Essays in Social Economics

by

Eric Theodore Chyn

A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Economics)
in the University of Michigan
2016

Doctoral Committee:

Associate Professor Martha J. Bailey, Chair
Professor John E. DiNardo
Professor Brian A. Jacob
Professor Jeffrey A. Smith

For Deanna and my parents.

ACKNOWLEDGEMENTS

I am indebted to many people who provided advice and support necessary to complete this dissertation. In particular, I learned a tremendous amount about conducting careful empirical work from my dissertation chair Martha Bailey. Moreover, her unfailing enthusiasm motivated me to overcome research obstacles and improve the quality of my work. Brian Jacob provided important lessons on how to focus on details in my research. I am also extremely grateful for Brian's generous help in accessing the data used in the first and second chapters of my dissertation. My committee members John DiNardo and Jeff Smith provided invaluable guidance on the research strategies and methodologies used in this dissertation. Justin Wolfers provided a fresh perspective and further encouragement of my research. Finally, I owe special thanks to Justine Hastings who has been a mentor for several years.

Many other economists provided important feedback for my dissertation. I want to particularly thank Achyuta Adhvaryu, Manuela Angelucci, Lint Barrage, Charles Brown, John Bound, Michael Eriksen, Mel Stephens, Susan Dynarski, Jens Ludwig and Michael Mueller-Smith. I am grateful for helpful conversations about my research with Lasse Brune, Sarah Johnston, Max Kapustin, Jason Kerwin, Rishi Sharma, Isaac Sorkin, Bryan Stuart and Chris Sullivan. The first chapter of my dissertation also benefited from comments and suggestions I received from seminar participants at the University of Michigan, Vanderbilt University, the Sixth Workshop of the Centre for Research Active Labour Market Policy Effects, the University of Notre Dame, the W.E. Upjohn Institute, the University of Oregon, the University of Virginia, the University of Texas at Austin and the University of California at Riverside. The third chapter of my dissertation also benefited from insightful comments from Emily Breza, Dean Karlan, Supreet Kaur, Dan Keniston, Tavneet Suri, Chris Udry and seminar participants at the University of Michigan, the University of Minnesota and Yale University. Ndema Longwe also provided outstanding research support in field work for my third chapter.

The work in this dissertation was supported by generous financial support. During my doctoral training, I received fellowship from a training grant provided by National Institute for Child and Human Development (NICHD) to the Population Studies Center at the University of Michigan (T32 HD0077339). In addition, I am grateful for the use of services and facilities at the Population Studies Center, which is funded by an NICHD Center Grant (R24 HD041028). Data collection for the third chapter of my dissertation was supported from grants from the Michigan Institute for Teaching and Research in Economics (MITRE), the Population Studies Center, the Center for Education of Women and the Rackham Graduate School at the University of Michigan.

Finally, I am thankful for my family's help in completing my doctoral studies. I would not have been able to complete this dissertation without the love and support from my wife Deanna. Importantly, she has consistently encouraged me to pursue research that I believe matters. In addition, Deanna provided outstanding editing and proofreading assistance that improved the writing in this dissertation. My parents have provided unwavering moral support and inspiration. My time in graduate school was also happier due to the good company provided by Lasse Brune, Michael Gelman, Alan Griffith, Enda Hargaden, Sarah Johnston, Jason Kerwin, David McCune, Doug Piper, Daniel Schaffa, Rishi Sharma and many others.

TABLE OF CONTENTS

LIST OF FIGURES

Figure

LIST OF TABLES

Table

ABSTRACT

Essays in Social Economics

by

Eric Theodore Chyn

Chair: Martha J. Bailey

The central idea in social economics is that an individual's actions are influenced by the choices and characteristics of peers. This occurs because group (network) membership provides information and shapes beliefs (norms). For both researchers and policymakers, the interest in social economics stems from its ability to explain a range of phenomena including the persistence of urban poverty and the spatial distribution of crime.

This dissertation contributes to the literature by providing new empirical evidence on the effects of social interactions. Specifically, the first two chapters examine the impact of neighborhood peers on children. The third chapter (joint with Lasse Brune and Jason Kerwin) studies how coworkers affect workplace productivity. In what follows, I provide a detailed description of each chapter of my dissertation.

Chapter 1: Moved to Opportunity: The Long-Run Effect of Public Housing Demolition on Labor Market Outcomes of Children

This chapter 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.

Chapter 2: The Impact of Disadvantaged Peers: Evidence from Resettlement after Public Housing Demolition

During the mid-1990s, Chicago began to demolish its public housing stock, forcing thousands of disadvantaged households to relocate across the city. There has been widespread concern about the potential impact on residents living in areas where former public housing households relocated. Using novel administrative data from Illinois, this chapter estimates how the presence of these displaced households affected children living in resettlement neighborhoods. My empirical strategy compares criminal behavior of children who lived on city blocks that received displaced public housing residents with that of children that lived on similar, nearby blocks with no displaced households. I find evidence that children who grew up near former public housing residents have more arrests for property crimes. Additional analysis provides suggestive evidence that the effects are largest in areas where displaced young males relocated.

Chapter 3: Peer Effects in the Workplace: Experimental Evidence from Malawi (with Lasse Brune and Jason Kerwin)

This chapter sheds light on the nature of workplace peer effects by analyzing an experiment with a tea estate in Malawi. We randomly allocate tea-harvesting workers to locations on fields to estimate the impact of peers on worker performance. Using data on daily productivity, we find strong evidence of positive effects from working near higher-ability peers. Our estimates show that increasing the average of co-worker ability by 10 percent increases own-productivity by about 0.5 percent. We find nonlinearities in the magnitude of peer effects across the distribution of own-ability: peer effects are the largest for the lowest ability workers. Since workers receive piece-rates and there is no teamwork, peer effects in our setting are not driven by production or compensation externalities. In additional analysis, we find evidence against learning or worker socialization as mechanisms. Results from an incentivized choice experiment suggest instead that peer effects in this context are driven by co-workers as a source of "motivation." When given a choice to be re-assigned, the majority of workers want to be assigned near a fast (high-ability) coworker, even if switching is assigned an explicit cost. In open-ended survey responses, workers with demand for high-ability peers state that working near faster peers provides motivation to work harder.

CHAPTER I

Moved to Opportunity: The Long-Run Effect of Public Housing Demolition on Labor Market Outcomes of Children

1.1 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, Hendren and Katz, 2015). 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.[1] For example, Durlauf (2004) writes: "[O]ne cannot extrapolate the [MTO] findings to the broader population of the poor."

[1] The MTO researchers have been careful in discussing the issue of external validity. Ludwig, Duncan and Hirschfield (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."

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 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.[2]

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.[3]

The estimates in this paper contrast with previous findings in the neighborhood effects literature.[4] 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.[5] Moreover,

[2] 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.

[3] 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.

[4] 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.

[5] 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, Hendren and Katz (2015) differs from this previous work by focusing on younger (below age 13) children who were not old

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. 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.[6] 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, Kane and Staiger, 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.

1.2 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

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.

[6] 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.

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 offered to them.[7] 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.[8] 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.[9] 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).[10] 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, 1999*a*). Similarly, harsh winter weather damaged several buildings in the Henry Horner Homes project, prompting the CHA to close these buildings for demolition (Garza, 1999*b*). When not reacting to specific crises, the CHA generally sought to close buildings that had the most severe maintenance issues.[11]

[7] 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.

[8] For a detailed history of public housing in Chicago, see Popkin et al. (2000).

[9] 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.

[10] 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).

[11] 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,

When the CHA selected a building for demolition, the authority provided Section 8 housing vouchers to displaced residents which allowed recipients to rent housing on the private market.[12] 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.[13]

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^{th} 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.[14]

1.3 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.[15] 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 neighborhoods, existing empirical studies provide mixed support for this prediction. For ex-

1992). I exclude the projects and the buildings where these crime-based closures occurred from the analysis in this paper.

[12] 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).

[13] 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).

[14] For the interested reader, a full list of program rules for housing vouchers is contained in Appendix Section A2.

[15] 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.

ample, 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).[16] 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, Hendren and Katz (2015) 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.[17] 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, Öckert and Oosterbeek, 2013; Fryer and Katz, 2013; Chetty, Friedman and Rockoff, 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.[18]

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

[16] 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, Rosenbaum and Meaden (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.

[17] 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.

[18] As will be discussed in Section 1.7, 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.

child development irrespective of changes in exposure to neighborhood poverty.

1.4 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.[19] 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.

1.4.1 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.[20] I also restrict attention to high-rise buildings (defined as having 75 units or more).[21] Finally, I exclude projects where documented evidence suggests that building demolition was correlated with unobserved tenant characteristics. Specifically, I exclude the Cabrini Green and Henry Horner projects.[22]

[19] I also use the police data for supplementary analysis of adolescent criminal behavior.

[20] 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.

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

[22] 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.

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.[23] 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.

1.4.2 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.[24]

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.[25] The final 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.

[23] Appendix Section A3 provides further details on the process for determining the year of demolition.

[24] 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).

[25] 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.

1.5 Empirical Approach

1.5.1 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.[26] 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,

$$Y_{it} = \alpha + \beta D_{b(i)} + X_i'\theta + \psi_{p(i)} + \delta_t + \epsilon_{it}, \tag{1.1}$$

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 δ_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, β represents the net impact of demolition on children's outcomes.

In addition, I also estimate two augmented versions of Equation 1.1. First, I estimate a model which includes interactions for gender.[27] 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, Ludwig and Katz, 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

[26] 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.

