mandatory and households had no ability to control whether they were affected by demolition. In this section, I review the importance of these differences with a formal model of causal inference in the context of vouchers and heterogeneous treatment effects.

Let  $V_i$  be an indicator of whether the family of child  $i$  used a subsidized housing voucher to

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<sup>50</sup>Note that I use the MTO estimates as reported in Sanbonmatsu et al. (2011) because the age of children in this sample best aligns with the ages observed in my data. Specifically, children affected by demolition are ages 21 to 32 at the end of my sample. Similarly, the sample used Sanbonmatsu et al. (2011) examines “grown” children who were between the ages 21 of 31 by December 31, 2007.

lease private market housing. Our main interest is in learning about the effects of vouchers on children. Denote by  $Y_{1i}$  the outcome of child  $i$  if their family used a voucher to move, and let  $Y_{0i}$  be the outcome if their family did not use a voucher. Represent the causal effect of a voucher on child  $i$  as  $\Delta_i = Y_{1i} - Y_{0i}$ .

To identify the impact of a housing voucher, let  $O_i$  be a binary variable that indicates whether the family of child  $i$  received a randomly allocated housing voucher. Using the general framework of Heckman et al. (2001), Angrist (2004) and DiNardo and Lee (2011), I assume that the housing voucher offer  $O_i$  affects voucher use according to the following latent-index assignment mechanism,

$$V_i = \mathbf{1}(\gamma_0 + \gamma_1 O_i > \epsilon_i), \tag{4}$$

where  $\epsilon_i$  is a random error that represents the unobserved cost of voucher use for child  $i$ 's family. The coefficient  $\gamma_1$  represents the impact of receiving a voucher offer on the decision to use a voucher.

As shown in Angrist et al. (1996), the 2SLS estimate using a randomly assigned voucher offer  $O_i$  as an instrument for voucher use ( $V_i$ ) is the local average treatment effect (LATE) of vouchers,<sup>51</sup>

$$\frac{\mathbb{E}(Y_i|O_i = 1) - \mathbb{E}(Y_i|O_i = 0)}{\mathbb{E}(V_i|O_i = 1) - \mathbb{E}(V_i|O_i = 0)} = \mathbb{E}(\Delta_i|\gamma_0 + \gamma_1 > \epsilon_i > \gamma_0). \tag{5}$$

The left side of this expression is the Wald Estimator, which is the ratio of the reduced form (ITT) effect to the difference in the voucher-use rate in the treatment and control groups. Note that this parameter is nearly identical to the TOT estimates reported in the MTO studies.<sup>52</sup> The right-hand side is the local average treatment effect (LATE) of housing vouchers. This estimate represents the average effect of a housing voucher on children from households who “comply” by moving using a voucher that is randomly assigned to them.

With Equation 5 in mind, I consider a parametric model to illustrate how the LATE depends on the fraction of the public housing population that is induced to use a voucher within an ex-

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<sup>51</sup>Note that the standard monotonicity assumption is implied because this latent index model in this framework has a constant coefficient for the impact of the instrument.

<sup>52</sup>MTO provides an estimate of the treatment-on-the-treated (TOT), which differs from the LATE because the endogenous (first stage) variable is using an MTO voucher rather than any voucher (Kling et al., 2005). Since voucher allocation outside of MTO was limited, there are few “always-takers” who obtain a voucher even when they are assigned to the control group. This implies that  $\mathbb{E}(V_i|O_i = 0) \approx 0$  and that the TOT reported in MTO is very close to the LATE of voucher use.



periment. For simplicity, I assume the distribution of  $(\Delta_i, \epsilon_i)$  is bivariate normal:  $(\Delta_i, \epsilon_i) \sim N_2(\mu_\Delta, \mu_\epsilon, \sigma_{\Delta_i}^2, \sigma_{\epsilon_i}^2, \rho)$ . Here,  $\mu_\Delta$  and  $\mu_\epsilon$  are the mean benefits to children and mean costs of using a voucher, respectively. The correlation between child gains and costs is  $\rho$  while the respective variances for benefits and costs are  $\sigma_{\Delta_i}^2$  and  $\sigma_{\epsilon_i}^2$ , respectively.

Under these assumptions, the LATE of using a voucher is:

$$\begin{aligned} \mathbb{E}(Y_{1i} - Y_{0i} | \gamma_0 + \gamma_1 > \epsilon_i > \gamma_0) &= \mathbb{E}(\Delta_i | \gamma_0 + \gamma_1 > \epsilon_i > \gamma_0) \\ &= \underbrace{\mu_\Delta}_{\text{ATE}} + \rho\sigma_\Delta \left( \frac{\phi(\gamma_0) - \phi(\gamma_0 + \gamma_1)}{\Phi(\gamma_0 + \gamma_1) - \Phi(\gamma_0)} \right) \end{aligned} \quad (6)$$

The right side of Equation 6 shows that the LATE will differ from the average treatment effect (ATE) as a function of the parameters of Equation 4, which models whether a family uses a voucher to move and the correlation between gains and costs ( $\rho$ ). Importantly, the discrepancy between the LATE and ATE decreases with  $\mathbb{P}(V_i = 1 | O_i = 1) \equiv \Phi(\gamma_0 + \gamma_1)$ , which is the fraction of households that use a voucher.

Figure 8 considers two different scenarios which show that the LATE may be far above or below the ATE when few households are induced to use a voucher to move. Panel (a) assumes a slight positive selection on gains ( $\rho = -0.15$ ). This pattern is referred to as “Roy style selection,” in which parents are more likely to use a voucher if their children would experience higher benefits to moving. In this case, the LATE is much higher than the ATE if a small fraction of households use a voucher to move. Intuitively, few households have sufficiently low costs of using a voucher ( $\epsilon_i$ ), and these children have higher returns if  $\rho < 0$ . In contrast, Panel (b) illustrates a scenario where there is slight negative selection on gains ( $\rho = 0.15$ ). With this type of selection, the children who would most benefit from moving live in households with parents who are the least likely to use a voucher. This implies that the LATE is far below the ATE when few families elect to use a voucher. Here, the children in these selected households with lower costs have less to gain if  $\rho > 0$ .

The key insight in this section is that the context and pattern of results in the MTO and demolition settings fit the model presented in Panel (b), which features negative selection in terms of voucher use. In the case of MTO, there was relatively low interest in using a housing voucher - that is,  $\mathbb{P}(V_i = 1 | O_i = 1)$  was low. [Goering et al. \(1999\)](#) report that only 25 percent of eligible families

at the five MTO sites volunteered.<sup>53</sup> In addition, the point estimates and confidence intervals in MTO suggest that there are small benefits to moving for children among the households that had sufficiently low costs that they sought vouchers by signing up for the experiment. In contrast, the natural experiment created by demolition induced a broader population to use a voucher because the housing authority (essentially) randomly allocated vouchers to a large fraction of public housing residents and use of vouchers was effectively mandatory. Hence, the demolition context implies that  $\mathbb{P}(V_i = 1|O_i = 1)$  is high, and the effects in the demolition sample suggest that there are larger gains for children from families that have a relatively high cost of moving using a voucher.

### 9.3 The Chicago 1997 Housing Voucher Lottery

To test further for the existence of negative selection into voluntary housing mobility programs, I study a housing voucher lottery that occurred in Chicago shortly after the public housing demolition began. During the late 1990s, the Chicago Housing Authority Corporation (CHAC) created a new waiting list for Section 8 vouchers.<sup>54</sup> Because demand for these vouchers far exceeded supply, the CHAC randomly assigned vouchers to applicants.

Using data collected for [Jacob and Ludwig \(2012\)](#) and [Jacob et al. \(2015\)](#), I analyze the CHAC 1997 lottery using the following definitions for treatment and control groups.<sup>55</sup> There were 82,607 income-eligible applicants and the CHAC randomly assigned each a position on a voucher wait list. The top 18,110 applicants and their families comprise the treated group that received housing vouchers. Families whose lottery position was between 35,001 and 82,607 are the control group because the CHAC told these families that they would not receive vouchers.<sup>56</sup>

Panel (a) of Table 8 shows voucher-usage statistics for my main analysis sample of children from lottery households that lived in public housing at the time of applying for the CHAC 1997 lottery.<sup>57</sup> The first row shows that 63 percent of the treated group uses a housing voucher, while a

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<sup>53</sup>In keeping with the spirit of the model shown in Equation 4, I assume that all individuals who did not volunteer for the MTO experiment know their own idiosyncratic cost. Any household with cost that exceeds  $\gamma_0 + \gamma_1$  would never volunteer for the experiment and is a “never-taker” for the treatment ( $V_i$ ).

