# 13 Appendix

## A1 Detailed Analysis of Differential Attrition

As explained in Section 5.1, I use the following specification for my analysis of the impact of demolition:

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

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

One identification condition for this analysis is that $cov(A_{i,t}, \epsilon_{i,t}) = 0$ where $A$ is a binary indicator of attrition. While I do not actually observe $A$, I follow Grogger (2013) and impute $A$ using various administrative sources. Specifically, the measure of attrition that I calculate is straightforward. Permanent attrition at time $t$ implies that an outcome is zero after the point of departure (i.e. $Y_{i,t+j} = 0 \ \forall j \ \in \{1, \ldots, T-t\}$, where $Y$ is an administrative data outcome and $T$ denotes the last unit of time in the data). For a single outcome $k$, I measure attrition by creating a binary indicator of a $d$-period run of zeros as

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

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

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

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

Table A1 shows the distribution of terminal runs of zeros by the year in which the run begins. The first three pairs of columns report statistics based on terminal runs for three different outcomes:

(1) employment, (2) foodstamp receipt and (3) TANF or Medicaid receipt. The first column in each pair reports the probability that a terminal run is observed in a given post demolition year for the sample of non-displaced youth. For example, the first entry of the first column shows that 20.8 percent of non-displaced youth began a terminal run of employment zeros in the first year after demolition. By the definition of terminal run, this sequence was 14 years-long in the first year after demolition. In the second year after demolition, the probability of observing a terminal run of zeros was 21.5 percent. Note that in the second year post demolition, the definition of a terminal run is a 13 year-long sequence. Because the length of the terminal sequence of zeros shrinks in each row, the probability of observing a terminal run of zeros grows over the sample period. Based on the employment data alone, the imputed attrition is 63.1 percent in the final post-demolition year of the sample. Imputed attrition is slightly lower based on data for assistance outcomes as shown in Columns (3) and (5) of Table A1.

Attrition as measured by pooling these administrative sources is reported in Column (7). Combining the three data series dramatically affects the distribution of terminal runs of zeros. Based on the three outcomes, less than 2 percent of the sample begins a terminal run of zeros in the first year after demolition. This contrasts with the 20.8 for employment in isolation. Moreover, attrition based on all three measures is only 30.3 percent in the final year of the sample, which is less than half of the imputed attrition as measured using the employment data alone. This dramatic affect on the distribution is primarily due to the negative correlation among the outcomes under consideration.

The main concern in this analysis is whether demolition appears to be correlated with imputed attrition. For each pair of columns that pertain to a particular outcome in Table A1, the second column of the pair reports the regression computed difference in the probability of attrition for displaced (treated) and non-displaced (control) adolescents who were age 7 to 18 at the time of demolition. Specifically, I use Equation 1 where the outcome is imputed attrition $a_{i,t}^{k}$. There is no strong evidence of differential attrition by treatment status for any of the single outcomes in isolation. Across the three outcomes in 14 post-demolition years, the difference between the treated and control probability of attrition is statistically significant in just two of the 42 possibilities (5 percent). More importantly, Column (8) shows that there is no detectable difference in the probability of observing a terminal run of zeros in any post demolition year after pooling all three outcomes.

## A2 Detailed Description of Sample Definition

As stated in Section 4, one of the main data sources for this paper is data on social assistance participation from the Illinois Department of Human Services (IDHS). The raw sampling frame for the data used in this paper is the set of individuals ("grantees") living in Cook County who received some form of social service assistance (specifically, TANF/AFDC, Food Stamps or Medicaid) at *any* point between June 1, 1994 and July 1, 1997. Note that the record for using social assistance during this time period is referred to as the "target case". With the initial list of grantees, IDHS created a list of other members of the grantee's household. These additional household members are identified as the set of additional individuals listed on the grantee's target case. Using this definition for the sampling frame, the raw IDHS data contains 992,729 individuals (463,542 are grantees while 529,187 are individuals living in the same household). Note that everyone in this sample of social assistance households has a unique ID code created by Chapin Hall at the University of Chicago. Chapin Hall uses IDHS data on social assistance utilization to define a unique identifier for individuals who appears in these data. Using information such as name, date of birth and social security number, Chapin Hall used a probabilistic matching technique to link these IDHS-based identifiers to other administrative data such as Illinois state employment data and Illinois State Police (ISP) records, which I also use in the present paper.

## A3 Program Rules for Housing Vouchers

### A3.1 Voucher Eligibility

Unlike other major social programs, housing vouchers are *not* an entitlement, and there are long waiting lists to receive housing assistance in many large cities. Housing voucher program eligibility is based on the local median household income. For example, a family of four is eligible for assistance if they fall under 50 percent of the local median income for all families in an area (although some families with incomes up to 80 percent of the local median income may be eligible depending on their location) (Olsen, 2003). Note that, unlike other means-tested programs, there are no asset tests for eligibility for housing vouchers. The eligibility limits for families of different sizes are equal to the following percentages of the four-person limit:

Housing Voucher Income Eligibility Adjustment by Family Size (Percentage of Four-Person Limit)

| Family Size | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
|---|---|---|---|---|---|---|---|---|
| Adjustment | 70 | 80 | 90 | 100 | 108 | 116 | 124 | 132 |

Notes: All numbers are taken from Olsen (2003), p. 379.

### A3.2 The Value of the Subsidy

There are two main components for determining the value of a housing voucher. First, the value of a voucher depends on the local Fair Market Rent (FMR) which is set by the U.S. Department of Housing and Urban Development (HUD). In 1995, the FMR was equal to the 40$^{\text{th}}$ percentile of the local rent distribution for a unit of a given size. For example, the FMR for a two-bedroom apartment in Chicago was equal to $699 (nominal dollars) in 1995. Starting in 2001, the FMR was raised to the 50th percentile in some specific metropolitan areas, including Cook County, Illinois (in which Chicago resides). Second, the value of the voucher depends on household income. Specifically, a fraction of the income – 30 percent – must be paid toward rent. Hence, the value of a housing voucher is given by:

$$\begin{aligned}
\text{Subsidy Value} &= \text{FMR} - S \\
S &= \max\{0.3 \times Y_{ah}, 0.1 \times Y_{gh}\} \\
Y_{ah} &= \text{Adjusted income under housing program rules} \\
&= \text{Earnings} + \text{TANF} \\
&\quad - (\$480 * \text{Children}) - (\$400 * \text{Disabled}) \\
&\quad - \text{Child care expenses} \\
&\quad - \text{Medical care expenses} \\
&\quad - \text{Attendant care expenses for disabled family} \\
Y_{gh} &= \text{Gross household income} \\
&= \text{Earnings} + \text{TANF}
\end{aligned}$$