[27] 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, β_b is the average difference in outcomes between males in the treatment and control group while β_g is the average difference for females.

non-displaced peers at each age of measurement observed in my sample.

Estimates of β in Equation 1.1 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.

1.5.2 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 1.1 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 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.[28] In addition, Table 1.1 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

[28] 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.

on schooling outcomes.[29] Columns (3) through (6) similarly show no detectable difference between displaced and non-displaced youth by gender subgroup.[30]

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 1.1 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.

1.5.3 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) 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 A.1, 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.

[29] 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.

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

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 1.1 with additional indicators for living in a stable building that is adjacent to a demolition building,

$$Y_{it} = \alpha + \beta' D_{b(i)} + \pi N_{b(i)} + X_i'\theta + \psi_{p(i)} + \delta_t + \epsilon_{it}, \tag{1.2}$$

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.[31] 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.[32] Formally, I use the following specification: [33]

$$H_{it} = \mu_i + \sum_{j=-4}^{-1} \pi_j \; 1(t - t^* = j) + \sum_{j=1}^{10} \tau_j \; 1(t - t^* = j) + \delta_t + \epsilon_{st} \tag{1.3}$$

where the dependent variable H_{it} is homicide rate for Census tract i at year t. The terms μ_i and δ_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.

[31] 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.

[32] 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.

[33] See Jacobson, LaLonde and Sullivan (1993) for further discussion of event-study specifications.

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 δ_t are identified using data from locations that have not yet or already have had a demolition. The Figure A1 plots the coefficients π_j and τ_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.

1.6 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 on any measure of public assistance utilization.

1.6.1 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.[34] In subsequent Section 1.6.3, I find that there is no detectable difference in the

[34] If demolition reduces the likelihood that a household uses social assistance, they will not have an active

probability that displaced children are on social assistance in the years following demolition.

Table 1.2 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 from the Chicago Public Schools) to examine changes in children's location.[35]

Figure 1.1 presents densities of neighborhood (tract) poverty rates to help characterize the cumulative impact of demolition on neighborhood quality.[36] 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, Ludwig and Katz, 2005; Sanbonmatsu et al., 2011).

The difference in neighborhood quality also shrinks over time. Column (4) of Table 1.2 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.[37] To further explore the impact on neighborhood quality over time, Figure 1.2 plots the difference in the (tract) neighborhood poverty rate in each post de-

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.

[35] 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.

[36] 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.

[37] 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.

molition 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.

1.6.2 Adult Labor Market Outcomes of Children

Table 1.3 examines the impact of demolition on children's adult labor market outcomes by presenting results from Equation 1.1. 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.[38] While I do not directly measure hours worked, Table 1.3 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 1.4 presents estimates from a modified version of Equation 1.1 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.[39]

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.[40] Figure 1.3 plots each point estimate for the estimated labor market treatment effect

[38] 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.

[39] 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).

[40] 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.

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 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 1.4 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, Hendren and Katz (2015) which reveals that treatment effects for MTO children younger than age 13 are positive starting in their mid-twenties. However, Chetty, Hendren and Katz (2015) 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.[41]

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 1.4. 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.[42]

[41] For more on this comparison with recent re-analysis of MTO, see Figure 1 and Table 3 of Chetty, Hendren and Katz (2015).

[42] At the same time, it is worth noting that I cannot reject the equality of treatment effects across these different subgroups.

Finally, Figure 1.5 and Appendix Table A3 further explore heterogeneity in the impact of demolition by estimating quantile treatment effects. Note that the lower bound of the x-axis on the figure is restricted to the 60$^{\text{th}}$ percentile because a large fraction of earnings are equal to zero.[43] 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$^{\text{th}}$ 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.

1.6.3 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, Luttmer and Mullainathan (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 1.5 presents results from Equation 1.1 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).[44] Even at the 90th percentile of the earnings dis-

[43] 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.

[44] Social service eligibility depends on both income and family size. The maximum income allowance described in 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.

tribution, treated individuals still qualify for both foodstamps and Medicaid assistance.[45] 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.

1.7 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 1.3, 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 1.6 explores whether there is any impact of relocation due to demolition on labor market outcomes of parents.[46] 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).[47] 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 1.7 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 1.1 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 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

[45] For descriptive statistics on the quantiles of the earnings distribution, see Appendix Table A3.

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

[47] The LATE estimates (not reported) are similarly small in magnitude (and imprecise).

Table A9 shows that there is a positive impact (reduction in arrests) on long-run crime. This analysis examine 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.[48]

Finally, as discussed in Section 1.3, 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.[49] 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.

1.8 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, Kapustin and Ludwig, 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, Krieger and Yekutieli (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 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 1.3, 1.5 and 1.7. 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

[48] Interestingly, Kling, Ludwig and Katz (2005) show that there was an increase in property crime for boys whose household moved to a low-poverty neighborhood through the MTO program.

[49] Jacob (2004) has data on student outcomes up to five years following demolition and relocation.

considered four different outcomes.

1.9 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.

1.9.1 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, Ludwig and Katz (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 1.6 and 1.7 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.[50] 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.

1.9.2 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 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, Tobias and Vytlacil (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,

$$V_i = \mathbf{1}(\gamma_0 + \gamma_1 O_i > \epsilon_i), \tag{1.4}$$

where ϵ_i is a random error that represents the unobserved cost of voucher use for child i's family. The coefficient γ_1 represents the impact of receiving a voucher offer on the decision to use a voucher.

As shown in (Angrist, Imbens and Rubin, 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

[50] 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.

effect (LATE) of vouchers,[51]

$$\frac{\mathbb{E}(Y_i|O_i=1)-\mathbb{E}(Y_i|O_i=0)}{\mathbb{E}(V_i|O_i=1)-\mathbb{E}(V_i|O_i=0)}=\mathbb{E}(\Delta_i|\gamma_0+\gamma_1>\epsilon_i>\gamma_0). \tag{1.5}$$

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.[52] 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 1.5 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 experiment. For simplicity, I assume the distribution of (Δ_i, ϵ_i) is bivariate normal: $(\Delta_i, \epsilon_i) \sim N_2(\mu_\Delta, \mu_\epsilon, \sigma^2_{\Delta_i}, \sigma^2_{\epsilon_i}, \rho)$. Here, μ_Δ and μ_ϵ are the mean benefits to children and mean costs of using a voucher, respectively. The correlation between child gains and costs is ρ while the respective variances for benefits and costs are $\sigma^2_{\Delta_i}$ and $\sigma^2_{\epsilon_i}$, respectively.

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

$$\begin{aligned}\mathbb{E}(Y_{1i}-Y_{0i}|\gamma_0+\gamma_1>\epsilon_i>\gamma_0) &= \mathbb{E}(\Delta_i|\gamma_0+\gamma_1>\epsilon_i>\gamma_0)\\ &= \underbrace{\mu_\Delta}_{\text{ATE}} + \rho\sigma_\Delta\left(\frac{\phi(\gamma_0)-\phi(\gamma_0+\gamma_1)}{\Phi(\gamma_0+\gamma_1)-\Phi(\gamma_0)}\right)\end{aligned} \tag{1.6}$$

The right side of Equation 1.6 shows that the LATE will differ from the average treatment effect (ATE) as a function of the parameters of Equation 1.4, which models whether a family uses a voucher to move and the correlation between gains and costs (ρ). 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 1.8 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

[51] 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.

[52] 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, Ludwig and Katz, 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.

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 (ϵ_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 (i.e., $\mathbb{P}(V_i = 1|O_i = 1)$ was low). Goering et al. (1999) report that only 25 percent of eligible families at the five MTO sites volunteered.[53] 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.

1.9.3 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.[54] 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, Kapustin and Ludwig (2015), I analyze the CHAC 1997 lottery using the following definitions for treatment and control groups.[55] There were 82,607 income-eligible applicants and the CHAC randomly

[53] In keeping with the spirit of the model shown in Equation 1.4, 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).

[54] 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).

[55] For a detailed description of the data including summary statistics, see Appendix Section A4.

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.[56]

Panel (a) of Table 1.8 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.[57] The first row shows that 63 percent of the treated group uses a housing voucher, while a notable share (31 percent) of the control group also obtains a housing voucher from some program other than the CHAC 1997 lottery.[58] 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 1.8 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 zeroes, 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 1.6 and 1.7).

[56] 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 constrains the CHAC was unable to meet this initial plan. For more detailed background on the CHAC lottery, see Jacob and Ludwig (2012).

[57] This analysis of lottery children from public housing is distinctly different from the analysis in Jacob, Kapustin and Ludwig (2015) 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 because the housing contribution rules for vouchers and project-based housing are similar.

[58] 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.

1.9.4 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.[59] 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-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.[60] 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 1.8 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 1.8, 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

[59] 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, Tobias and Vytlacil, 2001).

[60] 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.

capital or other margins may 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, Liebman and Katz (2001) report that moving through MTO actually reduced parents' child monitoring intensity, presumably because treated parents felt safer in their new neighborhood.

To provide further intuition for why reverse Roy selection could occur in the context of a housing mobility program, Appendix A5 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, Kane and Staiger (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.

1.10 Cost-Benefit Analysis

This section uses the results from Section 1.6 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, Ellen and Ludwig, 2015).[61]

To understand the earnings benefit of relocation, I use the reduced form estimate from Table 1.3. Recall from Section 1.5 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 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, Hendren and Katz (2015), 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.[62] 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.[63] 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.[64]

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

[61] 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, Ellen and Ludwig, 2015).

[62] 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.

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

[64] The fixed moving payment is determined by the number of furnished bedrooms and varies by state.