<sup>54</sup>In 1995, the U.S. Department of Housing and Urban Development (HUD) transferred housing operations from the Chicago Housing Authority (CHA) to a non-profit organization, the Chicago Housing Authority Corporation (CHAC).

<sup>55</sup>For a detailed description of the data including summary statistics, see Appendix Section A5.

<sup>56</sup>The CHAC originally planned to provide voucher offers to applicants whose position on the wait list fell between 18,111 and 35,000. However, due to fiscal constraints the CHAC was unable to meet this initial plan. For more detailed background on the CHAC lottery, see [Jacob and Ludwig \(2012\)](#).

<sup>57</sup>This analysis of lottery children from public housing is distinctly different from the analysis in [Jacob et al. \(2015\)](#)

notable share (31 percent) of the control group also obtains a housing voucher from some program other than the CHAC 1997 lottery.<sup>58</sup> Similar to the MTO unrestricted treatment arm, Appendix Table A6 shows that households that used a voucher through the CHAC 1997 program moved to notably better neighborhoods. For example, winning a voucher decreased the neighborhood poverty rate by about 10 percentage points (22 percent).

Also similar to the MTO results, Panel (b) of Table 8 shows that there is no measurable impact of vouchers for children from households that volunteered for the CHAC 1997 lottery. Column (2) provides the reduced form (ITT) estimates while Column (3) provides estimates of the LATE of vouchers. Specifically, I compute the LATE estimates by estimating a 2SLS system in which the dependent variable in the first stage is an indicator for whether an individual used a voucher. The reduced form effects on the probability of employment are fairly precisely estimated zeros, while the estimate for earnings is also small but has a larger standard error. Notably, the lottery sample 2SLS estimate for the labor market participation and earnings fall far below the lower bound on the 95-percent confidence interval around the demolition estimates (see Figure 6 and 7).

#### 9.4 Interpretation of the CHAC 1997 Lottery Results

The relatively small estimates of the impact of vouchers detected in the CHAC 1997 lottery sample are consistent with the hypothesis that there are relatively small gains for children from households who are likely to use or seek vouchers voluntarily. However, there are other interpretations. One concern is that the treatment effects may be correlated with observed characteristics that differ between the demolition and lottery samples.<sup>59</sup> For example, Appendix Table A7 shows that households in the demolition sample are much more disadvantaged across a number of important characteristics such as adult labor market activity and criminal history.

To address this difference, I re-weight the demolition sample to be more similar to the more-

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which focused on lottery children living in private market housing. For these households, receiving a voucher is a large in-kind transfer because they were not previously receiving housing assistance. In contrast, the set of public housing children in the 1997 lottery do not experience a change in household income because the housing contribution rules for vouchers and project-based housing are similar.

<sup>58</sup>Previous studies have highlighted that voucher utilization rates tend to be far less than unity. For example, [Jacob and Ludwig \(2012\)](#) find that about 43 percent of households living in private market housing at baseline chose to use a voucher offer provided through the CHAC 1997 lottery. [Finkel and Buron \(2001\)](#) also study voucher take-up rates across U.S. cities and find that the overall voucher utilization rate was 69 percent in 2000.

<sup>59</sup>This can occur under a potential outcomes model where  $Y_{ji} = \mu_j(X_i) + U_j$  for  $j \in \{0, 1\}$  where  $\mu_j(X_i)$  is a general function of observables  $X_i$  and  $U_j$  represents unobservable determinants of potential outcomes ([Heckman et al., 2001](#)).

advantaged CHAC 1997 lottery sample. Specifically, I pool children in the demolition and lottery samples and estimate a propensity score where the dependent variable is an indicator for membership in the lottery sample. The covariates include a rich set of observable household and child characteristics including baseline measures such as adult labor market activity and criminal behavior.<sup>60</sup> As shown in Column (4) of Table A7, re-weighting using these propensity scores insures that the characteristics of the weighted demolition sample are statistically indistinguishable from the lottery sample.

Panel (c) of Table 8 shows that even after re-weighting the demolition estimates are still strikingly different from the lottery sample results. Moreover, the weighted sample point estimates are similar to the results for the un-weighted analysis. This demonstrates that differences in observed characteristics are unlikely to explain why the impact of moving is larger in the demolition sample. Moreover, it also suggests that observed characteristics are not correlated with unobservable (costs) that drive the decision to move.

Overall, this evidence suggests that large gains among children from households that are unlikely to use or seek vouchers drive the effects detected in the demolition sample. The point estimates and confidence intervals for labor market participation in the MTO and CHAC 1997 lottery samples suggest that children from volunteer households have relatively smaller gains. As shown in Figure 8, this pattern of negative selection between the probability that a parent decides to move using a voucher and the gains to children is consistent with “reverse Roy” selection where children with the most to gain from moving have parents who are the *least* likely to move using a voucher.

One explanation for this negative selection is that parents who invest more in human capital or other margins may be the most willing or able to move out of public housing. In addition, moving to a neighborhood with lower poverty and less crime may substitute for parenting effort or household resources. Interestingly, supplementary research of the MTO evaluation provides suggestive evidence that parents living in public housing do compensate and substitute parenting effort in response to concerns over neighborhood safety. Specifically, [Kling et al. \(2001\)](#) report that moving through MTO actually reduced parents’ child monitoring intensity, presumably because treated parents felt safer in their new neighborhood.

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<sup>60</sup>Appendix Figure A2 provides further details on the propensity score estimates and illustrates the propensity score distributions for households in the lottery and demolition samples, respectively.

To provide further intuition for why reverse Roy selection could occur in the context of a housing mobility program, Appendix A6 provides a stylized model in which neighborhood safety and parenting effort (e.g., child monitoring) are substitutes in the production of child outcomes. In the model, parents living in the same public housing project have different perceptions about neighborhood safety while having identical preferences and resources. As expected, parents who have the worst view of their neighborhood’s safety opt into a program that provides vouchers. In an experimental setting, these parents will choose high parenting effort when they are randomized into the control group, and this reduces the contrast between treatment and control child outcomes. Intuitively, the model predicts larger treatment effects for children of parents who would *not* seek vouchers because these non-volunteering parents have relatively more optimistic perception of safety, and this view causes them to choose lower parenting (child monitoring) effort.

Finally, it is worth highlighting that previous studies have found a similar pattern of negative selection in education markets. [Walters \(2014\)](#) and [Hastings et al. \(2006\)](#) examine school choice and academic performance in Boston and North Carolina, finding that children who would have the highest gains from attending charters or other high-quality schools are the least likely to seek these schooling options. Similarly, [Kline and Walters \(2015\)](#) find that children who would have the largest test score gains from enrolling in Head Start have a lower likelihood of participating.

## 10 Cost-Benefit Analysis

This section uses the results from Section 6 to provide back-of-the-envelope estimates of the benefits and costs of relocating youth from project-based public housing assistance using unrestricted Section 8 vouchers. These calculations are informative because we know little about the optimal design of housing assistance ([Olsen, 2003](#)). Moreover, the fact that the U.S. federal government currently spends \$46 billion on housing assistance underscores the need for a comparison of the relative efficiency of different housing assistance programs ([Collinson et al., 2015](#)).<sup>61</sup>

To understand the earnings benefit of relocation, I use the reduced form estimate from Table 3. Recall from Section 5 that these estimates reflect the impact of relocating using a subsidized Section 8 housing voucher relative to the counterfactual of living in project-based public housing. With

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<sup>61</sup>The \$46 billion in expenditures on housing programs is more than twice the level of federal spending on cash welfare and more than five times the amount spent on Head Start ([Collinson et al., 2015](#)).

this in mind, the estimates show that replacing project-based assistance increased youth earnings by about \$602, a 16 percent increase above the control group mean.

Similar to [Chetty et al. \(2016\)](#), I translate the impact of relocation into a predicted lifetime impact on income using the following assumptions: (1) the 16 percent increase in annual income is constant over the lifecycle; (2) the lifecycle profile of income for demolition participants follows the U.S. population average; (3) the real wage growth rate is 0.5 percent; and (4) the discount rate is 3 percent, approximately the current 30-year Treasury bond rate. Under these assumptions, moving youth out of public housing using vouchers would increase pre-tax lifetime income by about \$45,000. The present value of this increase in lifetime income is about \$12,000.<sup>62</sup> For a family with two children, this corresponds to a total family benefit of about \$24,000.