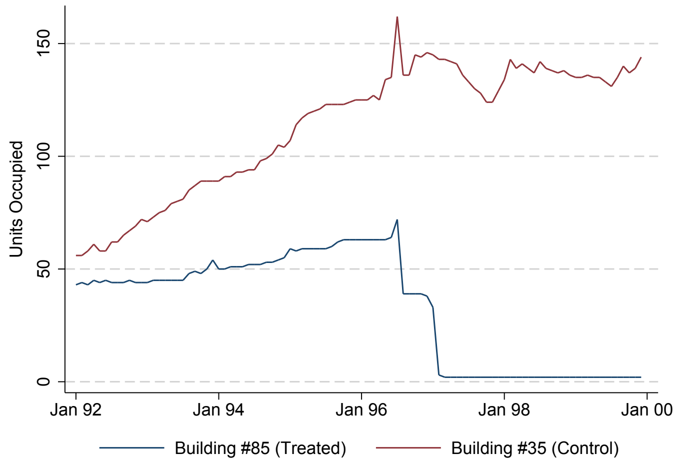
Note that TANF benefits are included when determining program eligibility and the family's rent contribution, while the value of other forms of government assistance such as foodstamps, EITC and Medicaid are not. Earnings by children younger than age 18 or payments received for foster children are also not counted under the voucher program rules. Medical care and attendant care expenses must exceed 3 percent of annual income.

Note that families offered housing vouchers usually have a limited time to lease a private market unit. The time limit is usually 3 to 6 months after initial receipt of the offer. In addition to the time limit, families must also obtain a private-market housing unit that meets HUD's minimum quality standards. As noted in previous work studying vouchers, landlords may prefer non-voucher tenants because of these quality standards or other paperwork associated with the voucher program. Finally, also note that once an individual qualifies for a housing voucher, they are not removed from the program if their income exceeds the eligibility limit. Of course, the value of the subsidy diminishes as income rises because a fraction of household income (generally 30 percent) must be paid toward rent.

## A4 Determining Dates of Building Closure Due to Demolition

The date of closure for demolished (treated) buildings used in this paper is taken from Jacob (2004). As explained in the appendix of his paper, Jacob determines the date of building closure by examining trends in administrative data on building-level occupancy rates. Specifically, the year of closure can be determined by *sharp declines* in building occupancy. As an example, the figure below shows how the year of closure is determined from occupancy data. Occupancy at building #85 (the blue line)of the Washington Park project drops notably in early 1996 and later falls to zero starting in 1997. Because CHA policy requires tenants to be notified at least 120 days prior to building closure, the pattern in the occupancy data implies that residents of building #85 knew the building would close in late 1995. As a point of comparison, the figure also shows occupancy at a stable (control) building. We see that occupancy in building #35 (the red line) is relatively stable after 1995 which is the year of the first closures due to demolition at Washington Park. In addition to using administrative data on occupancy, Jacob (2004) also used information from interviews with CHA officials, housing advocates and the presidents of the Local Advisory Councils (LACs) in projects affected by building demolition during the 1990s.

Occupancy Trends and the Date of Building Closure Due to Demolition




Notes: The figure displays monthly occupancy at two buildings at the Washington Park project in Chicago. Occupancy data is from administrative records from the Chicago Housing Authority (CHA).

## A5 CHAC 1997 Lottery Data and Summary Statistics

### A5.1 Data and Summary Statistics

The data sources used in this paper's analysis of the CHAC housing voucher lottery have been used previously in Jacob and Ludwig (2012) and Jacob et al. (2015). As mentioned in Section 9, these two studies examine individuals (adults and children) living in private market housing at the time that they applied for the CHAC housing voucher which differs from this paper's interest in youth living in public housing. The following discussion, which describes the lottery data, is similar to the information provided in the online appendices for Jacob and Ludwig (2012) and Jacob et al. (2015).

The starting point for creating the lottery analysis sample is the application data for the 82,607 adults who applied for a CHAC housing voucher in 1997. These files include information on lottery number and basic household demographics. In addition, the application data also contains baseline address data which allows me to identify the subset of lottery applicants that are the focus of my analysis: applicants living in public housing at baseline. Note that one drawback of the application data is that these data do not contain information on other members of the applicant's household such as children. Instead, I obtain a list of these household members by linking the application data to the Illinois Department of Human Services (IDHS) data. The procedure for linking applicants to IDHS data is described in greater detail below in A5.2. Note that this linking process uses entirely pre-lottery data, so measurement error in identifying non-applicant household members is orthogonal to winning the housing voucher lottery.

To measure labor market activity, I link data from the Illinois Department of Employment Security (IDES) to the sample of individuals identified in IDHS data as living in lottery households at baseline. The link between the IDES and IDHS data is based on probabilistic matching techniques using name, date of birth and Social Security number. The IDES lets me examine both earnings and labor market participation (defined as having any positive earnings) over time for individuals residing in the state of Illinois.

Similar to my analysis of relocation due to project-demolition, I rely on address information contained in IDHS case records to examine neighborhood outcomes. Recall a concern is that address information is not available when youth are not on active social assistance cases. If this attrition from the IDHS data is correlated with winning a lottery, it may bias estimates of the impact on neighborhood quality. In the following results section, I show that lottery winning has no statistically significant effect on the probability of having an active social assistance case suggesting that my mobility analysis is not biased by differential sample attrition.

Linking lottery application and administrative data allows me to construct panel data for each youth from the baseline year 1997 to 2009. As mentioned above, I limit my main analysis focuses on the set of lottery applicant households living in project-based public housing at baseline. The sample in my CHAC 1997 analysis has $4,661$ children who are between ages 7 and 18 at randomization.

The table on page Appendix - 8 presents summary statistics on my main analysis sample of children. Nearly the entire sample (98 percent) of my sample is African American and lives in a disadvantaged household at baseline. Among adults living in youth households, only 33 percent were employed and average annual income was about $4,300. Nearly 77 percent of adults received some form of social assistance such as TANF, Medicaid or Foodstamps.

Finally, the key to my analysis is that the CHAC randomly assigned its voucher offers in its 1997 lottery. The descriptive statistics in the table on page Appendix - 8 provide evidence consistent with such random assignment for my main analysis sample of children. The mean values of children and adults in treated and control households are nearly identical. None of the 23 pair-wise differences is significant at the 5 percent level.