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 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.[65]

1.11 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, Hendren and Katz, 2015).

One of the main contributions of this paper is to provide a new explanation to reconcile findings in the neighborhood effects literature. On the one hand, a number of observational

[65] 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.

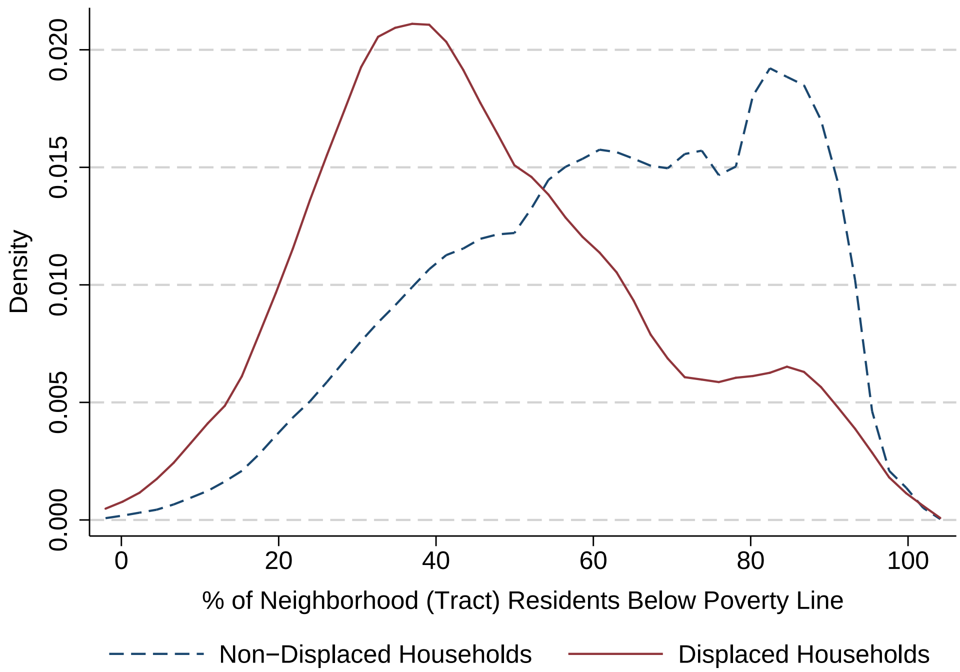
studies find a substantial positive correlation between child outcomes and measures of neighborhood quality (Galster et al., 2007; Ellen and Turner, 1997). This pattern inspired a large theoretical literature to support a causal interpretation of these estimates (e.g., Massey and Denton (1993); Wilson (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.

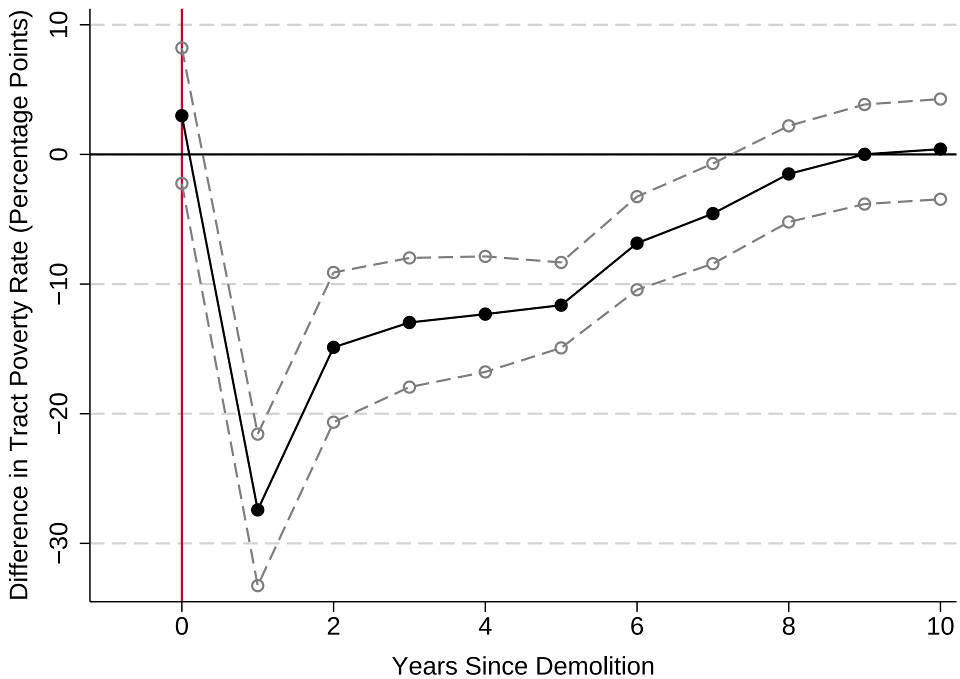
1.12 Figures and Tables

Figure 1.1: 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 1.2: Difference in Neighborhood Poverty For Displaced and Non-displaced Households by Post-Demolition Year



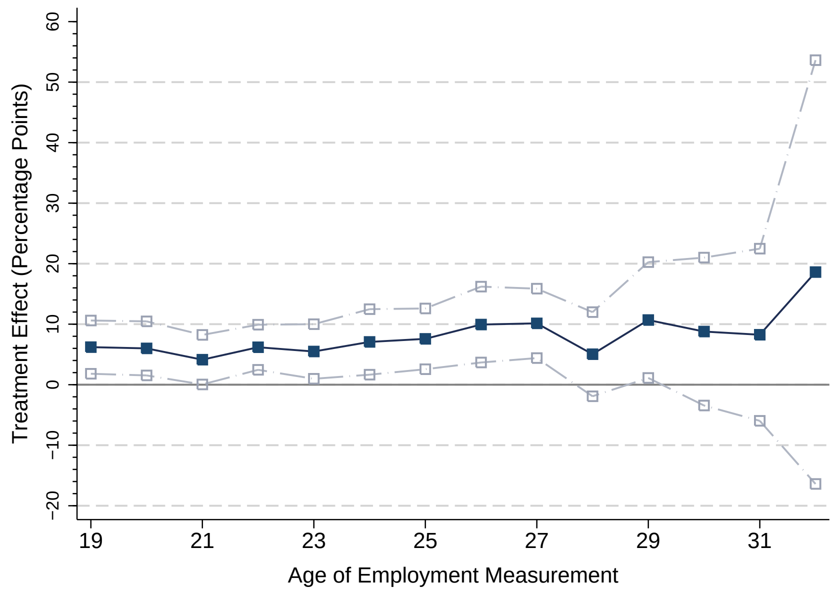
Notes: This figure illustrates the change over time in the difference in neighborhood poverty rate between displaced (treated) and non-displaced (control) households with children (age 7 to 18 at baseline). Specifically, I plot (in solid black) the set of coefficients π_y for $y \in \{0, \cdots, 10\}$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

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

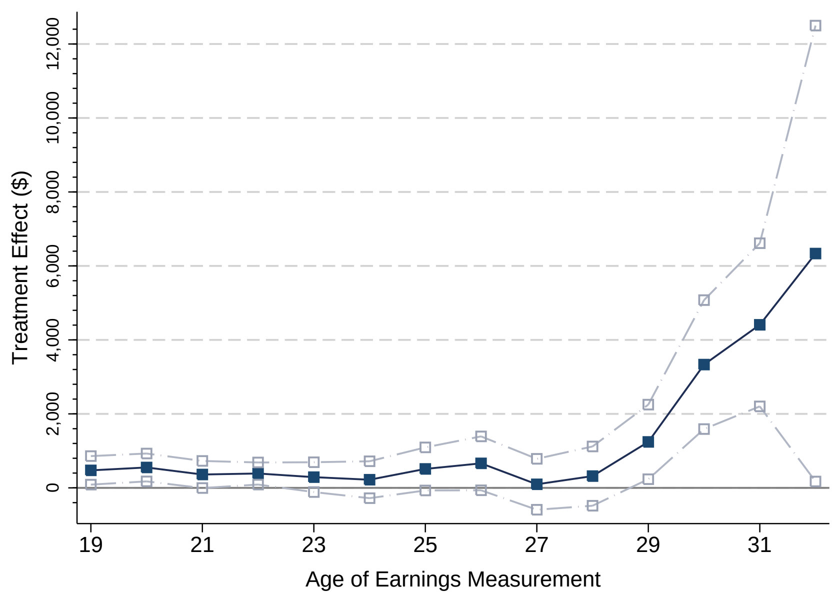
where h indexes a household; t represents years; and p indexes projects. The dependent variable is the percentage of residents living below the poverty line in a Census tract and ψ_p is a set of project fixed effects. The variable t^* represents the year of demolition for a particular household. Recall 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 π_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 1.3: Labor-Market Treatment Effects for All Children by Age of Measurement

(a) Dependent Variable: Employed (=1)



(b) Dependent Variable: Annual Earnings ($)