In terms of cost, a variety of previous studies suggests that the direct fiscal cost of housing voucher programs is much lower than the cost of project-based housing assistance ([Olsen, 2014](#)). This implies that housing authority payments for moving expenses were the main direct cost of replacing project-based assistance with Section 8 vouchers.<sup>63</sup> To the best of my knowledge, there is no record on moving payments provided by the Chicago Housing Authority. However, one way to get a sense of these costs is to look at the federal moving-cost fixed payment schedule that the U.S. federal government uses to reimburse individuals displaced by government projects such as highway construction. The fixed moving cost payment is set at \$1,100 for a furnished four bedroom apartment in the state of Illinois.<sup>64</sup>

Overall, this simple accounting suggests that relocating children from public housing is likely to generate a high rate of return on investment since the value of increased lifetime earnings is about \$24,000 for a family with two children and the main cost comes from moving expenses which are most likely around \$1,100 per family. Assuming that there is just a 10 percent increase in tax revenue for relocated children, this implies that the government would save about \$1,300 ( $= \$24,000 \times 0.10 - \$1,100$ ) per family. At the same time, it is important to recognize that these

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<sup>62</sup>The estimate of the impact on lifetime income is calculated as follows. First, I calculate the mean of individual pre-tax annual income for all working-age adults (age 19 to 65) from the 2000 Census. Next, I apply a 0.5 percent wage growth rate, which yields an undiscounted sum of lifetime earnings for the average American at \$1.75 million. Average income for non-displaced (control) youth in my demolition sample is about 16 percent of the average adult in the U.S. This implies that the estimated effect of relocation on pre-tax undiscounted lifetime earnings is about \$45,000 ( $= 0.16 \times 0.16 \times \$1.75m$ ). Note that all monetary values are in 2012 dollars.

<sup>63</sup>Unlike households in the MTO study, there were few supplemental services provided to families forced to relocate due to building demolition ([Jacob, 2004](#)).

<sup>64</sup>The fixed moving payment is determined by the number of furnished bedrooms and varies by state.

cost-benefit calculations ignore any *negative* spillover effects on residents of neighborhoods where displaced households move. In a neighborhood-level study of demolition and crime, [Aliprantis and Hartley \(2015\)](#) found no detectable increase in homicides for neighborhoods that received displaced individuals. However, there were detectable increases in other types of crime such as assaults and property crime.<sup>65</sup>

## 11 Conclusion

This paper exploits public housing demolition as a natural experiment to study the impact of growing up in a disadvantaged neighborhood. During the 1990s, the CHA selected some public housing buildings for demolition while leaving other buildings untouched. This event provides quasi-random variation in childhood environment because authorities selected buildings for demolition for reasons unrelated to resident characteristics.

Importantly, the setting in this study provides a unique opportunity to study public housing families who were not volunteers and were required to relocate. This contrasts with the well-known MTO experiment that provided volunteering families with the option of leaving public housing using vouchers. This optional feature of the MTO treatment is notable because 75 percent of the (eligible) population targeted by MTO chose not to volunteer for the experiment. While the MTO evaluation cannot speak to the effects of relocation for these “never-takers”, there is no such compliance issue in the demolition sample because nearly all displaced households relocate from public housing.

My analysis reveals that the benefits of relocation are more general than what one might expect given estimates from MTO. I find that children displaced by public housing demolition have notably better adult labor market outcomes compared to their non-displaced peers. This positive impact is detectable regardless of the age at which a child’s family relocates. This contrasts with analysis from MTO, which finds a positive impact only for children who were young when their families moved ([Chetty et al., 2016](#)).

One of the main contributions of this paper is to provide a new explanation to reconcile findings

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<sup>65</sup>[Popkin et al. \(2012\)](#) also studies the impact of public housing demolition and relocation on neighborhood-level crime measures. Using different data and methodology relative to [Aliprantis and Hartley \(2015\)](#), [Popkin et al. \(2012\)](#) find evidence that both violent and property crime increased in areas that received displaced public housing residents.

in the neighborhood effects literature. On the one hand, a number of observational studies find a substantial positive correlation between child outcomes and measures of neighborhood quality (Galster et al., 2007; Ellen and Turner, 1997). This pattern inspired a large theoretical literature to support a causal interpretation of these estimates (e.g., Massey and Denton (1993); Wilson (1987)). On the other hand, some have suggested that neighborhood effects must be small in light of the lack of detectable improvement in outcomes for some children who moved through the MTO experiment.

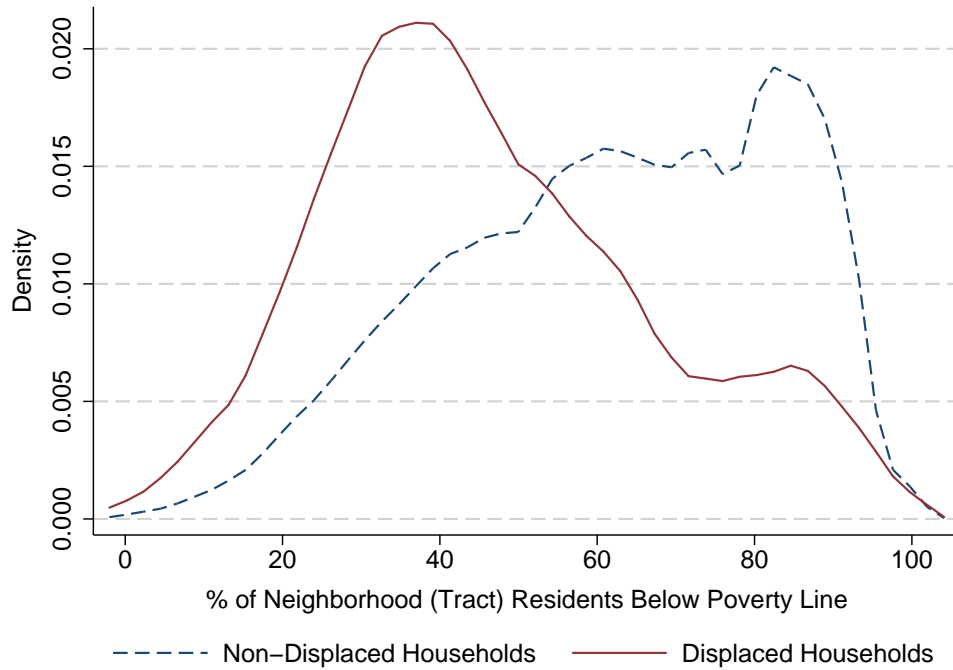
The empirical results presented in the present paper support the idea that the discrepancy between observational studies and MTO exists because children whose families move through voluntary mobility programs are negatively selected on the potential gains to treatment (moving using a voucher). The lack of effects in Sanbonmatsu et al. (2011) suggests that the large benefits of relocation detected in the present paper are driven by children whose households had relatively low demand for moving out of public housing. This interpretation is further supported by my analysis of the CHAC 1997 housing voucher lottery that occurred in Chicago after the beginning of the demolitions. Similar to MTO, there are no detectable benefits of moving using a voucher for children from households that volunteered and won the CHAC 1997 lottery. The contrast in effects persists even after re-weighting the demolition sample to match the observed characteristics in the lottery sample.

In terms of housing policy, this paper demonstrates that relocation of low-income families from distressed public housing has substantial benefits for both children (of any age) and government expenditures. I estimate that moving a child out of public housing using a standard housing voucher would increase total lifetime earnings by about \$45,000, which has an equivalent present value of \$12,000. In all likelihood, this will yield a net gain for government budgets because there are negligible moving costs to relocating families and housing vouchers have similar costs compared to project-based assistance.