Descriptive Statistics for the CHAC 1997 Housing Voucher Lottery Sample

| | (1) Control Mean | (2) Treated Mean | (3) $p$-value Difference: Treated–Control |
|---|---|---|---|
| **Panel A. Children (Age 7-18)** | | | |
| **Demographics** | | | |
| Black (=1) | 0.98 | 0.98 | 0.97 |
| Age | 11.69 | 11.81 | 0.28 |
| Male (=1) | 0.49 | 0.47 | 0.23 |
| **Arrests (Age>13)** | | | |
| Violent | 0.03 | 0.03 | 0.49 |
| Property | 0.02 | 0.02 | 0.41 |
| Drugs | 0.05 | 0.05 | 0.90 |
| Other | 0.03 | 0.03 | 0.94 |
| N (Individuals) | 3,402 | 1,300 | |
| **Panel B. Adults in Households with Children** | | | |
| Black (=1) | 0.97 | 0.98 | 0.78 |
| Age | 31.16 | 31.34 | 0.47 |
| Male (=1) | 0.18 | 0.2 | 0.10 |
| Any Arrest | 0.77 | 0.76 | 0.83 |
| Employed (=1) | 0.37 | 0.38 | 0.22 |
| Earnings | $4,340.24 | $4,422.76 | 0.70 |
| Any social assistance (=1) | 0.77 | 0.75 | 0.09 |
| N (Individuals) | 4,694 | 1,781 | |
| **Panel C. Households with Children** | | | |
| # of Kids | 2.59 | 2.66 | 0.31 |
| **Neighborhood** | | | |
| Percent Black | 89.14 | 89.45 | 0.81 |
| Percent Below Poverty Line | 67.14 | 66.88 | 0.81 |
| N (Households) | 1,464 | 556 | |

Notes: All descriptive statistics are for children (age 7 to 18 at baseline) or adults in these households with children.

### A5.2 Linking the CHAC Applicants to Other Households Members

Since the CHAC 1997 lottery application form data do not include identifying information for other household members such as children, Jacob and Ludwig contracted Chapin Hall at the University of Chicago to match CHAC applicants to administrative data on social program participation from the Illinois Department of Human Services (IDHS). Chapin Hall matched these two data sources using name, date of birth and Social Security numbers and successfully linked nearly 94 percent of CHAC applicants to the IDHS data. For each CHAC applicant who matched to the IDHS data, Chapin Hall identified the spell of social program participation – referred to hereafter as the "target case" – that was closest in time prior to the date of the CHAC lottery drawing (July 1, 1997). Individuals (such as children) linked to these target cases are counted as residing in the CHAC applicant's household and included in the lottery analysis sample. For further details on this process of imputing household members see the online appendix for Jacob et al. (2015).

For the present paper, I focus on children (age 7-18 at baseline) who are members of households that reported living in public housing at the time they applied for a housing voucher. Note that baseline residency (address) information is taken from the CHAC 1997 lottery application forms. The list of youth affected by the CHAC lottery is merged to longitudinal administrative data using unique identifiers created by Chapin Hall. These identifiers link individuals across data sources and are created by matching on name, date of birth and social security number.

Specifically, the sample of children living in public housing is merged to the following sources: (1) Illinois State Police (ISP) data recording all arrests up to the first quarter of 2012; (2) Illinois Department of Employment Security (IDES) data on quarterly earnings (1995-2009) and (3) IDHS data on AFDC/TANF, foodstamp and Medicaid participation (1989-2009). Note that these administrative data are also used for the analysis of youth affected by public housing demolition presented in Section 6.

## A6 An Economic Model of Reverse Roy Selection in Voucher Programs

This section presents a stylized model of parental investment in child outcomes to explain the pattern of negative selection in the context of a housing mobility program. In particular, the model accounts for two salient features of MTO. First, 80 percent of MTO participants listed fear of crime as their main motivation for joining the program (Orr et al., 2003). In addition, 24 percent of MTO participants reported that someone in their household had been beaten or assaulted in the past six months. This rate of victimization was about four times greater than contemporaneous statistics for other public housing households (Zelon, 1994). Second, fear of neighborhood crime affected parental behavior in MTO households. Kling et al. (2001) interviewed MTO participants before the program and found that "[f]ear has led mothers to constantly monitor their children's activities." Notably, studies of MTO showed that the program reduced parents' active supervising behavior, plausibly because parents who moved felt safer in their new neighborhood (Kling et al., 2001).

A simple model captures this context and generates negative selection into a housing mobility (voucher) program. Assume that each parent living in public housing has a different belief about the safety of their neighborhood $q_i$. Parents care about their own consumption $p_i$ and their child's outcome $Y_i$. Let them believe that their child's outcome is a function of their parenting effort $e_i$ (e.g., active parental monitoring) and their perception of neighborhood safety $q_i$. To ensure that parents face tradeoffs, assume that there is a budget constraint: $I = p_i + e_i$.

If parents have no ability to move, they make different investments in their children based on their beliefs about the relative safety (high $q_i$) or danger (low $q_i$) of their neighborhood. To make this point clearly, consider the following parameterization of the parent's preferences:

$$U(p_i, Y_i) = log(p_i) + log(Y_i).$$

Parents maximize utility subject to the budget constraint and the child production function which I specify as $Y_i = e_i + q_i$. To optimize, parents choose higher $e_i$ when they have a relatively low $q_i$. This compensatory behavior is driven by the assumption that parental effort and neighborhood safety are substitutes, which aligns with the behavior of MTO parents who reduced their child monitoring activities after moving to lower poverty neighborhoods.

Now consider how parents respond if an experimental housing voucher program such as MTO program begins recruiting. Assume that the program randomly offers parents the chance to win a housing voucher with probability $\pi$. Parents can use the voucher to lease private market housing in a new neighborhood that has high safety $\theta$, where $\theta > \bar{q}$ and $\bar{q}$ denotes the mean of parents' perception of neighborhood safety. In this case, parents with sufficiently low $q_i$ are incentivized to move because they believe their child will live in a much safer neighborhood. Finally, to complete the model, let us assume that there is a utility cost $c$ which ensures that parents with high values of $q_i$ will not want to move through MTO.