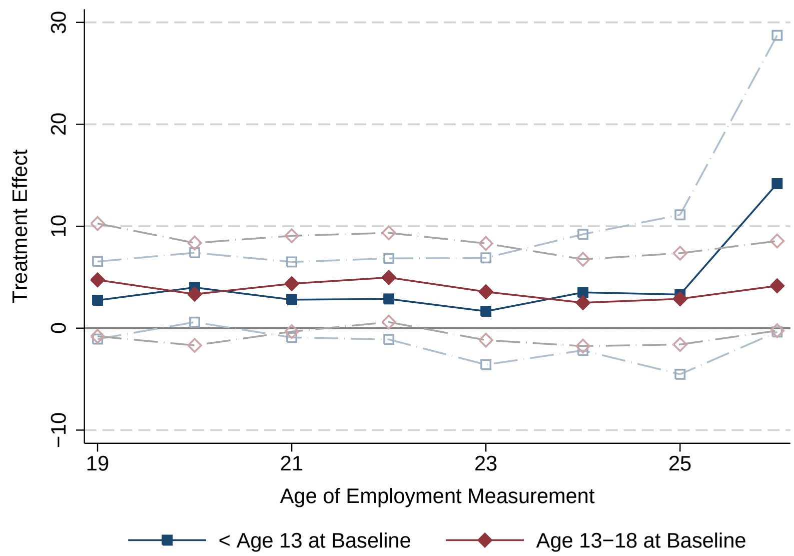
Notes: The figures in Panels (a) and (b) display estimated treatment effects at adult ages for children (age 7 to 18 at the time of demolition). Specifically, each point (red circles) is a coefficient $\alpha_j \ \forall\, j\ \in \{19, \ldots, 32\}$ from the following specification:

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

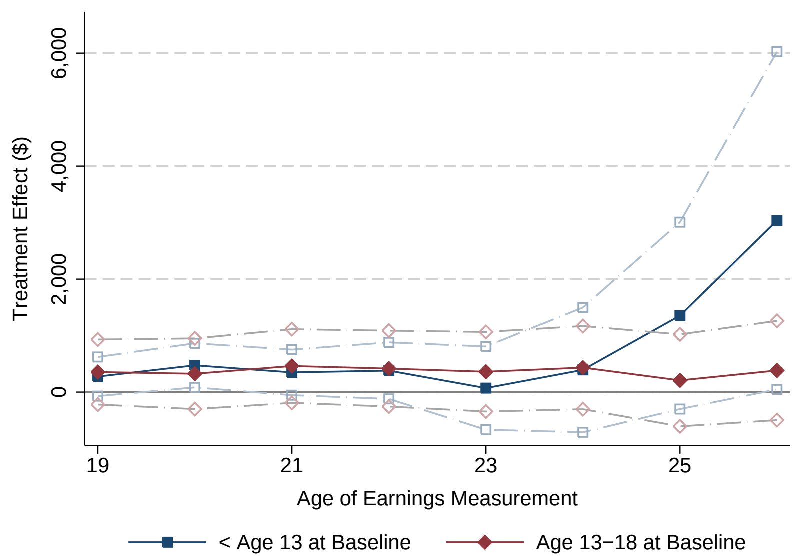
where i is an individual; t indexes years; and p is a project. The terms δ_t and ψ_p are year and project fixed effects, respectively. The dummy variable D takes a value of one if an individual was displaced by demolition. The main effects for the indicator terms for individual age are included in the 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 1.4: Younger vs Older Children: Labor-Market Treatment Effects by Age of Measurement

(a) Dependent Variable: Employed (=1)



(b) Dependent Variable: Annual Earnings ($)

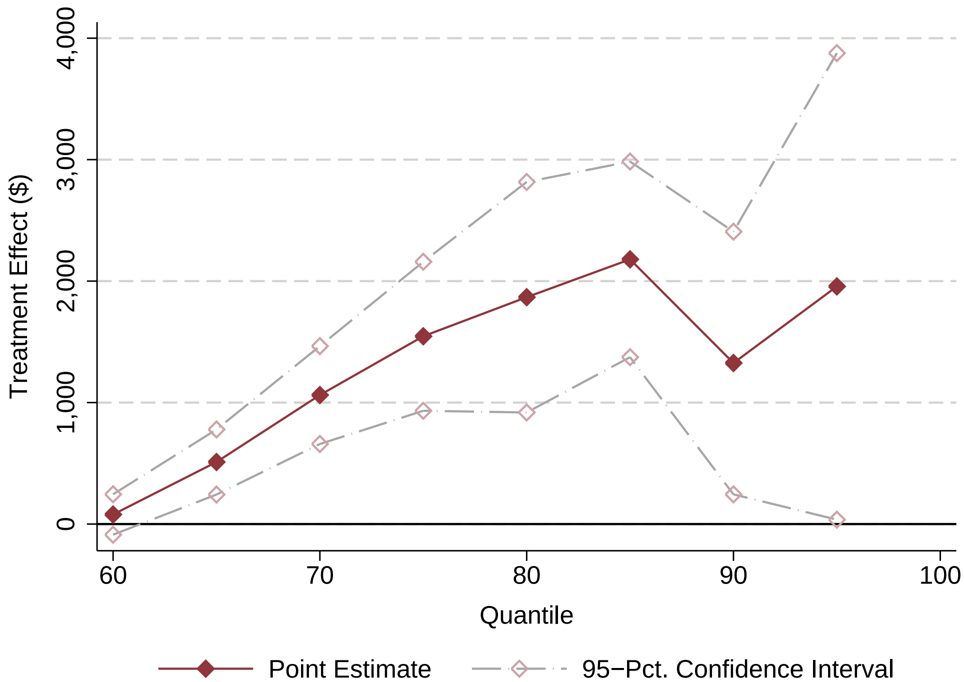


Notes: The figures in Panels (a) and (b) display estimated treatment effects at adult ages for children affected by demolition. Each figure presents two sets of results: estimates for younger (age < 13 at baseline) and older (age 13 to 18 at baseline) children as solid red diamonds and solid blue squares,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

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

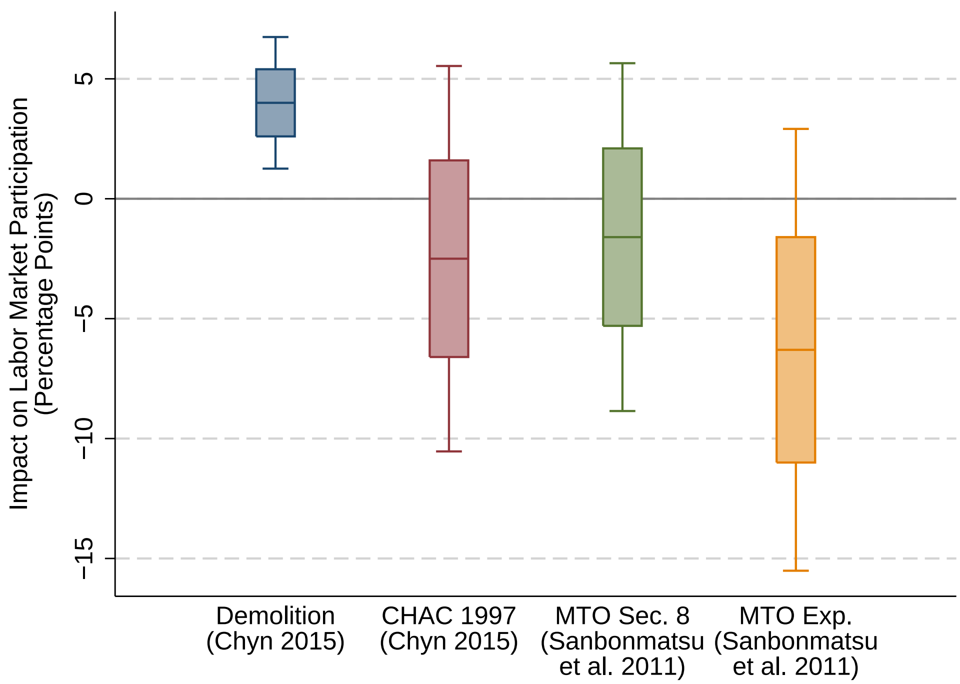
where i is an individual; t indexes years; and p is a project. The terms δ_t and ψ_p are year and project fixed effects, respectively. The dummy variable D takes a value of one if an individual was displaced by demolition. The main effects for the indicator terms for individual age are included in the 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 1.5: Quantile Treatment Effects for Adult Earnings of Children



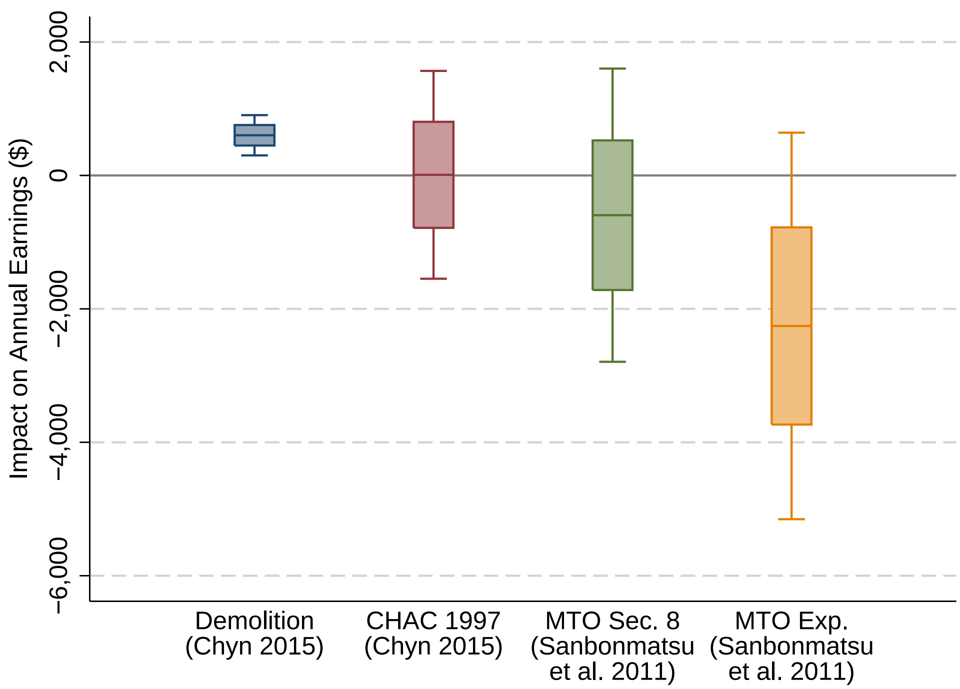
Notes: This figure plots estimates of the quantile treatment effect on adult earnings outcomes for children (age 7 to 18 at baseline) affected by public housing demolition. These estimates measure the treatment effect for particular percentiles of the distribution of earnings. In other words, the quantile treatment effect estimate for the 60^{th} percentile measures the difference between the 60^{th} 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^{th} percentile because a large fraction of earnings are equal to zero.