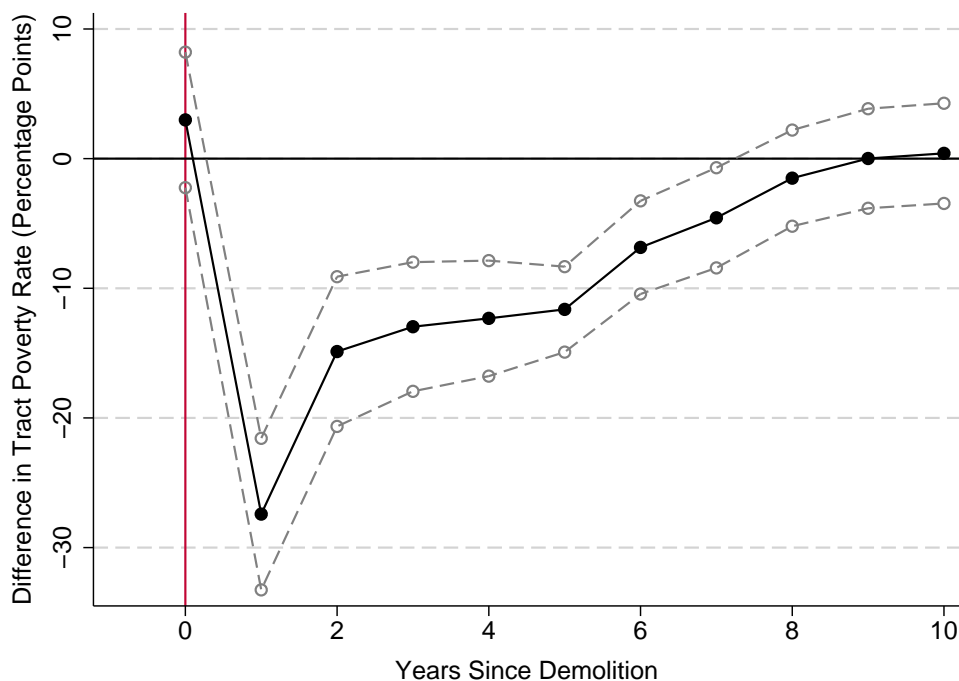
## 12 Figures and Tables

Figure 1: Density of Neighborhood Poverty for Displaced (Treated) and Non-displaced (Control) Households



Notes: This figure displays the density of the Census tract-level poverty rate for households ( $N = 2,767$ ) with at least one child (age 7 to 18 at baseline) affected by demolition. Poverty rates for each household are duration-weighted averages over all locations that a household lived since being displaced (treated) by housing demolition. Household location is tracked to 2009. The duration-weighted poverty rate for households that were displaced by demolition is shown in the solid red line, while households from non-demolished buildings are shown in the dashed blue line.

Figure 2: Difference in Neighborhood Poverty For Displaced and Non-displaced Households by Post-Demolition Year



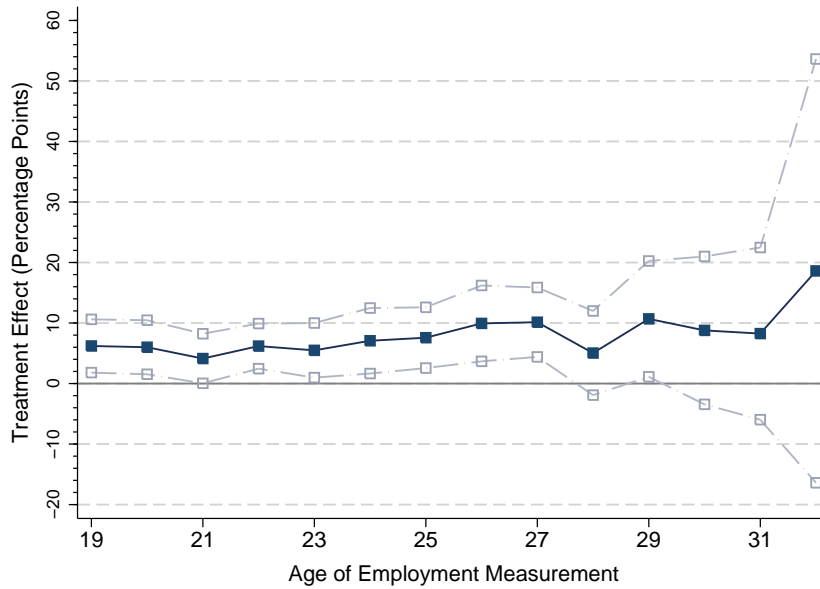
Notes: This figure illustrates the change over time in the difference in neighborhood poverty rate between displaced (treated) and non-displaced (control) households with children (age 7 to 18 at baseline). Specifically, I plot (in solid black) the set of coefficients  $\pi_y$  for  $y \in \{0, \dots, 10\}$  from the following specification:

$$pbpov_{htp} = \sum_{y=0}^{y=10} \pi_y treat_h \mathbf{1}(t - t^* = y) + \sum_{y=0}^{y=10} \delta_y \mathbf{1}(t - t^* = y) + \psi_p + \epsilon_{ht}$$

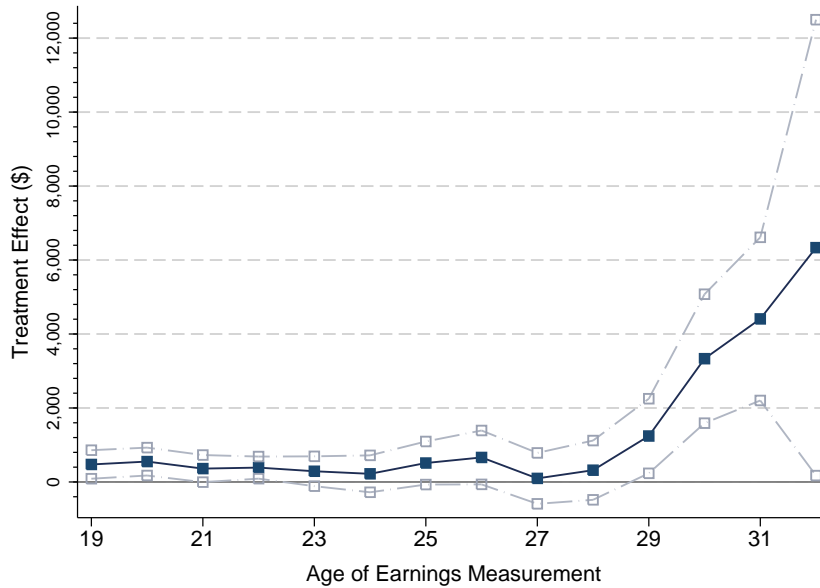
where  $h$  indexes a household;  $t$  represents years; and  $p$  indexes projects. The dependent variable is the percentage of residents living below the poverty line in a Census tract and  $\psi_p$  is a set of project fixed effects. The variable  $t^*$  represents the year of demolition for a particular household. Recall that public housing demolitions occur from 1995-1998 in my sample. The variable  $treat_h$  is an indicator for treatment (displaced) status. The data used with this specification is a panel for a particular household where the first observation is the poverty rate based on the household's address at the time of demolition ( $t^*$ ). Hence, the set of coefficients  $\pi_y$  represent the difference in poverty rate between displaced (treated) and non-displaced (control) households in a particular post demolition period ( $y$ ). There are 2,767 households in the sample. The dashed gray lines in the figure also outline the 95-percent confidence interval for the year-specific point estimates.

Figure 3: Labor-Market Treatment Effects for All Children by Age of Measurement

(a) Dependent Variable: Employed (=1)



(b) Dependent Variable: Annual Earnings (\$)



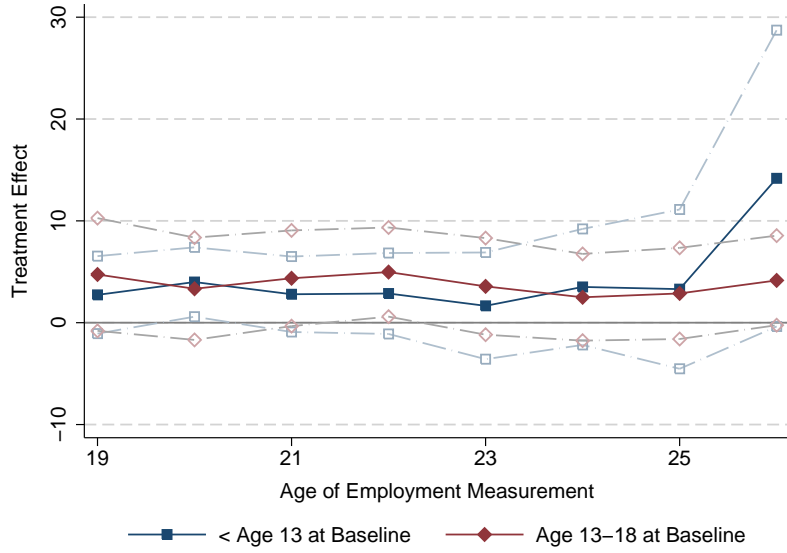
Notes: The figures in Panels (a) and (b) display estimated treatment effects at adult ages for children (age 7 to 18 at the time of demolition). Specifically, each point (red circles) is a coefficient  $\alpha_j \forall j \in \{19, \dots, 32\}$  from the following specification:

$$y_{itp} = \sum_{j=19}^{32} \alpha_j D_{i,b} \mathbf{1}(\text{age}_{i,t} = j) + X_i' \theta + \psi_p + \delta_t + \epsilon_{itp}$$