In this case, the model fits into the treatment effects framework presented in Jones (2015) and Pinto (2015), and solving the model proceeds in two stages. In the first stage, parents decide whether they want to volunteer ($V_i = 1$) or not ($V_i = 0$). Next, parents choose optimal consumption $p_i^*(D_i)$ and effort $e_i^*(D_i)$ as solutions to a second-stage problem that takes into account whether they are in the program and they receive a voucher ($D_i = 1$). (To be clear, there is no compliance problem in this model. A parent who signs up for MTO and is assigned to the treatment group always uses their voucher.)

Based on the solutions to the second stage, the first-stage decision is a cutoff condition based on the realization of $q_i$. Specifically, parents predict their consumption in the second stage and choose to volunteer if the following inequality holds:

$$\underbrace{\pi U(p^*(1), y_i^*(1)) + (1-\pi)U(p_i^*(0), y_i^*(0))}_{\text{Expected Payoff to } V_i = 1} > \underbrace{U(p_i^*(0), y_i^*(0))}_{\text{Payoff to } V_i = 0}$$

Note that the cost $c$ is borne by the parent only if she is assigned to the treatment group and thereby induced to move.

This two-stage model yields a simple expression for the volunteering decision.[66] Specifically, the log-utility functional form implies that we can re-write the volunteering condition as:

$$\begin{gathered} 2\ log \left(\frac{I-\theta}{I+q_i}\right) > c \\ \Rightarrow \underbrace{\frac{I+\theta}{exp^{.5c}}}_{\equiv \gamma} > q_i \end{gathered}$$

In other words, parents with sufficiently poor perceptions of neighborhood safety – below $\gamma$ – will select into MTO.

With this in mind, we can consider treatment effects generated by the experiment when there are heterogeneous perceptions of neighborhood safety that generate different parental investments. Importantly, note that parents choose to volunteer in the program based on their idiosyncratic signal of neighborhood safety $q_i$, the mean of which is $\bar{q}$ and is assumed to be the actual level of

[66] Note that the model is solved via backward induction whereby the parent forecasts consumption and utility when they volunteer for MTO.

neighborhood safety. That is, I assume that the actual outcome for a child who does not move is $Y_i = e_i + \overline{q}$. This setup for the model implies that parents with low values of $q_i$ are overly concerned about neighborhood safety.

Now, define the potential outcomes for each child as:

$$Y_{1i} = e_i^*(1) + \theta$$

$$Y_{0i} = e_i^*(0) + \overline{q}$$

where each expression shows that the difference in potential outcomes (and treatment effect heterogeneity) is due to different parental choices for child investment. With selective volunteering, we can use the demand functions to examine treatment effects for children. For the sake of illustration, let us assume that the parent's signal $q_i$ is a normally distributed random variable with mean $\overline{q}$ and variance $\sigma^2$.

In this case, the effects for children of MTO volunteers are:

$$\mathbb{E}(Y_{1i} - Y_{0i}|\gamma > q_i) = .5\left(\theta - \overline{q} + \sigma\frac{-\phi(\alpha)}{\Phi(\alpha)}\right)$$

where $\alpha = (\gamma - \lambda)/\sigma$. This expression shows that the average treatment effect for participants is decreasing in the standard deviation of parents' perception of safety. Correspondingly, the effects for children of non-volunteers are:

$$\mathbb{E}(Y_{1i} - Y_{0i}|\gamma < q_i) = .5\left(\theta - \overline{q} + \sigma\frac{\phi(\alpha)}{1 - \Phi(\alpha)}\right)$$

where treatment effects are now increasing in the variance of parents' beliefs about neighborhood safety.

The key point in this model is that the effects for children of non-volunteers exceed effects for children of volunteers. Intuitively, this occurs because parents who select into the experiment would have chosen high levels of $e_i$ if they do not move through MTO. Correspondingly, if these fearful households move via MTO, they reduce $e_i$ because their new neighborhood has relatively high safety. Again, this effect corresponds to MTO reports, which note that treated parents who moved were less likely to engage in intense parental monitoring relative to parents in control households.

In contrast, parents with high values of $q_i$ are overly optimistic about their neighborhood safety. These households will forgo MTO and choose low values of $e_i$. This implies that forced relocation would generate large benefits for children because non-volunteers engage in less active child monitoring. Hence, this simple model, which features heterogeneity in beliefs, can explain the Reverse Roy pattern of treatment effects that appears when comparing the impact of vouchers allocated by MTO or Chicago's public housing demolition.

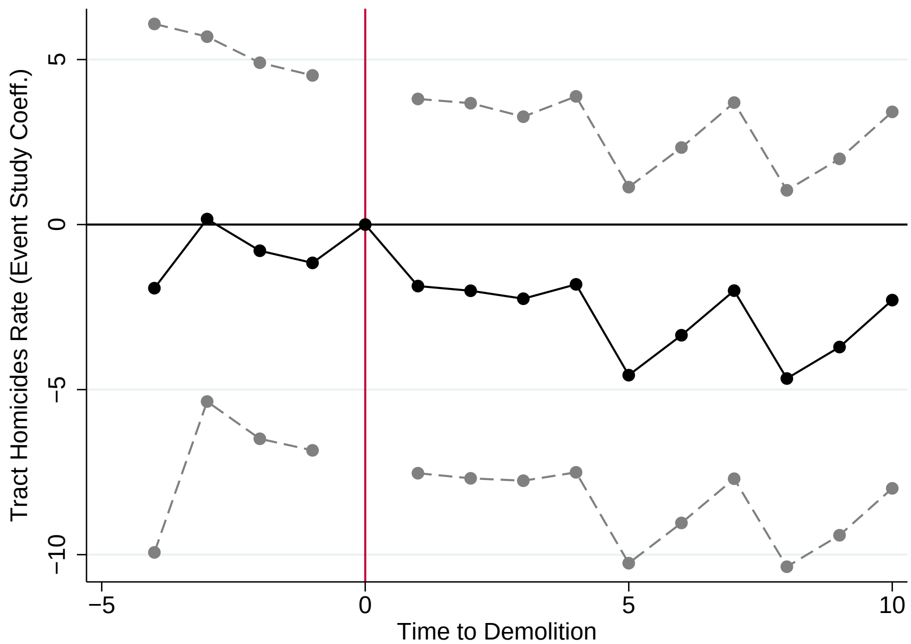
But, is this model reasonable? One way to address this question is to illustrate the quantitative implications of the model by examining treatment effects on child outcomes after calibrating the model. For this illustration, I will focus on the studying effects on adult earnings of children. The model has five parameters: (1) household income $I$; (2) private market housing quality $\theta$; (3) the mean of public housing neighborhood quality $\overline{q}$; (4) the variance in signals about neighborhood quality $\sigma^2$; and (5) the (utility) cost $c$ of moving through a voucher program. To calibrate the model, I assume that the annual household income is \$1,700 which was the mean household income for families living in public housing projects subject to Chicago's demolitions during the 1990s. In addition, I normalize the value of living in private market housing $\theta$ to zero.