Figure 1.6: 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.

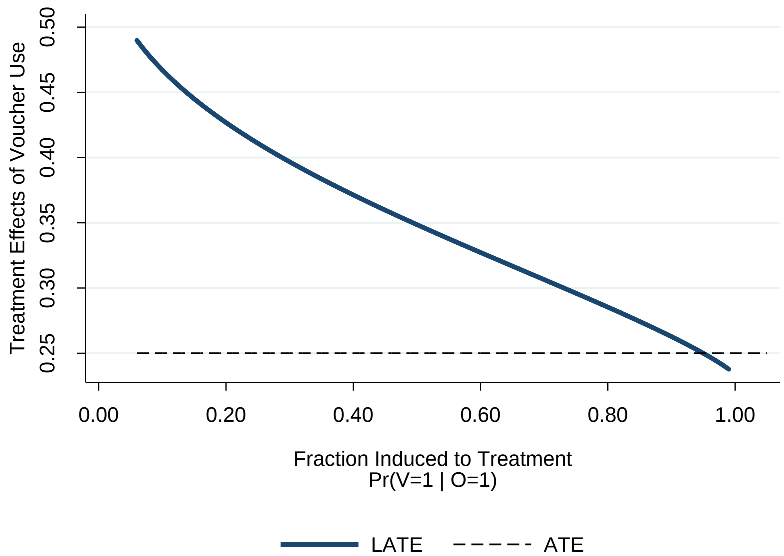
Figure 1.7: Effects on Adult Earnings of Children Across Studies



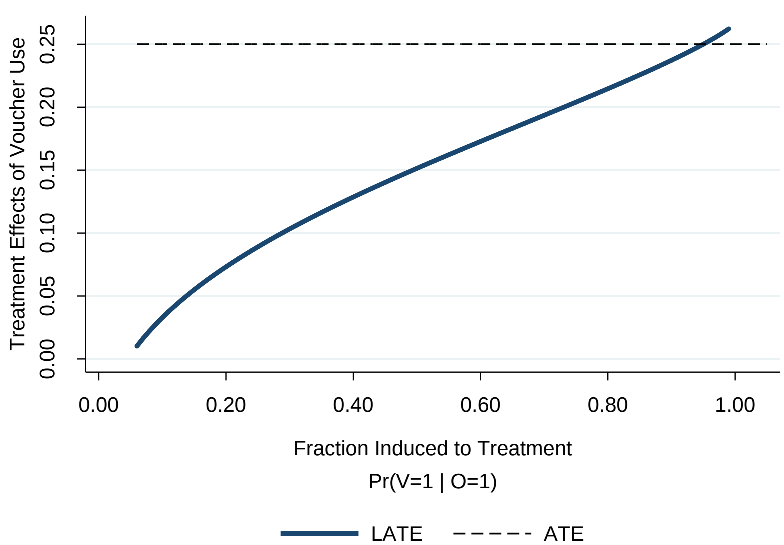
Notes: This figure displays box and whisker plots of the effects on adult earnings outcomes for children (age 7 to 18 at baseline) from different studies. The center of each box is the point estimate in each sample while the top and bottom of each box represent effects that are one standard error above 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 1.8: The Local Average Treatment Effect When the Fraction Induced to Treatment Varies

(a) Positive Selection on Gains (Roy Style)



(b) Negative Selection on Gains (Reverse Roy)



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 1.9 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 1.1: 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
Demographics								
Age	11.714	0.035	11.548	0.145	11.873	-0.070	28.851	0.810**
		[0.159]		[0.196]		[0.186]		[0.312]
Male (=1)	0.489	-0.008					0.128	-0.001
		[0.017]						[0.011]
Past Arrests (#)								
Violent	0.015	0.005	0.028	0.011	0.004	-0.003	0.185	-0.017
		[0.007]		[0.014]		[0.009]		[0.032]
Property	0.011	0.010	0.018	0.015	0.004	0.004	0.156	0.016
		[0.009]		[0.014]		[0.010]		[0.020]
Drugs	0.025	0.000	0.054	0.017	0.000	-0.018	0.166	0.031
		[0.013]		[0.023]		[0.012]		[0.022]
Other	0.022	0.004	0.035	0.015	0.011	-0.008	0.230	-0.018
		[0.008]		[0.014]		[0.008]		[0.028]
School Outcomes†								
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]						
Economic Activity††								
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 1.2: 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	0.656	0.007
		[0.020]		[0.020]
Restricted to Households with Address				
Public Housing Address (=1)	0.549	-0.440**	0.105	-0.058*
		[0.042]		[0.033]
Distance from Baseline Address (miles)	1.993	1.409**	4.914	-0.447
		[0.458]		[0.419]
Tract Characteristics:				
Black (%)	94.897	-2.563**	90.042	-0.82
		[1.125]		[1.230]
Below Poverty (%)	64.208	-12.929**	40.858	-1.571
		[2.531]		[1.884]
On Welfare (%)	57.153	-18.365**	34.821	-1.392
		[2.164]		[1.673]
Unemployed (%)	39.337	-12.422**	24.852	-2.363**
		[1.497]		[1.156]
Violent Crime (per 10,000)	68.855	-23.426**	30.801	3.236
		[4.371]		[2.547]
Property Crime (per 10,000)	103.247	-15.72	68.675	5.902
		[10.122]		[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 1.3: 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 1.1 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 1.4: 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	0.505	0.066***
		[0.019]		[0.014]
Employed Full Time (=1)	0.080	0.013	0.117	0.015*
		[0.008]		[0.008]
Earnings	$2,946.51	$417.46*	$4,416.94	$806.22***
		[236.705]		[188.520]
Earnings (>0)	$9,55.43	$552.21	$8,739.53	$609.26**
		[439.299]		[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 1.5: 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	0.502	0.020	0.746	0.012
		[0.022]		[0.028]		[0.025]
Foodstamps	0.509	-0.001	0.349	0.006	0.656	0.000
		[0.021]		[0.023]		[0.026]
Medicaid	0.477	0.005	0.307	0.015	0.633	0.002
		[0.021]		[0.025]		[0.025]
TANF	0.123	-0.001	0.022	0.005	0.216	-0.002
		[0.010]		[0.008]		[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 1.1 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 1.6: 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 1.1 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 1.7: 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)
Adolescent Age Criminal Arrests				
Total	0.369	-0.022	21,097	4,917
		[0.024]		
Violent	0.086	-0.005	21,097	4,917
		[0.007]		
Property	0.048	0.008*	21,097	4,917
		[0.004]		
Drugs	0.106	-0.012	21,097	4,917
		[0.011]		
Other	0.129	-0.013	21,097	4,917
		[0.011]		

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 1.8: CHAC 1997 Voucher Lottery and Re-Weighted Demolition Analysis

(a) Voucher Use and Mobility Analysis

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

(c) Re-weighted Demolition Sample: Reduced Form Estimates

	(1) Control Mean	(2) 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 *** $p < 0.01$.

CHAPTER II

The Impact of Disadvantaged Peers: Evidence from Resettlement After Public Housing Demolition

2.1 Introduction

Over the past 30 years, policymakers have sought to reduce concentrated poverty by providing more than $6 billion to redevelop and demolish nearly 155,000 units of public housing across the U.S. (GAO, 2007). As part of this process, city housing authorities have provided housing vouchers to displaced public housing residents, many of whom have used these vouchers to move to higher-income neighborhoods (GAO, 2003). This inflow of new residents – many of whom, especially in large cities, relocate from housing projects which have serious problems with violent crime and gang activity – has caused concern about the possible impact on individuals already living in the areas that received displaced households.

Two key issues have hampered research on the effects of resettlement on residents living in areas where households displaced by demolition relocated. First, displaced public housing residents typically have some degree of choice over where they relocate. This complicates analysis because differences in outcomes for individuals in resettlement and non-resettlements areas may be caused by unobserved characteristics correlated with relocation choices. Second, the few existing studies on resettlement rely on aggregate-level data, which prevents researchers from analyzing any interactions (spillovers) between displaced residents and their new neighbors. Effects on neighborhood-level outcomes could arise from the actions of displaced residents rather than a change in behavior of existing residents.

In this paper, I test whether the arrival of displaced residents affects criminal activity of children already living in resettlement neighborhoods by studying public housing demolition in Chicago. During the mid-1990s, thousands of former public housing residents resettled throughout the city after the housing authority began demolition of high-rise public housing. To estimate the effects, I use an approach pioneered by Bayer, Ross and Topa (2008) and compare outcomes of children living on the same block as displaced residents with children who live on nearby blocks.[1] By comparing households living in close proximity, this strategy attempts to minimize differences in pre-existing characteristics that may affect youth crime.