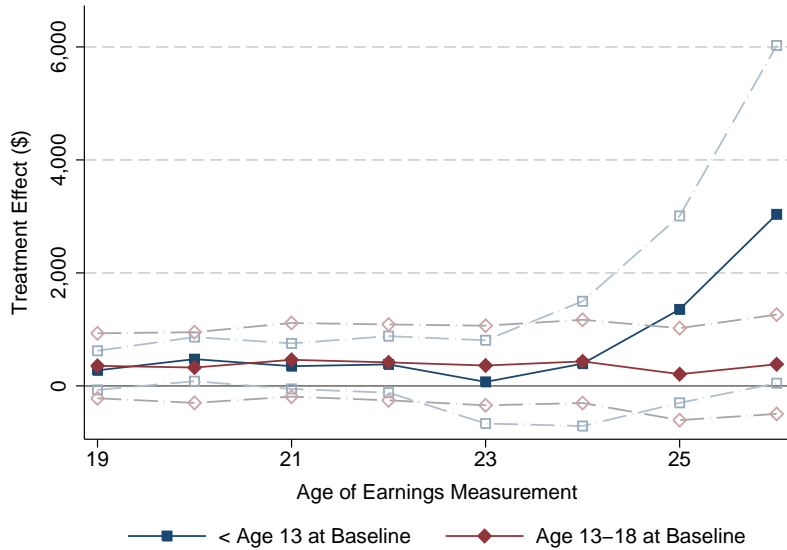
where  $i$  is an individual;  $t$  indexes years; and  $p$  is a project. The terms  $\delta_t$  and  $\psi_p$  are year and project fixed effects, respectively. The dummy variable  $D$  takes a value of one if an individual was displaced by demolition. The main effects for the indicator terms for individual age are included in the vector  $X_i$ . The area shaded in blue that surrounds the plotted coefficients covers the 95-percent confidence interval for the age-specific point estimates. Note that age 32 is the last age observed for the oldest children in the sample. All monetary units are in 2012 dollars.

Figure 4: Younger vs Older Children: Labor-Market Treatment Effects by Age of Measurement

(a) Dependent Variable: Employed (=1)



(b) Dependent Variable: Annual Earnings (\$)

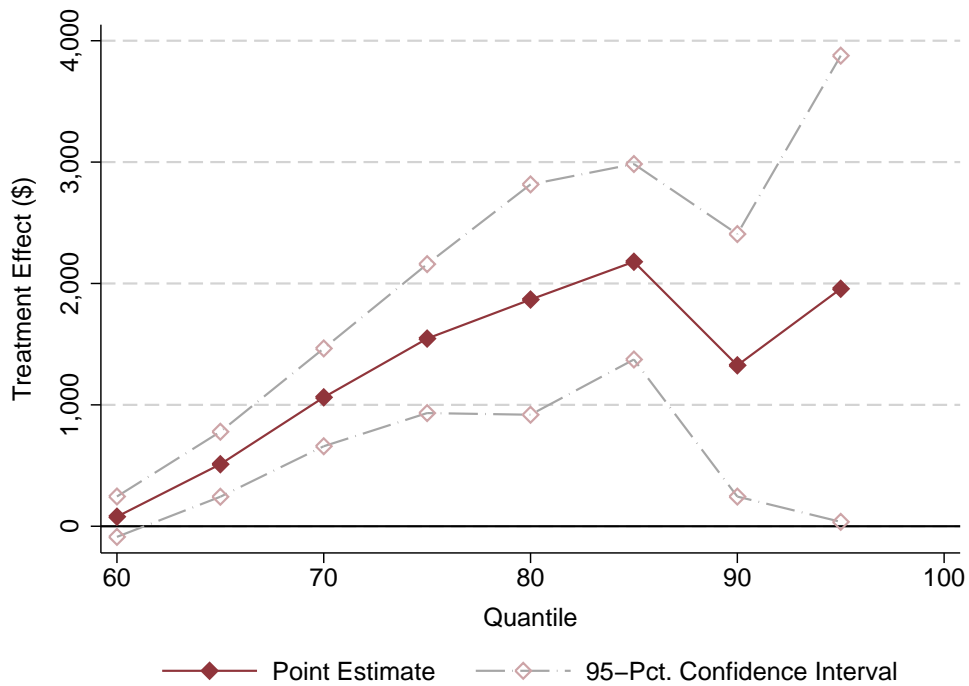


Notes: The figures in Panels (a) and (b) display estimated treatment effects at adult ages for children affected by demolition. Each figure presents two sets of results: estimates for younger (age < 13 at baseline) and older (age 13 to 18 at baseline) children as solid red diamonds and solid blue squares, respectively. Each point plotted on the figure is a coefficient  $\alpha_j \forall j \in \{19, \dots, 26\}$  from the following specification:

$$y_{itp} = \sum_{j=19}^{26} \alpha_j D_{i,b} \mathbf{1}(\text{age}_{i,t} = j) + X_i' \theta + \psi_p + \delta_t + \epsilon_{itp}$$

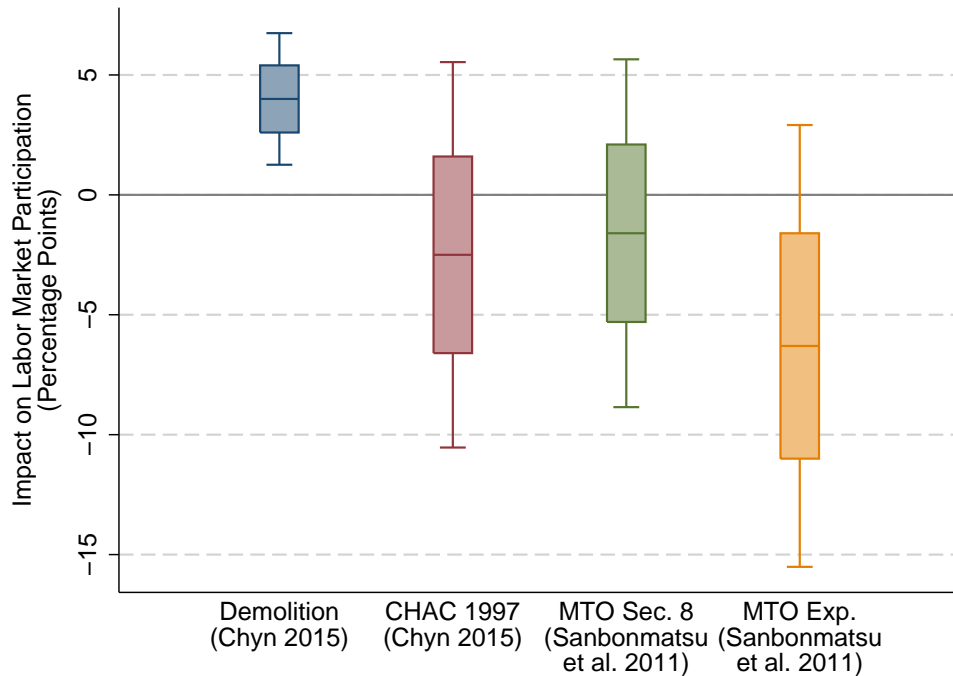
where  $i$  is an individual;  $t$  indexes years; and  $p$  is a project. The terms  $\delta_t$  and  $\psi_p$  are year and project fixed effects, respectively. The dummy variable  $D$  takes a value of one if an individual was displaced by demolition. The main effects for the indicator terms for individual age are included in the vector  $X_i$ . Note that the index only goes up to age 26 because this is the oldest age of measurement observed for younger children (age < 13 at baseline). The dashed lines in the figures plot out the 95-confidence intervals for each age-specific point estimate. All monetary units are in 2012 dollars.