With these assumptions, I can use three moments from the data to solve for the three remaining parameters: $\overline{q}$, $\sigma$ and $c$. First, we have an equation for aggregate volunteering. In the data, we know that about 25 percent of public housing parents opted into the CHAC 1997 lottery. In the model, this implies that the parameters need to be set such that $\Phi\left((\gamma - \overline{q})/\sigma\right) = 0.25$. Second, the model should be able to generate to generate the earnings treatment effects observed for children of parents that joined the 1997 CHAC voucher lottery which is about roughly \$10 (although this is an imprecise estimate). In terms of the model, this implies $10 = \mathbb{E}(Y_{1i} - Y_{0i}|\gamma > q_i) = .5\left(-\overline{q} - \sigma\phi(\alpha)/\Phi(\alpha)\right)$. Similarly, the model should also generate the treatment effects for non-participants. A back of the envelope calculation using the effects observed in the demolition and lottery samples suggests that this is roughly \$770, and this implies: $770 = E(Y_{1i} - Y_{0i}|\gamma < q_i) = .5\left(-\overline{q} + \sigma\phi(\alpha)/(1 - \Phi(\alpha))\right)$.[67] Solving for these three equations provides the following model parameters: $\overline{q} = 1129$, $\sigma = 906$ and $c = 9.44$. Hence, this simple calibration of this stylized model matches the observed pattern of Reverse Roy selection when there is seemingly moderate variation in parents' beliefs about neighborhood quality ($q_i$) and a moderate utility cost of moving ($c$).

[67] The effect observed in the demolition sample can be interpreted as the average treatment effect of moving using a voucher which is a weighted average of the effects for children whose parents would *and* would not voluntarily seek vouchers. In terms of the model, we can write this as: $\mathbb{E}(Y_{1i} - Y_{0i}) = \mathbb{E}(Y_{1i} - Y_{0i}|\gamma < q_i)\mathbb{P}(\gamma < q_i) + \mathbb{E}(Y_{1i} - Y_{0i}|\gamma > q_i)\mathbb{P}(\gamma > q_i)$. The estimate for the participation rate in the CHAC 1997 housing voucher lottery and the treatment effects in the demolition and lottery samples imply that the effects for non-participants are \$770.

## A7 Appendix Figures and Tables

Figure A1: Homicide Rate Before and After Demolition: Event Study Coefficients and 95-Percent Confidence Interval

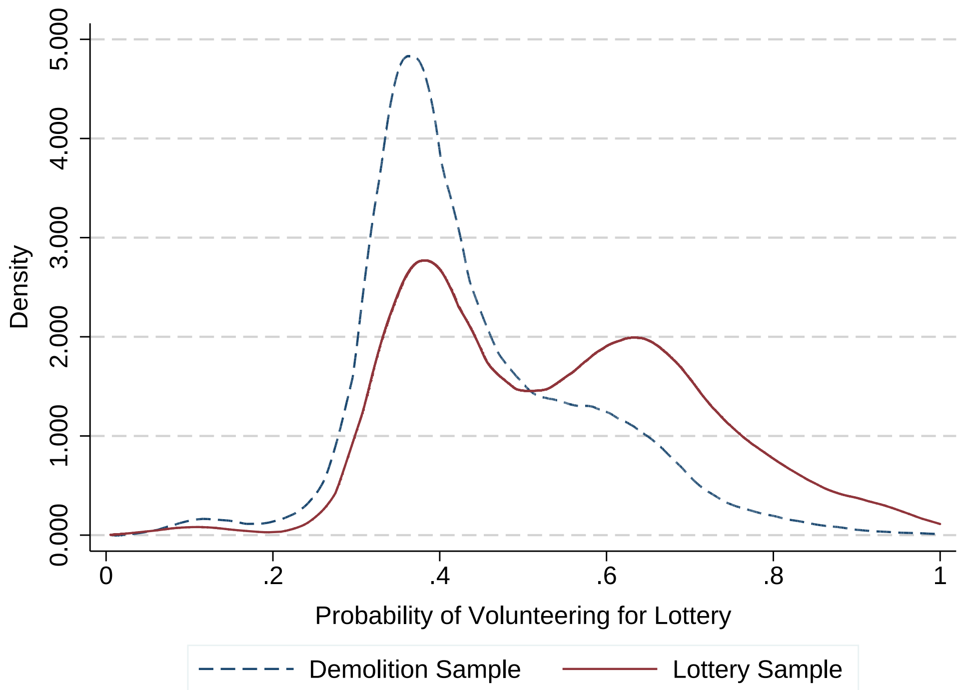



Notes: This figure plots event-study coefficients (solid black dots) from a regression of the tract-level homicide rate on time-dummies. Specifically, the figure plots a set of coefficients $\pi_j$ and $\tau_j$ from the following specification:

$$h_{i,t} = \mu_i + \sum_{j=-4}^{-1} \pi_j \ 1(t - t^* = j) + \sum_{j=1}^{10} \tau_j \ 1(t - t^* = j) + \delta_t + \epsilon_{s,t}$$

where the dependent variable $h_{i,t}$ is homicide rate for 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 treatment (displacement). 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 is one year before the policy is implemented. I restrict the estimation sample to include (1) tracts that contained public housing which had at least one building demolition and (2) tracts that are within 1 mile of a public housing demolition site. By definition, all tracts included in this specification are treated at some time. The data contains 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. Grey dots and dashed lines illustrate the 95-percent confidence interval for the coefficients. Data comes from the extended version of Block and Block's Homicides in Chicago (ICPSR #6399).