Novel administrative data from Illinois allow me to both track displaced households and create a sample of children living in resettlement areas. Specifically, I use address information in social assistance records to identify 7,363 public housing residents displaced during the initial wave of Chicago's public housing demolitions (1993-1998). I then identify 811 resettlement areas, defined as any Census block on which at least one displaced resident lived one year after demolition. Next, I construct a treatment group of 25,668 children (18 years old or younger) receiving government assistance who lived on a resettlement block. Finally, I again use the social assistance data to construct the comparison group of 37,726 children in low-income households located near a resettlement block.

Using arrest data linked to the social assistance records, my preferred estimates show that children who lived on the same block as a displaced household are 7 percent more likely to be arrested for property crimes in the years after resettlement. Notably, the analysis also shows small and non-significant effects on the number of an individual's violent or drug crime arrests. Supplementary analysis provides suggestive evidence that the effects on property crime are larger for children living near 13- to 24-year-old males displaced from public housing.

While this finding of increased property crime is consistent with previous studies of neighborhood and peer effects, the estimates in this paper help shed light on mechanisms. Specifically, several studies exploit experimental or quasi-experimental variation in the assignment of individuals to neighborhoods or schools to provide evidence of causal effects on criminal activity or risky behavior (Case and Katz, 1991; Kling, Ludwig and Katz, 2005; Ludwig and Kling, 2007; Carrell and Hoekstra, 2010; Deming, 2011; Imberman, Kugler and Sacerdote, 2012; Damm and Dustmann, 2014; Billings, Deming and Ross, 2016).[2] By design, many of these interventions have limited ability to distinguish between the distinct channels driving the effects. For instance, an individual's criminal behavior may depend on his classmates' characteristics. These "endogenous" or "exogenous" effects are the standard mechanisms of social interaction Manski (1993). Alternatively, individuals in the same neighborhood or classroom may have similar outcomes because they share the same institutional influences such as police or teacher quality. In Manski's (1993) terminology, these

[1] Specifically, Bayer, Ross and Topa (2008) provide evidence on the effects of job referrals by examining whether individuals residing in the same city block are more likely to work together than those who live on nearby blocks.

[2] Another related literature shows that exposure to different peers within juvenile detention centers affects recidivism (Bayer, Hjalmarsson and Pozen, 2009; Stevenson, 2015).

are "correlated effects" and do not represent the impact of social interactions. Relative to studies that examine the impact of a child changing neighborhoods and schools, estimating the impact on individuals in resettlement areas speaks more forcefully to the role of social interactions since the broader institutional environment may not change after a relatively small number of displaced persons move into the community.[3]

In addition, this study also contributes to an active debate over the externalities associated with housing assistance.[4] Specifically, the effects on long-run property crime in this paper complement previous neighborhood-level analysis of Chicago by Aliprantis and Hartley (2015) which showed that resettlement of public housing households caused an increase in crime rates for burglary, trespassing and gang activity in resettlement neighborhoods.[5] The estimates in the current paper suggest that part of this increase in crime may be due to peer effects for the youth living in resettlement areas. Interestingly, the results in both this paper and Aliprantis and Hartley (2015) contrast somewhat with popular media accounts which, at the time of demolition and resettlement, suggested that public housing demolition led to increases in homicides (Rosin, 2008; Bovard, 2011).[6]

Overall, the increase in the number of property crimes among children who lived in resettlement areas should be weighed against the benefits of relocating public housing households from severely disadvantaged areas. The estimates in this paper suggest that there are 1.9 additional arrests for property crime after resettlement for every 1,000 children living on blocks where displaced residents relocated.[7] Offsetting this cost, both Chetty, Hendren and Katz (2016) and Chyn (2015) find that relocation of children from public housing results in large gains in adult labor market outcomes. Moreover, Chyn (2015) also finds that displaced children experience a large and significant reduction in violent crimes arrests as adults, which have far higher social costs than property crime.

[3] Moffitt (2004) discusses the difficulty in separating environmental and social interaction effects within the context of neighborhood relocation programs such as Moving to Opportunity (MTO). Damm and Dustmann (2014) also address this point in their study of random assignment of refugees in Denmark. Their approach to isolating social interaction effects relies on including neighborhood fixed effects and measures of time-varying neighborhood characteristics to control for institutional and environmental factors.

[4] Several papers cover a wide range of effects of housing assistance programs on crime. For example, Carr and Koppa (2016) find that individuals who live in private market housing have increased rates of criminal arrest for violent crime after receiving a Section 8 housing voucher. Freedman and Owens (2011) find that the U.S. Low-Income Housing Tax Credit (LIHTC) program reduces violent crime in poor neighborhoods.

[5] Using different data and methodology than Aliprantis and Hartley (2015), Popkin et al. (2012) study the impact of public housing demolition and relocation on neighborhood-level crime measures, and find evidence that crime increased in areas that received displaced public housing residents.

[6] Aliprantis and Hartley (2015) find no statistically significant effect of displacement on neighborhood-level homicides; however, they do observe increases in arrests for assault and battery.

[7] As detailed in Section 2.6, a back-of-the-envelope calculation using estimates from Lochner and Moretti (2004) suggests the social cost of these additional crimes is about $370,000.

2.2 Background

At the start of the 1990s, Chicago had 17 developments (also known as "projects") providing more than 40,000 units of apartment 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. During this period, nearly all public housing residents were African-American, and the majority of households were single-parent, female-headed households (Popkin et al., 2000).

Beginning in the mid-1990s, the Chicago Housing Authority (CHA) began demolishing public housing in Chicago as a response to both serious issues in the city's housing management system and major changes in national housing policy. After decades of mismanagement, Popkin (2000) writes that the city's public housing was "...among the worst in the nation – poorly constructed, poorly maintained and extremely dangerous." Yet, these conditions in Chicago were mirrored elsewhere in the country: after visiting and investigating 21 U.S. cities, a Congressional committee identified 86,000 "severely distressed" units of public housing that were physically deteriorating buildings and located in communities plagued by high-levels of drug trafficking and violent crime (National Commission, 1992). Reacting to these findings, Congress created the HOPE VI Urban Revitalization Demonstration (hereafter, HOPE VI) program in October 1992 to fund rehabilitation or building demolition (GAO, 1997).

Bolstered by funding from the HOPE VI, the CHA closed and eventually demolished 54 high-rise buildings between 1993 and 1998 (Jacob, 2004). For displaced residents, the CHA offered Section 8 housing vouchers that subsidized rent in private market housing.[8,9] Alternatively, displaced households could request a transfer to another public housing unit, pending availability. Notably, for households seeking to use a voucher, the CHA did not provide relocation counseling during this initial wave of demolition.

Although official statistics on relocation outcomes from the mid-1990s are not available, existing studies suggest that a large fraction of Chicago's displaced public housing families relocated to private market housing. Jacob (2004) identifies 3,500 displaced children in administrative school records and finds that about 60 percent were living in private market housing three years after demolition. Similarly, Aliprantis and Hartley (2015) use credit

[8] Prior to the CHA demolitions, public housing households could *not* access housing vouchers because the CHA stopped taking requests in 1985 (Jacob, Kapustin and Ludwig, 2014).

[9] Typically, the voucher subsidy is equal to the difference between the gross rent and the family's required rent contribution, which is 30 percent of adjusted income. An exception to this is when the gross rent exceeds the local Fair Market Rent (FMR). During the 1990s, the FMR was equal to the 40th percentile of the local private market rent distribution. Note that the CHA allowed families to keep the voucher as long as they remained eligible for assistance.

report data to track about 700 displaced households and find that, in the third year after demolition, 55 percent lived in Census blocks that did not contain any public housing.

2.2.1 Expected Effects of Displaced Households on New Neighbors

Presumably, social interactions are the primary mechanism by which former public housing households could affect the behavior of their new neighbors. In a simple linear model, the context of the present paper suggests that social interaction effects would be negative, especially for criminal behavior which is the focus of the present paper. This prediction stems from the fact that residents of Chicago's public housing developments lived in extremely disadvantaged neighborhoods as is evident from the neighborhood statistics in Table 1. Specifically, Column (2) shows that residents of demolished public housing lived in high-poverty areas where the rate of violent crime was about three times higher than the city's average.

Although research testing specifically for social interactions in neighborhood settings is relatively limited, the empirical evidence thus far confirms this as a mechanism for criminal activity.[10] For example, Damm and Dustmann (2014) find that immigrant children in Denmark grow up to commit more crime if they are (quasi-randomly) assigned to neighborhoods with high levels of youth crime. To support the interpretation of these effects as social interactions, they show that their results are robust to the inclusion of neighborhood fixed effects and a number of controls for other time-varying neighborhood measures. In other relevant work studying children in North Carolina, Billings, Deming and Ross (2016) find that youth who live in the same neighborhood and attend the same school are much more likely to be arrested for committing the same crime together.

2.3 Data

I use multiple administrative sources to test whether households displaced by demolition increased long-run criminal behavior among children living in their new neighborhoods. Specifically, I first create a sample of public housing residents displaced by public housing demolition by merging building records from the CHA with social assistance (i.e., TANF/AFDC, Food Stamp and Medicaid) case files (1989-2009) from the Illinois Department of Human Services (IDHS). Using the panel of addresses in the assistance case files, I

[10] As discussed in the introduction, a number of prominent studies examine criminal behavior in natural experiments which randomly assign individuals to neighborhoods or schools. By design, these papers are usually unable to separate the effects of social interactions from broader features of the neighborhood or schooling environment.

identify the Census blocks to which at least one displaced resident moved.[11,12] Next, I create a sample of low-income non-displaced children (age 18 or younger) living on the same block as displaced households by merging the list of resettlement blocks with the social assistance case files. To estimate the effect of resettlement, I construct a comparison group from children in the social assistance files who live on a non-resettlement block that is within one-quarter of a mile of a resettlement block. To analyze children's outcomes, I link the samples of resettlement and comparison group children to arrest records from the Illinois State Police. The police data are linked to the social assistance case files using identifiers created by Chapin Hall at the University of Chicago, a research institute and leader in administrative-data linking. In what follows, I describe my sample and the data construction in more detail.