Figure 5: Quantile Treatment Effects for Adult Earnings of Children



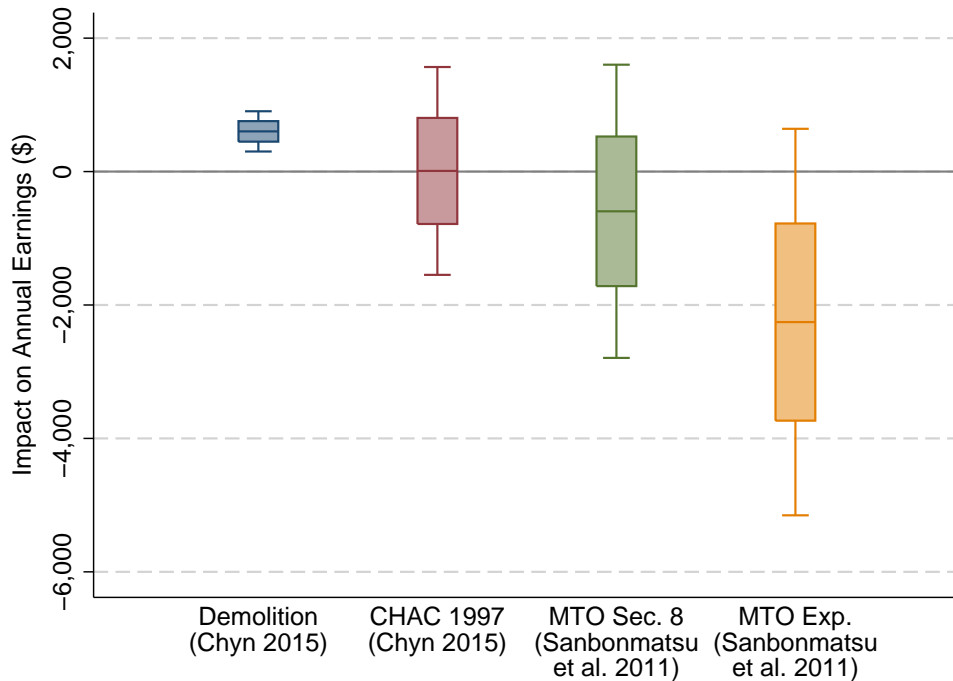
Notes: This figure plots estimates of the quantile treatment effect on adult earnings outcomes for children (age 7 to 18 at baseline) affected by public housing demolition. These estimates measure the treatment effect for particular percentiles of the distribution of earnings. In other words, the quantile treatment effect estimate for the 60<sup>th</sup> percentile measures the difference between the 60<sup>th</sup> percentile of the treated (displaced) and control (non-displaced) earnings distributions. The bars surrounding each point estimate are the 95-percent confidence interval. Note that the lower bound of the  $x$ -axis on the figure is restricted to the 60<sup>th</sup> percentile because a large fraction of earnings are equal to zero.

Figure 6: Effects on Adult Employment of Children Across Studies



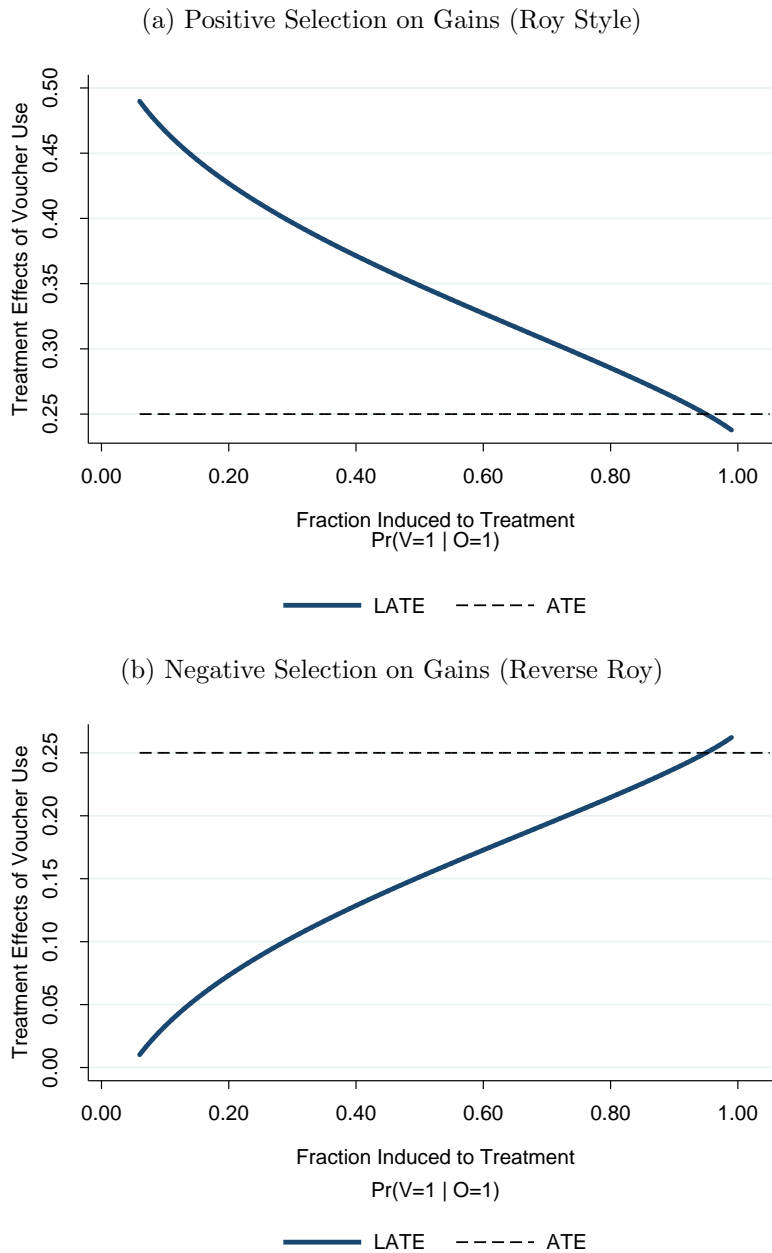
Notes: This figure displays box and whisker plots of the effects on adult labor market participation for children (age 7 to 18 at baseline) from different studies. The center of each box is the point estimate in each sample while the top and bottom of each box represent effects that are one standard error above and below the point estimate. The top and the bottom of the whiskers display the 95-percent confidence interval. The MTO evaluation provided vouchers to low-income households, and this figure reports the treatment-on-the-treated effects (TOT) of vouchers as reported in [Sanbonmatsu et al. \(2011\)](#). Similarly, households received vouchers if they were displaced by public housing demolition or if they won the CHAC 1997 housing lottery. The demolition results are the intent-to-treat (ITT) effect which is equal to TOT effect of a voucher if all households displaced by demolition used their voucher offer and none of the non-displaced households used a voucher. The CHAC lottery result is the local average treatment effect (LATE) which is similar to a TOT because only a small fraction of the control group received vouchers even if they did not win the CHAC 1997 lottery. The demolition and CHAC lottery samples contain 5,246 and 4,661 children, respectively. The MTO final evaluation has 3,052 children in the sample for the labor market outcome analysis.

Figure 7: Effects on Adult Earnings of Children Across Studies



Notes: This figure displays box and whisker plots of the effects on adult earnings outcomes for children (age 7 to 18 at baseline) from different studies. The center of each box is the point estimate in each sample while the top and bottom of each box represent effects that are one standard error above and below the point estimate. The top and the bottom of the whiskers display the 95-percent confidence interval. The MTO evaluation provided vouchers to low-income households, and this figure reports the treatment-on-the-treated effects (TOT) of vouchers as reported in [Sanbonmatsu et al. \(2011\)](#). Similarly, households received vouchers if they were displaced by public housing demolition or if they won the CHAC 1997 housing lottery. The demolition results are the intent-to-treat (ITT) effect which is equal to TOT effect of a voucher if all households displaced by demolition used their voucher offer and none of the non-displaced households used a voucher. The CHAC lottery result is the local average treatment effect (LATE) which is similar to a TOT because only a small fraction of the control group received vouchers even if they did not win the CHAC 1997 lottery. The demolition and CHAC lottery samples contain 5,246 and 4,661 children, respectively. The MTO final evaluation has 3,052 children in the sample for the labor market outcome analysis.