Figure A2: Propensity Score Distribution




Notes: The figure shows kernel density estimates of the propensity scores for demolition and lottery households, respectively. I construct propensity scores by pooling data on household characteristics for both samples. The unit of observation is at the household level and I estimate a probit with the binary dependent variable equal to 1 if a household selected into the lottery sample. Variables included in the propensity score include baseline measures of the following: (1) the number of criminal arrests (by category for violent, property, drugs and other crimes), (2) household labor market outcomes such as total household income and the fraction of adults that are working in the household, (3) demographic characteristics such as the number of adults and children in the household and (4) measures of past criminal arrests for children. Note that I trim the sample for the figure to exclude propensity scores below 0.01 and above 0.99. These covariates allow me to construct the estimated propensity score $p_i = \mathbb{P}r(\text{Lottery}_i = 1|X_i)$ which I use to construct weights $w_i = p_i(1-q)/(1-p_i)q$ where $q$ is the overall fraction of the pooled household sample that participates in the housing voucher lottery. These weights are used in my analysis of the impact of demolition on long-run child outcomes.

Table A1: Testing for Differential Attrition Using Administrative Data, Child (Age 7 to 18 at Demolition) Sample

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Employment | | Foodstamps | | TANF/Medicaid | | All Three Outcomes | |
| Years Since Demolition ($d$) | Probability of Attrition by Year $d$ | Difference: Treated–Control, Within Estimate | Probability of Attrition by Year $d$ | Difference: Treated–Control, Within Estimate | Probability of Attrition by Year $d$ | Difference: Treated–Control, Within Estimate | Probability of Attrition by Year $d$ | Difference: Treated–Control, Within Estimate |
| 1 | 0.208 | -0.012 | 0.068 | -0.014 | 0.054 | 0.005 | 0.014 | -0.022 |
| | | [0.027] | | [0.027] | | [0.020] | | [0.017] |
| 2 | 0.215 | -0.012 | 0.098 | -0.015 | 0.103 | 0.002 | 0.027 | -0.019 |
| | | [0.027] | | [0.027] | | [0.022] | | [0.018] |
| 3 | 0.224 | -0.002 | 0.135 | -0.023 | 0.156 | -0.002 | 0.040 | -0.023 |
| | | [0.027] | | [0.026] | | [0.024] | | [0.019] |
| 4 | 0.241 | 0.005 | 0.165 | -0.011 | 0.200 | -0.002 | 0.054 | -0.016 |
| | | [0.028] | | [0.028] | | [0.026] | | [0.021] |
| 5 | 0.260 | 0.012 | 0.198 | -0.003 | 0.244 | -0.002 | 0.070 | -0.011 |
| | | [0.028] | | [0.029] | | [0.025] | | [0.021] |
| 6 | 0.283 | 0.008 | 0.235 | -0.002 | 0.293 | 0.028 | 0.090 | 0.003 |
| | | [0.028] | | [0.028] | | [0.023] | | [0.022] |
| 7 | 0.315 | -0.005 | 0.268 | 0.003 | 0.338 | 0.034 | 0.111 | 0.007 |
| | | [0.028] | | [0.027] | | [0.023] | | [0.023] |
| 8 | 0.343 | -0.003 | 0.305 | 0.012 | 0.394 | 0.029 | 0.133 | 0.016 |
| | | [0.027] | | [0.028] | | [0.025] | | [0.024] |
| 9 | 0.377 | 0.002 | 0.336 | 0.022 | 0.445 | 0.024 | 0.157 | 0.011 |
| | | [0.028] | | [0.028] | | [0.026] | | [0.025] |
| 10 | 0.419 | 0.026 | 0.380 | 0.035 | 0.468 | 0.029 | 0.183 | 0.017 |
| | | [0.028] | | [0.029] | | [0.027] | | [0.027] |
| 11 | 0.479 | 0.054* | 0.427 | 0.051* | 0.489 | 0.029 | 0.220 | 0.039 |
| | | [0.031] | | [0.027] | | [0.028] | | [0.028] |
| 12 | 0.471 | -0.01 | 0.441 | 0.006 | 0.495 | 0.003 | 0.221 | -0.011 |
| | | [0.035] | | [0.024] | | [0.036] | | [0.033] |
| 13 | 0.550 | -0.011 | 0.490 | 0.015 | 0.525 | 0.004 | 0.264 | -0.018 |
| | | [0.031] | | [0.029] | | [0.032] | | [0.031] |
| 14 | 0.631 | -0.042 | 0.525 | 0.021 | 0.542 | 0.016 | 0.303 | -0.015 |
| | | [0.029] | | [0.031] | | [0.034] | | [0.038] |

Notes: This table presents tests for differential attrition based on the administrative data for children (age 7 to 18 at baseline) in my sample. Specifically, I follow Grogger (2013) and construct a measure of attrition based on terminal runs of zeros for a given outcome (e.g. employment) measured in an individual-level panel. For each different outcome, columns (1), (3) and (5) report the probability of observing a terminal run of zeros that begins in a given post demolition year for non-displaced children. For example, the first entry of the first column shows that 20.8 percent of the non-displaced sample of youth began a terminal run of employment zeros in the first year after demolition. Note that for the first entry the definition of a terminal run is a 14 year period. Columns (2), (4) and (6) test whether displaced and non-displaced youth have detectably different rates of attrition. Specifically, these columns report the difference in attrition computed by regressing an indicator for attrition on a dummy for treated (displaced) status and a set of project fixed effects. See Section 4.3 for further details. Columns (7) and (8) examine attrition by pooling data sources.

Table A2: Spillover Specification Results: Adult Outcomes for Children

| | (1) | (2) | (3) |
|---|---|---|---|
| | All Children | | |
| | Control Mean | Difference: Treated–Far, Within Estimate ($\beta'$) | Difference: Near–Far, Within Estimate ($\pi$) |
| Employed (=1) | 0.419 | 0.044** | 0.005 |
| | | [0.017] | [0.014] |
| Earnings | $3,713.00 | $513.422** | $-122.782 |
| | | [195.356] | [167.376] |
| Total Arrests | 0.358 | -0.031 | 0.017 |
| | | [0.040] | [0.027] |

Notes: Children are age 7 to 18 at the time of demolition. In the table, "near" refers to the group of children who lived in a public housing building that was adjacent to a building that was demolished while "far" refers to the group of children who lived in public housing buildings that were not adjacent to demolished buildings. The control mean statistics – Columns (1) and (4) – refer to the averages for non-displaced individuals living in the group of far buildings. The regression estimates are from a spillover specification as specified in the text in Equation 2. As described in the text, the estimate $\beta'$ is the difference in outcomes between displaced and non-displaced children who are part of the far group. Similarly, the estimate $\pi$ is the difference in outcomes between non-displaced children in the near group and non-displaced children in the far group. Standard errors are presented below each regression estimate and are clustered at the public housing building level. Note that statistical significance is denoted by: $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$.