2.3.1 Sample of Demolished Public Housing Buildings

The analysis in this paper studies the resettlement of residents displaced by public housing closures and demolitions that occurred in from 1993 to 1998.[13] Specifically, the identity and date of demolition for these buildings is obtained from Jacob (2004) who determined the year of closure by examining CHA administrative data on building occupancy supplemented by qualitative sources. Note that focusing on this particular period (1993 to 1998) allows me to examine criminal activity for children over a number of years after resettlement occurs. In total, there are 54 buildings that were closed during this initial wave of selective building demolition.

To identify areas where resettlement occurred, I create a sample of households living in public housing selected for demolition and examine their addresses after relocating. Creating this sample requires linking the list of demolished buildings with social assistance case files using exact street address information contained in both files. With this linked data, the sample I study is the set of welfare recipients who have a street address that matches the building sample in the year prior to demolition. Under this definition, the assistance data contains 7,363 adults and children on who were displaced by public housing demolition and can be tracked for at least one post-demolition year. These households relocated to 811 blocks immediately after demolition. Consistent with previous research on public housing demolition in Chicago, Table 2.1 shows that the resettlement blocks identified in the IDHS data are low-income areas which have notably high rates of violent and property crime (Popkin et al., 2002; Jacob, 2004; Popkin et al., 2004; Aliprantis and Hartley, 2015).

[11] Note that Census blocks correspond roughly to actual city blocks.

[12] This data construction implies that I can track displaced residents only if they remain on forms of social assistance after building demolition occurs.

[13] Following Jacob (2004), I designate a building with more than 75 units as a high-rise.

2.3.2 Sample of Low-Income Children Living in Resettlement Areas

My analysis studies a sample of low-income children who lived in areas of Chicago where resettlement of displaced households occurred. Specifically, I begin by creating a treatment group based on matching the list of 811 resettlement blocks to a panel of addresses listed in social assistance case files from the IDHS. From the merged panel, I find 25,668 children whose household matched to a resettlement block in the year prior to resettlement.[14] Note that by focusing on the year *before* resettlement, the sample definition is unrelated to any subsequent effects on neighborhood composition. Next, I create a comparison group of children by returning to the IDHS panel of addresses and merging the data with a list of the 2,297 non-resettlements blocks in the city of Chicago that are within one-quarter mile of a resettlement block. As discussed in greater detail in Section 2.4, the outcomes for the set of 37,726 comparison group children will serve as the counterfactual for inferring the effects of living on a resettlement block near displaced public housing residents. I link this sample of non-displaced children to individual-level data from the Illinois State Police that contain all arrests up to the first quarter of 2012.

Figure 2.1 presents hypothetical Census blocks to illustrate the definition of the sample of children living in resettlement areas. Different types of households on social assistance with children are represented by colored dots. There is one resettlement block based on the location of a single displaced household. Children living on this resettlement block (red dots) are the treated group. Children who live on non-resettlement blocks are included in my analysis (green dots) only if they live within one-quarter of a mile of the resettlement block. All non-resettlement area children in the IDHS data that live outside the one-quarter mile radius (black dot) are not included in the analysis.

2.4 Empirical Strategy and Interpretation

The main question in this paper is whether children who grew-up near resettled public housing residents are more likely to engage in criminal behavior later in life. If resettlement of households were random, I would be able to answer this question simply by comparing children who lived on the same block as a displaced household to all other children. However, Column (4) of Table 2.1 shows that the resettlement of former public housing residents appears to be far from random. For example, former public housing residents moved to areas with much lower income and higher rates of crime relative to areas that did not receive any public housing residents. This pattern of neighborhood sorting suggests that children in

[14] To be clear, the sample of children in this analysis are age 18 or younger in the year that resettlement occurred in their area.

resettlement and non-resettlement areas are likely to have different unobserved characteristics that may be correlated with later-life outcomes.

To attempt to minimize differences in unobserved characteristics, I use an approach pioneered by Bayer et al. (2008) and compare outcomes of children who lived on resettlement blocks to those who lived in *nearby* blocks which did not receive any displaced households. Specifically, my main specification models criminal behavior observed over adulthood for individual i living on block b as follows:

$$y_{ib} = \theta D_b + \beta X_i + \mu_r + \epsilon_{ib} \tag{2.1}$$

where the dependent variable y_{ib} is the total number of arrests for different types of offenses observed in my sample. Alternatively, I also conduct analysis where the measure of crime is an indicator for having at least one arrest. The key variable in Equation 2.1 is D_b which is an indicator for whether individual i lived on a block where displaced public housing residents resettled. Importantly, the reference group fixed effect μ_r is included to control for area-specific effects – that is, this is the baseline probability of committing a crime for children who grew up in the nearby, non-resettlement blocks surrounding block b. Note that I define the reference group as individuals residing in non-resettlement blocks that are within one-quarter of a mile of a resettlement block. Finally, the vector $\mathbf{X}_i$ includes controls for race, gender and parental criminal history (measured before resettlement) to control for differences in background characteristics.

In this specification, the key concern for interpreting estimates of the parameter θ is is whether there is a correlation between residency on blocks with displaced households and unobserved determinants of criminal behavior ϵ_{ib} *after* conditioning on reference group fixed effects and observed background characteristics. As discussed by Bayer, Ross and Topa (2008), there is likely to be little difference between households within a narrowly defined reference group if the housing market is relatively thin. Within the context of this paper, the scarcity of housing within each block group would make it relatively difficult for displaced households to sort to specific blocks in the aftermath of public housing demolition.

To shed light on the internal validity of this research design and residential sorting patterns, I examine background characteristics of both adults and children that resided in blocks that received displaced households and the reference group of nearby, non-resettlement blocks.[15] Column (4) and (6) of Table 2.2 provides estimates from a regression where the dependent variable is a measure of demographic characteristics or past criminal history regressed on an indicator for living on a resettlement block and reference group fixed effects.

[15] Note that while adults are not the focus of my main analysis, an examination of their characteristics still provides insight on whether resettlement and non-resettlement areas are comparable.

If the relocation pattern of displaced households was random within the narrowly specified reference group, we would expect that the coefficient on the indicator for living on a resettlement block would be zero. There are two notable findings in these results. First, there are some detectable differences in demographic characteristics, but these effects are substantively small in magnitude. For example, adults in resettlement areas are only about 1 percent more likely to be African American than residents of nearby, non-resettlement areas. Second, the results show that there are never any detectable differences in criminal background between individuals living on resettlement and nearby non-resettlement blocks.[16] This shows there is no evidence that residents of resettlement blocks lived in areas with greater levels of resident criminality. Overall, these results suggest that children in resettlement and non-resettlement areas may have similar unobserved characteristics because there is little evidence of sorting of displaced public housing residents. As foreshadowed in Equation 2.1, the analysis that follows includes background characteristics to control for differences in demographic characteristics.

2.5 Results

Table 2.3 shows the results from estimating versions of Equation 2.1 using data on the sample of children living in resettlement and nearby, non-resettlement neighborhoods. Column (2) and Column (3) present estimates from specifications with and without controls for background characteristics, respectively. Each row provides results for a different type of criminal arrest record.

The estimates in Panel A show that resettlement of public housing households significantly increased property crime arrests: children who lived on blocks with displaced public households have about 7 percent more total arrests for property crime in the years following relocation. There are no detectable effects for violent or drug crime arrests and the point estimates are small relative to the mean rate of arrests in the comparison group shown in Column (1). In Panel B, I also estimate the effect of resettlement where the dependent variable is a binary indicator of ever having a given type of criminal arrest in my sample of post-resettlement years. The results show that relocation also significantly increased the extensive margin of criminal activity for both property (5 percent) and drug crime (3 percent). Note that there is little change in the estimates after adding background controls for

[16] As evident in Table 2.2, there are few baseline (juvenile age) arrests for children in the analysis sample. Prior to 1998, the arrest data for juveniles in the ISP data is limited to serious felonies. This implies that baseline criminal activity of children (not adults) is incomplete. However, it is important to note that for the main analysis of post-resettlement crime that the ISP data is complete after 1998: revisions in the Illinois Juvenile Court Act allowed for the submission of juvenile misdemeanor arrests into the ISP database.

the full specification in Column (3).

Interestingly, this finding of an increase in property crime has a parallel in Chyn (2015) which studies the long-run behavior of displaced children. Comparing displaced and non-displaced public housing children, Chyn (2015) finds that displaced children commit about 17 percent more property crime. This evidence of increased property crime lends credibility to the possibility that displaced children may have been "partners in crime" with their new peers who lived in resettlement areas. Due to data limitations, I am unable to directly test for this phenomenon; however, recent research on criminal partnerships by Billings, Deming and Ross (2016) using detailed data from North Carolina reveals that children who live in close proximity and attend the same school are more likely to be arrested for the exact same crime.

The results in Table 2.4 explore the possibility of heterogeneous effects of resettlement. Given that previous research has detected notable differences in the effects of neighborhoods by sex, Columns (2) and (4) provide effects for males and females, respectively (Kling, Ludwig and Katz, 2005; Damm and Dustmann, 2014). There are two notable results in this analysis. First, the effects for property crime in Table 2.3 appear to be driven by the impact on males. Second, the results pooling males and females obscure significant increase in drug arrests for females. The point estimate represents a 16 percent increase relative to the comparison group mean for females.