Figure 8: The Local Average Treatment Effect When the Fraction Induced to Treatment Varies



Notes: Panels (a) and (b) display the local average treatment effect (LATE) as a function of the fraction of the population that is induced to use a housing voucher. Panel (a) is drawn so that there is positive selection on gains ( $\rho = -0.15$ ), and treatment effects are largest for individuals who are most likely to take the treatment. Panel (b) is drawn so that there is negative selection on gains ( $\rho = 0.15$ ), and treatment effects are larger for individuals who are less likely to take the treatment. The  $x$ -axis shows the fraction of the public housing population that is induced to use a voucher ( $\mathbb{P}(V = 1 | O = 1)$ ). The models depicted in each panel show how the effects of vouchers in the demolition setting differ from findings from the MTO evaluation. In the MTO context, few families within the targeted public housing population were induced to use a voucher. In the demolition context, the fraction of families that used vouchers was higher because the housing authority randomly selected buildings for demolition and voucher-use was effectively mandatory for displaced families. See Section 9 for further details and discussion. Other parameters are set to the following values in both panels:  $\sigma_\epsilon = 0.15$ ,  $\mu_\Delta = 0.25$  and  $\gamma_0 = 0.05$ .



Table 1: Comparison of Displaced (Treated) and Non-displaced (Control) Children and Adults at Baseline (Prior to Demolition)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All Children		Male Children		Female Children		Adults	
	Control Mean	Difference: Treated–Control, Within Estimate	Control Mean	Difference: Treated–Control, Within Estimate	Control Mean	Difference: Treated–Control, Within Estimate	Control Mean	Difference: Treated–Control, Within Estimate
<b>Demographics</b>								
Age	11.714	0.035 [0.159]	11.548	0.145 [0.196]	11.873	-0.070 [0.186]	28.851	0.810** [0.312]
Male (=1)	0.489	-0.008 [0.017]					0.128	-0.001 [0.011]
<b>Past Arrests (#)</b>								
Violent	0.015	0.005 [0.007]	0.028	0.011 [0.014]	0.004	-0.003 [0.009]	0.185	-0.017 [0.032]
Property	0.011	0.010 [0.009]	0.018	0.015 [0.014]	0.004	0.004 [0.010]	0.156	0.016 [0.020]
Drugs	0.025	0.000 [0.013]	0.054	0.017 [0.023]	0.000	-0.018 [0.012]	0.166	0.031 [0.022]
Other	0.022	0.004 [0.008]	0.035	0.015 [0.014]	0.011	-0.008 [0.008]	0.230	-0.018 [0.028]
<b>School Outcomes†</b>								
Old for Grade (=1)	0.197	-0.013 [0.012]						
Reading Score (Rank)	22.800	-0.400 [1.070]						
Math Score (Rank)	25.100	-0.730 [1.530]						
<b>Economic Activity††</b>								
Employed (=1)							0.173	0.006 [0.016]
Earnings							\$1,493.75	\$-45.91 [193.358]
N (Individuals)		5,250		2,547		2,703		4,331

Notes: Children are age 7 to 18 in the year prior to demolition (baseline) while adults are over age 18. The control mean statistics – Columns (1), (3), (5) and (7) – refer to the averages for non-displaced individuals. For each outcome (row), I compute the difference between displaced and non-displaced individuals by regressing the individual-level outcome on an indicator for treatment (displaced) status and a set of project fixed effects. The coefficient for the treatment indicator is reported in Columns (2), (4), (6) and (8). Standard errors are presented below each regression estimate and are clustered at the public housing building level. Note that statistical significance is denoted as follows: \*  $p < 0.10$ , \*\*  $p < 0.05$  and \*\*\*  $p < 0.01$ . † These schooling statistics come from Jacob (2004) which examined children from a nearly identical sample of public housing projects subject to building demolition. †† The administrative data on employment begins in the first quarter of 1995. For individuals who experience a demolition in 1995, I use this first quarter of earnings (scaled to an annual figure) as the baseline measure because this first quarter precedes any demolition activity.

Table 2: Impact of Demolition on Neighborhood Characteristics

	(1)	(2)	(3)	(4)
	3 Years After Demolition		8 Years After Demolition	
	Control Mean	Difference: Treated–Control, Within Estimate	Control Mean	Difference: Treated–Control, Within Estimate
Household Has Address (=1)	0.777	0.01 [0.020]	0.656	0.007 [0.020]
<i>Restricted to Households with Address</i>				
Public Housing Address (=1)	0.549	-0.440** [0.042]	0.105	-0.058* [0.033]
Distance from Baseline Address (miles)	1.993	1.409** [0.458]	4.914	-0.447 [0.419]
<b>Tract Characteristics:</b>				
Black (%)	94.897	-2.563** [1.125]	90.042	-0.82 [1.230]
Below Poverty (%)	64.208	-12.929** [2.531]	40.858	-1.571 [1.884]
On Welfare (%)	57.153	-18.365** [2.164]	34.821	-1.392 [1.673]
Unemployed (%)	39.337	-12.422** [1.497]	24.852	-2.363** [1.156]
Violent Crime (per 10,000)	68.855	-23.426** [4.371]	30.801	3.236 [2.547]
Property Crime (per 10,000)	103.247	-15.72 [10.122]	68.675	5.902 [4.269]
N (Households)		2,767		2,767
N (Households with Address)		2,162		1,824

Notes: The unit of analysis in this table is a household with at least one child (age 7 to 18 at baseline). The table shows the analysis of location and neighborhood characteristics in the years following public housing demolition for displaced and non-displaced households. Location is measured using address data from IDHS social assistance case files. Some households lack address data in the years following demolition because they are not on active social assistance cases. The control mean statistics in Columns (1) and (3) refer to averages for non-displaced households. The mean difference between displaced and non-displaced households three years after building demolition is reported in Column (2). This difference is computed from a regression model where the dependent variable is a household location outcome (each row) measured three years after demolition and the independent variables include an indicator for treatment (displaced) status and a set of project fixed effects. Column (4) reports similar results where the location outcome is measured eight years after demolition. Robust standard errors are clustered at the public housing building level. Neighborhood measures such as percent black, percent of residents below poverty and percent on public assistance are measured at the tract level using Census 2000 data. Note that statistical significance is denoted by: \*  $p < 0.10$ , \*\*  $p < 0.05$  and \*\*\*  $p < 0.01$ .

Table 3: Impact of Demolition on Adult Labor Market Outcomes of Children

	Panel Model Results	
	(1)	(2)
	Control Mean	Difference: Treated-Control, Within Estimate
Employed (=1)	0.419	0.040*** [0.014]
Employed Full Time (=1)	0.099	0.013** [0.006]
Earnings	\$3,713.00	\$602.27*** [153.915]
Earnings (> 0)	\$8,856.91	\$587.56** [222.595]
N (Obs.)		35,382
N (Individuals)		5,246

Notes: This table analyzes adult labor market outcomes for displaced (treated) and non-displaced (control) children (age 7 to 18 at baseline). The control mean statistic – Column (1) – refers to averages for non-displaced individuals. The mean difference between displaced and non-displaced children is reported in Column (2). This difference is computed from a regression model where a labor market outcome (each row) is the dependent variable for individual  $i$  in year  $t$ . The independent variables in the regression include an indicator for treatment (displaced) status and a set of project fixed effects. See Equation 1 of the text for more details. The indicator variable for “Employed Full Time” is based on whether an individual makes more than \$14,000 in annual earnings – this is the equivalent of 35 hours a week at \$8 per hour for 50 weeks. Robust standard errors are clustered at the public housing building level. Note that statistical significance is denoted by: \*  $p < 0.10$ , \*\*  $p < 0.05$  and \*\*\*  $p < 0.01$ . Note that the analysis omits some observations (less than one percent) that are outliers in the distribution of earnings.