Table A3: Earnings Quantile Treatment Effects by Sex

| | Quantiles | | | | | | |
|---|---|---|---|---|---|---|---|
| | 50 | 60 | 70 | 80 | 90 | 95 | Fraction with Zero Earnings |
| **Panel A:** Descriptive Statistics, Controls | | | | | | | |
| Male | $0.00 | $0.00 | $253.57 | $3207.53 | $11,301.13 | $19,269.51 | 0.67 |
| Females | $50.07 | $1277.54 | $3841.67 | $8236.44 | $15,409.34 | $21,599.07 | 0.49 |
| **Panel B:** Quantile Treatment Effects | | | | | | | |
| Males | – | – | $0.00 | $856.296** | $1,314.96 | $542.76 | |
| | | | [13.367] | [408.933] | [1,743.996] | [1,312.683] | |
| Females | $171.43 | $1,033.82** | $1,877.97** | $2,461.81** | $1,724.63** | 2,415.52** | |
| | [105.731] | [104.998] | [223.522] | [386.654] | [607.650] | [787.502] | |

Notes: This table presents descriptive statistics and quantile regression results using adult annual earnings data for displaced and non-displaced children (age 7 to 18 at baseline) from public housing projects. 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 A4: Subgroup Analysis: Impact on Adult Labor-Market Outcomes of Children

(a) Dependent Variable: Labor Participation (=1)

| | (1) | (2) | (3) |
|---|---|---|---|
| Subgroup | Fraction of All Children | Employment Control Mean | Employment Difference: Treated–Control, Within Est. |
| **Baseline Age** | | | |
| <13 | 0.59 | 0.374 | 0.038** |
| | | | [0.017] |
| 13-18 | 0.41 | 0.436 | 0.041** |
| | | | [0.018] |
| **Household Employment** | | | |
| > 0 Working Adults | 0.18 | 0.454 | 0.03 |
| | | | [0.032] |
| No Working Adults | 0.82 | 0.403 | 0.042** |
| | | | [0.014] |
| **Household Past Arrests** | | | |
| > 0 Adults with Arrest(s) | 0.31 | 0.39 | 0.021 |
| | | | [0.028] |
| No Adults with Arrest(s) | 0.69 | 0.418 | 0.050** |
| | | | [0.012] |

(b) Dependent Variable: Annual Earnings ($)

| | (1) | (2) | (3) |
|---|---|---|---|
| Subgroup | Fraction of All Children | Earnings Control Mean | Earnings Difference: Treated–Control, Within Est. |
| **Baseline Age** | | | |
| <13 | 0.59 | $2424.83 | $583.34** |
| | | | [200.505] |
| 13-18 | 0.41 | $4106.29 | $588.36** |
| | | | [247.348] |
| **Household Employment** | | | |
| > 0 Working Adults | 0.18 | $3,983.29 | $-77.61 |
| | | | [408.349] |
| No Working Adults | 0.82 | $3,305.27 | $723.79** |
| | | | [185.151] |
| **Household Past Arrests** | | | |
| > 0 Adults with Arrest(s) | 0.31 | $2,998.69 | $386.71 |
| | | | [354.330] |
| No Adults with Arrest(s) | 0.69 | $3,571.25 | $713.292** |
| | | | [167.586] |

Notes: This table presents results from labor market analysis of subgroups of children defined on baseline (the year before demolition) characteristics. Panels (a) and (b) present subgroup results where the dependent variable in the regression is an indicator for annual employment and earnings, respectively. The control mean statistic in Column (2) refers to the averages for non-displaced individuals. Each specification includes indicators for treatment group interacted with subgroup membership indicators as well as project fixed effects. 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 A5: Adjusted $p$-values for Main Demolition Analysis of Adult Outcomes of Children

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | | | $p$-values | |
| Outcome | Difference: Treat-Control, Within Estimate | Standard Error | Pairwise | FDR-Adjusted |
| Employed (=1) | 0.040 | [0.135] | 0.0044 | 0.0040 |
| Earnings | $602.27 | [153.91] | 0.0003 | 0.0003 |
| Any Assistance (=1) | 0.128 | [0.022] | 0.5633 | 0.5633 |
| Total Arrests | -0.022 | [0.024] | 0.1628 | 0.3648 |

Notes: The results in Columns (3) and (4) are per-comparison (pairwise) and false discovery rate (FDR) adjusted $p$-values for four main outcomes considered in the analysis of children (age 7 to 18 at baseline) forced to relocate due to building demolition. The FDR-adjusted $p$-values control for the number of false positives when multiple hypotheses are tested. These adjusted $p$-values are calculated using the two-step procedure from Benjamini et al. (2006). Columns (1) and (2) repeat the results from Tables 3, 5 and 7 for convenience.

Table A6: Comparing the Short-run Impact of Demolition and Housing Voucher Offers on Neighborhood Characteristics

(a) Demolition Sample

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Control Mean | Difference: Treated-Control (Reduced Form) | | |
| Percent Black | 94.897 | -2.563** | | |
| | | [1.125] | | |
| Percent Below Poverty Line | 64.208 | -12.929** | | |
| | | [2.531] | | |
| Percent on Public Assistance | 57.153 | -18.365** | | |
| | | [2.164] | | |
| Percent Unemployed | 39.337 | -12.422** | | |
| | | [1.497] | | |
| Violent Crime per 10,000 Residents | 68.855 | -23.426** | | |
| | | [4.371] | | |
| Property Crime per 10,000 Residents | 103.247 | -15.72 | | |
| | | [10.122] | | |
| N (Households with Address) | | 2,162 | | |

(b) CHAC 1997 Lottery Sample

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Control Mean | Difference: Treated-Control (Reduced Form) | LATE (2SLS) | Control Complier Mean |
| Percent Black | 84.25 | 2.47 | 7.45 | 79.84 |
| | | [1.841] | [5.351] | |
| Percent Below Poverty Line | 45.59 | -3.236** | -9.771** | 48.39 |
| | | [1.550] | [4.428] | |
| Percent on Public Assistance | 38.04 | -2.719** | -8.210** | 40.45 |
| | | [1.293] | [3.689] | |
| Percent Unemployed | 27.61 | -1.790** | -5.423** | 29.52 |
| | | [0.894] | [2.568] | |
| Violent Crime per 10,000 Residents | 32.13 | -0.47 | -1.44 | 31.74 |
| | | [1.075] | [3.127] | |
| Property Crime per 10,000 Residents | 69.41 | -1.78 | -5.43 | 72.40 |
| | | [1.853] | [5.441] | |
| N (Households with Address) | | 1,363 | 1,363 | |

Notes: The unit of analysis in this table is a household and there is one observation per household. The dependent variables in each row of the table are neighborhood characteristics measured three years after baseline. This implies that households are only included in the regression if they have valid address (neighborhood) data three years after baseline. Recall that address data is only available if one member of a household actively receives social assistance. Panels (a) and (b) present results for the demolition and CHAC 1997 housing voucher lottery samples, respectively. The control mean statistic – Column (1) – refers to averages for individuals whose household is not displaced by demolition or does not win a voucher offer. The reduced form effect is calculated by regressing each outcome (row) on an indicator for living in a building marked for demolition in Panel (a) or winning a voucher offer in Panel (b). The local average treatment effect (LATE) is estimated using the two-stage system where the dependent variable in the first stage is an indicator for whether a household used a housing voucher and the instrument is an indicator for winning a CHAC 1997 housing voucher.

Table A7: Comparison of Demolition and CHAC 1997 Lottery Households

| | (1) Lottery Sample | (2) Demolition Sample | (3) $p$-value Difference: Lottery– Demolition | (4) Weighted Demolition Sample | (5) $p$-value Difference: Lottery – Weighted Demolition |
|---|---|---|---|---|---|
| (a) Adults in Households with Children (Age 7-18) | | | | | |
| # Adults | 1.44 | 1.15 | 0.00 | 1.38 | 0.01 |
| Single Female Head (=1) | 0.69 | 0.69 | 0.00 | 0.70 | 0.27 |
| Age | 31.68 | 32.16 | 0.01 | 31.87 | 0.28 |
| Earnings | $ 5,595.30 | $ 1,747.06 | 0.00 | $ 5,248.41 | 0.15 |
| Employed (=1) | 0.36 | 0.16 | 0.00 | 0.36 | 0.77 |
| Past Arrests, Any | 0.66 | 0.64 | 0.01 | 0.68 | 0.47 |
| Past Arrests, Violent | 0.19 | 0.16 | 0.77 | 0.19 | 0.95 |
| Past Arrests, Property | 0.16 | 0.15 | 0.21 | 0.17 | 0.72 |
| Past Arrests, Drugs | 0.14 | 0.15 | 0.02 | 0.14 | 0.55 |
| Past Arrests, Other | 0.20 | 0.18 | 0.37 | 0.20 | 0.84 |
| (b) Children (Age 7-18) | | | | | |
| # Kids | 2.44 | 2.10 | 0.00 | 2.43 | 0.86 |
| Past Arrests, Any | 0.02 | 0.04 | 0.11 | 0.03 | 0.26 |
| Past Arrests, Violent | 0.01 | 0.01 | 0.61 | 0.01 | 0.44 |
| Past Arrests, Property | 0.01 | 0.01 | 0.39 | 0.01 | 0.40 |
| Past Arrests, Drugs | 0.01 | 0.02 | 0.80 | 0.01 | 0.67 |
| Past Arrests, Other | 0.01 | 0.01 | 0.75 | 0.01 | 0.62 |
| N (Households) | 2,242 | 2,767 | | 2,767 | |

Notes: This table compares summary statistics for the demolition and lottery samples and the unit of analysis is at the household-level. Panel (a) presents statistics for adults in households with children (age 7 to 18 at baseline) while Panel (b) presents statistics for children (age 7 to 18 at baseline). Baseline in this context refers to the year before demolition or the year before randomization in the CHAC 1997 housing voucher lottery. Column (4) presents statistics for the demolition sample after re-weighting. The weighting procedure balances sample characteristics between the demolition and lottery samples which is evident from the $p$-values in Column (5). See Section 9.4 for further details on the weighting procedure.

Table A8: Sensitivity of Main Demolition Analysis to Sample Definition

(a) Sample: All Children Ages 5 to 18 at Baseline

| | Panel Model Results | |
|---|---|---|
| | (1) | (2) |
| | Control Mean | Difference: Treated-Control, Within Estimate |
| Employed (=1) | 0.415 | 0.037*** |
| | | [0.013] |
| Employed Full Time (=1) | 0.096 | 0.012** |
| | | [0.006] |
| Earnings | $3,628.97 | $549.582*** |
| | | [149.769] |
| Earnings (> 0) | $8,737.85 | $559.260** |
| | | [217.636] |
| N (Obs.) | | 36,601 |
| N (Individuals) | | 6,130 |

(b) Sample: All Children Ages 6 to 18 at Baseline

| | Panel Model Results | |
|---|---|---|
| | (1) | (2) |
| | Control Mean | Difference: Treated-Control, Within Estimate |
| Employed (=1) | 0.417 | 0.037*** |
| | | [0.014] |
| Employed Full Time (=1) | 0.097 | 0.013** |
| | | [0.006] |
| Earnings | $3,659.23 | $565.376*** |
| | | [152.780] |
| Earnings (> 0) | $8,777.10 | $579.157** |
| | | [219.569] |
| N (Obs.) | | 36,223 |
| N (Individuals) | | 5,752 |

Notes: This table analyzes adult labor market outcomes for displaced and non-displaced children using different definitions for the sample. Panel (a) uses children age 5 to 18 at baseline while Panel (b) uses children age 6 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 are 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 $^{***}$ $p < 0.01$. The analysis omits some observations (less than one percent) that are outliers in the distribution of earnings.

Table A9: Impact on Long-Run Criminal Arrests of Children

| | Panel Model Results | |
|---|---|---|
| | (1) | (2) |
| | Control Mean | Difference: Treated-Control, Within Estimate |
| Total Arrests | 0.362 | -0.035 |
| | | [0.024] |
| Violent Arrests | 0.072 | -0.010** |
| | | [0.004] |
| Property Arrests | 0.034 | 0.006* |
| | | [0.003] |
| Drug Arrests | 0.103 | -0.005 |
| | | [0.011] |
| Other Arrests | 0.154 | -0.025** |
| | | [0.011] |
| N (Obs.) | | 56,629 |
| N (Individuals) | | 5,250 |

Notes: This table analyzes criminal arrests for displaced and non-displaced children. The results here differ from Table 7 because the sample includes all observations where the individual is age 13 or older. Note that the panel for each individual is restricted to the years after demolition. 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 $t$. The independent variables in the regression are 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. Note that statistical significance is denoted by: $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$ and $^{***}$ $p < 0.01$.