Table 4: Impact of Demolition on Adult Labor Market Outcomes of Children By Sex

	Panel Model Results			
	(1)	(2)	(3)	(4)
	Males		Females	
	Control Mean	Difference: Treated–Control, Within Estimate	Control Mean	Difference: Treated–Control, Within Estimate
Employed (=1)	0.325	0.017 [0.019]	0.505	0.066*** [0.014]
Employed Full Time (=1)	0.080	0.013 [0.008]	0.117	0.015* [0.008]
Earnings	\$2,946.51	\$417.46* [236.705]	\$4,416.94	\$806.22*** [188.520]
Earnings (>0)	\$9,55.43	\$552.21 [439.299]	\$8,739.53	\$609.26** [274.111]
N (Obs.)		16,876		18,506
N (Individuals)		2,546		2,700

Notes: This table analyzes adult labor market outcomes for displaced (treated) and non-displaced (control) children (age 7 to 18 at baseline) by sex. The control mean statistics – Columns (1) and (3) – refer to averages for non-displaced children. The mean difference between displaced and non-displaced children is reported in Column (2) for males and in Column (4) for females. This difference is computed from a regression model where a labor market outcome (each row) is the dependent variable for individual  $i$  in year  $t$ . The independent variables in the regression include an indicator for treatment (displaced) status and a set of project fixed effects. The indicator variable for “Employed Full Time” is based on whether an individual makes more than \$14,000 in annual earnings – this is the equivalent of 35 hours a week at \$8 per hour for 50 weeks. Robust standard errors are clustered at the public housing building level. Note that statistical significance is denoted by: \*  $p < 0.10$ , \*\*  $p < 0.05$  and \*\*\*  $p < 0.01$ . The analysis omits some observations (less than one percent) that are outliers in the distribution of earnings.

Table 5: Impact of Demolition on Adult Public Assistance Usage of Children

	Panel Model Results					
	(1)	(2)	(3)	(4)	(5)	(6)
	All		Males		Females	
	Control Mean	Difference: Treated–Control, Within Estimate	Control Mean	Difference: Treated–Control, Within Estimate	Control Mean	Difference: Treated–Control, Within Estimate
Any Assistance	0.630	0.013 [0.022]	0.502	0.020 [0.028]	0.746	0.012 [0.025]
Foodstamps	0.509	-0.001 [0.021]	0.349	0.006 [0.023]	0.656	0.000 [0.026]
Medicaid	0.477	0.005 [0.021]	0.307	0.015 [0.025]	0.633	0.002 [0.025]
TANF	0.123	-0.001 [0.010]	0.022	0.005 [0.008]	0.216	-0.002 [0.014]
N (Obs.)		35,532		16,928		18,604
N (Individuals)		5,250		2,547		2,703

Notes: This table analyzes adult public assistance utilization for displaced and non-displaced children (age 7 to 18 at baseline). The control mean statistics – Columns (1), (3) and (5) – refer to averages for non-displaced individuals. The mean difference between displaced and non-displaced children is reported in Column (2). This difference is computed from a regression model where an assistance outcome (each row) is the dependent variable for individual  $i$  in year  $t$ . The independent variables in the regression include an indicator for displaced (treated) status and a set of project fixed effects. See Equation 1 of the text for more details. Robust standard errors are clustered at the public housing building level. Note that statistical significance is denoted by: \*  $p < 0.10$ , \*\*  $p < 0.05$  and \*\*\*  $p < 0.01$ .

Table 6: Impact on Labor Market Outcomes of Parents

	Panel Model Results	
	(1)	(2)
	Control Mean	Difference: Treated-Control, Within Estimate
Employed (=1)	0.489	0.004 [0.015]
Employed Full Time (=1)	0.192	0.015 [0.013]
Earnings	\$6,281.49	\$403.76 [335.892]
Earnings (>0)	\$12,836.39	\$783.19 [478.826]
N (Obs.)		52,028
N (Individuals)		4,077

Notes: This table analyzes labor market outcomes for displaced and non-displaced parents defined as adults (age > 18 at baseline) living in households with children affected by demolition. The control mean statistic – Column (1) – refers to averages for non-displaced individuals. The mean difference between displaced and non-displaced households is reported in Column (2). This difference is computed from a regression model where a labor market outcome (each row) is the dependent variable for individual  $i$  in year  $t$ . The independent variables in the regression include an indicator for treatment (displaced) status and a set of project fixed effects. See Equation 1 of the text for more details. The indicator variable for “Employed Full Time” is based on whether an individual makes more than \$14,000 in annual earnings – this is the equivalent of 35 hours a week at \$8 per hour for 50 weeks. Robust standard errors are clustered at the public housing building level. Note that statistical significance is denoted by: \*  $p < 0.10$ , \*\*  $p < 0.05$  and \*\*\*  $p < 0.01$ .

Table 7: Impact on Adolescent Criminal Activity of Children

	(1)	(2)	(3)	(4)
	All Children (Age 7 to 18 at Baseline)			
	Control Mean	Difference: Treated–Control, Within Estimate	N (Obs.)	N (Individuals)
<b>Adolescent Age Criminal Arrests</b>				
Total	0.369	-0.022 [0.024]	21,097	4,917
Violent	0.086	-0.005 [0.007]	21,097	4,917
Property	0.048	0.008* [0.004]	21,097	4,917
Drugs	0.106	-0.012 [0.011]	21,097	4,917
Other	0.129	-0.013 [0.011]	21,097	4,917

Notes: This table analyzes criminal activity for children (age 7 to 18 at the time of demolition). Note that the sample is restricted to post-demolitions observations where children are between ages 13 to 18 (adolescent ages). This implies that the very oldest children (by baseline age) are excluded from this analysis. The control mean statistic in Column (1) refers to averages for non-displaced children. The mean difference between displaced and non-displaced children is reported in Column (2). This difference is computed from a regression model where an outcome (each row) is the dependent variable for individual  $i$  in year  $t$ . Note that the panel for each individual is restricted to the years after demolition. The independent variables in the regression include an indicator for treatment (displaced) status and a set of project fixed effects. Robust standard errors are clustered by at the public housing building level. Statistical significance is denoted by: \*  $p < 0.10$ , \*\*  $p < 0.05$  and \*\*\*  $p < 0.01$ .

Table 8: CHAC 1997 Voucher Lottery and Re-Weighted Demolition Analysis

(a) Voucher Use and Mobility Analysis				
	(1)	(2)	(3)	(4)
	Control Mean	Treated Mean	<i>p</i> -value Difference: Treated–Control	N
Leased Using CHAC 1997 Voucher	0.00	0.49	0.00	4,702
Leased Using Any Voucher	0.31	0.63	0.00	4,702

(b) Lottery Sample: Effect of Vouchers				
	(1)	(2)	(3)	(4)
	Control Mean	Difference: Treated-Control (Reduced Form)	LATE (2SLS)	Control Complier Mean
Employed (=1)	0.463	-0.008 [0.013]	-0.025 [0.041]	0.482
Employed Full-Time (=1)	0.125	-0.003 [0.008]	-0.008 [0.026]	0.125
Earnings	\$4,724.83	\$3.04 [258.574]	\$9.37 [794.805]	\$4,586.11
Earnings (>0)	\$10,214.81	\$256.05 [380.898]	\$788.83 [1173.760]	\$9,263.54
N (Obs.)		33,718	33,718	
N (Individuals)		4,661	4,661	

(c) Re-weighted Demolition Sample: Reduced Form Estimates		
	(1)	(2)
	Control Mean	Difference: Treated-Control (Reduced Form)
Employed (=1)	0.430	0.060*** [0.015]
Employed Full-Time (=1)	0.105	0.013** [0.005]
Earnings	\$3,891.26	610.087*** [125.687]
Earnings (>0)	\$9,058.31	\$206.50 [225.969]
N (Obs.)		35,382
N (Individuals)		5,246

Notes: This table displays the results from analyzing the CHAC 1997 lottery and the re-weighted demolition sample. Panel (a) describes household voucher use for households that signed up for the 1997 CHAC housing voucher lottery. Panel (b) analyzes adult labor market outcomes for children (age 7 to 18 at baseline). The control mean statistic in Panel (b) Column (1) refers to averages for children whose household did not win a voucher offer. The reduced form difference is calculated by regressing the outcome (row) on an indicator for winning a voucher offer through the CHAC 1997 lottery. The 2SLS results are obtained by estimating a first-stage where the dependent variable is an indicator for using any housing voucher and the instrument is an indicator for winning a CHAC 1997 housing voucher. Panel (c) revisits the impact of mandatory relocation due to demolition after re-weighting the demolition sample to match observed characteristics in the lottery sample. As explained in the text, I use propensity score weights to achieve balance in observed characteristics between the two samples. Note that the indicator variable for “Employed Full Time” is based on whether an individual makes more than \$14,000 in annual earnings – this is the equivalent of 35 hours a week at \$8 per hour for 50 weeks. Robust standard errors are clustered at the public housing building level. Note that statistical significance is denoted by: \*  $p < 0.10$ , \*\*  $p < 0.05$  and \*\*\*  $p < 0.01$ .



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