To analyze the impact for children of different ages at the time of resettlement, Panel (B) of Table 2.4 shows results for younger (less than 13) and older (ages 13 to 18) in Columns (1) and (2). Examining the point estimates shows that there are only detectable effects on criminal activity for older children. The lack of effects for younger children is perhaps not surprising given that household mobility in this sample is relatively high over a sufficiently long window which would limit the possibility of social interactions between children and displaced households. For example, supplementary analysis (not presented) shows that after 3 years 44 percent of resettlement area children (who are reported on subsequent social assistance case records) have moved.[17]

As a final exercise to shed further light on resettlement effects, I examine whether there is heterogeneity across blocks where displaced young males did and did not resettle. Table 2.5 provides results from estimating a modified specification version of Equation 2.1. The key addition to my basic model is an indicator for living on a (treated) block with displaced

[17] To examine mobility behavior, I use a panel of address records in social assistance case files. By definition, an address is only available for children whose household receives some form of social assistance such as foodstamps, TANF or Medicaid. Three years after resettlement, I find that 52 percent of children in the analysis sample have an address in the social assistance case files. For subsequent post-resettlement years, the number of resettlement sample children in the address records declines notably.

households which has young (age 13 to 25) males. The model also includes: an indicator for living on a (treated) block with displaced households which have young females (and no males), and (2) an indicator for living on a (treated) block with displaced households that do not have any young members. As in previous analyses, the omitted group in this specification is a child who lives on a non-resettlement block that is within one-quarter mile of a resettlement block.

The results in Table 2.5 provide suggestive evidence that the effect of resettlement is larger in areas where displaced households with a young male resettled. At the same time, these results should be assessed cautiously the lack of precision of the estimates. In particular, I am unable to reject the null hypothesis that the effects of resettlement are the same in blocks with young displaced males and without any young displaced persons (p-value = 0.173).

2.6 Conclusion

This paper examines whether displaced public housing residents had adverse effects on criminal behavior of children living in their new neighborhood using novel administrative data from Illinois. Following the initial wave of demolitions that occurred in Chicago during the mid-1990s, displaced households tended to resettle in relatively low-income and high-crime areas. This sorting pattern suggests that children who lived in resettlement areas may have different unobserved characteristics than children who lived elsewhere in the city.

To minimize the differences in unobserved characteristics, I rely on an identification strategy pioneered by Bayer, Ross and Topa (2008) and compare children who lived on city blocks where resettlement occurred to their peers living in nearby, non-resettlement blocks. To support the validity of this empirical design, I show that there are few detectable differences in the characteristics of adults and children prior to the resettlement of former public housing residents. Most importantly, there are no detectable differences in past criminal histories of adults which suggests that children living in resettlement areas did not live in notably worse criminal environments.

Analyzing administrative data on arrests in the years after resettlement suggests that children who lived on resettlement blocks had about 7 percent more property crime arrests. There are also small increases in the likelihood of ever being arrested for property or drug crimes. Importantly, the effects for both the intensive and extensive margin of violent crime are small and never statistically significant.

Supplementary analysis supports the idea that the effects detected in this paper are driven by social interactions between children in resettlement areas and displaced public housing households. Two pieces of evidence support this interpretation. First, the effects of living on a resettlement block are largest for adolescents (age 13 to 18) rather than younger children who are likely to have moved away from the resettlement block by the time they became old enough to become involved with criminal activity. Second, the analysis provides suggestive evidence that the effects on property crime are larger on resettlement blocks with displaced young (age 13 to 25) males.

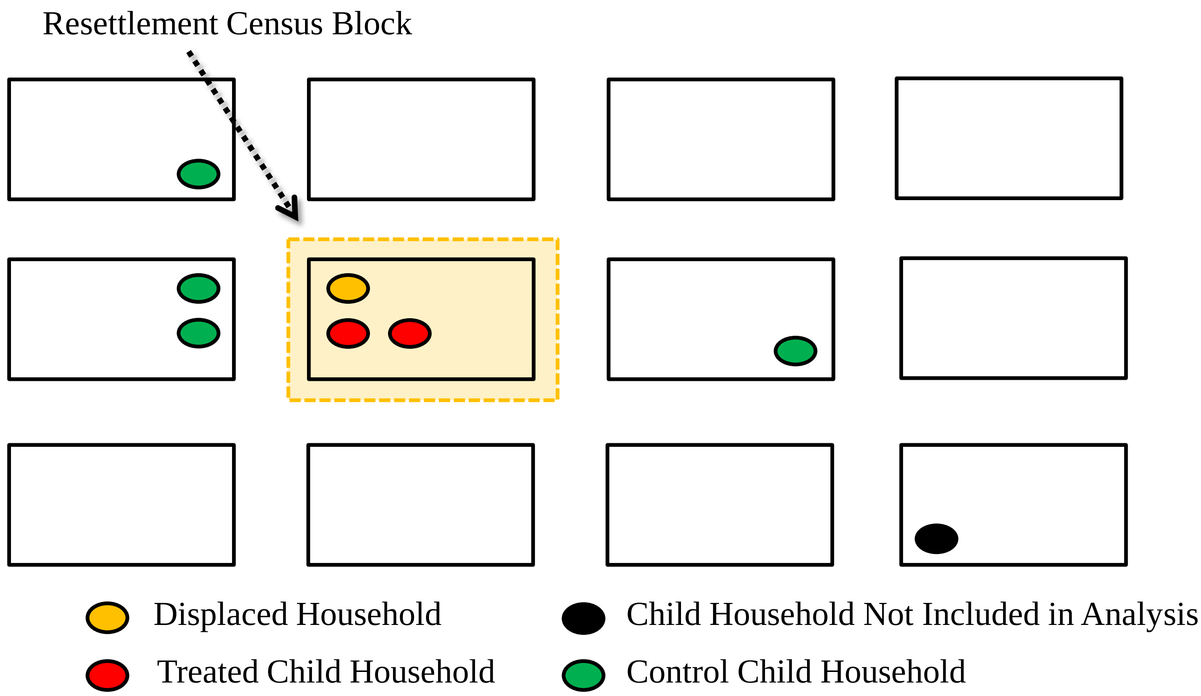
The negative effects of resettlement on criminal behavior of children living in resettlement areas imply that policy-makers face tradeoffs in decisions that affect the distribution of low-income households that receive housing assistance. Previous research by Chetty, Hendren and Katz (2016) and Chyn (2015) suggests that policies that reduce exposure to low-poverty, high-crime environments may positively affect the long-run labor market and criminal outcomes of disadvantaged children. The results in this paper suggest that these benefits come at a price: for every 1,000 children living in resettlement areas, the main estimates suggest there are 1.9 additional arrests for property crime.

Overall, a simple back-of-the-envelope calculation suggests that the social costs of these negative peer effects for resettlement area children are small relative to benefits that accrue to society from relocating displaced households. Based on Lochner and Moretti (2004), the social cost of burglary (the most expensive form of property crime) is $987. Using data from the 2000 Census, there are 200,000 total children living on resettlement blocks, and this implies that the social cost of effects on property crime is about $370,000. Compensating for these negative effects is the finding from Chyn (2015) which shows there are about 100 fewer arrests for violent crime for every 1,000 children that relocates from disadvantaged public housing. Since violent crimes have the highest social cost, the benefits from relocating public housing households appear to more than offset the costs.[18]

[18] Using to estimates from Lochner and Moretti (2004), the social costs per incident of assault (the least costly form of violent crime) is $9,917.

2.7 Figures and Tables

Figure 2.1: Hypothetical Census Blocks and Sample of Children



Notes: This figure shows hypothetical Census blocks to illustrate the definition of the sample used in my analysis. The colored circles represent different types of households on social assistance with children. There is a single displaced public housing household, and the block of their address is the resettlement block. All non-displaced children living on the resettlement block are the treatment group. Only non-displaced children who live on nearby (within one-quarter of a mile) non-resettlement blocks are included as controls in my analysis.

Table 2.1: Comparing Resettlement and Non-Resettlement Areas of Chicago

	(1) All	(2) Demolition	(3) Non-Resettlement	(4) Resettlement
Total Population	1,267.80	770.54	1,331.76	1,122.29
	[860.434]	[915.604]	[827.047]	[915.772]
% Black	0.31	0.84	0.13	0.72
	[0.402]	[0.248]	[0.253]	[0.368]
% Govt. Assistance	0.06	0.24	0.03	0.13
	[0.088]	[0.179]	[0.041]	[0.118]
% Below Poverty	0.14	0.44	0.08	0.28
	[0.152]	[0.287]	[0.083]	[0.181]
Average Family Size	3.38	3.72	3.30	3.57
	[0.553]	[0.618]	[0.505]	[0.612]
Median HH Income	$48,111.09	$27,222.91	$55,185.47	$32,017.55
	[25,094.913]	[28,590.478]	[24,203.411]	[18,897.718]
Violent Crime (per 10,000)	26.55	77.31	11.80	39.81
	[24.041]	[31.629]	[9.758]	[25.314]
Property Crime (per 10,000)	80.60	156.13	59.02	100.00
	[154.036]	[63.445]	[41.202]	[206.790]
N (Census Block Groups)	4,241	46	2,946	1,295

Notes: This table displays averages and standard deviations (in brackets) for neighborhoods in Chicago. All statistics are measured at the block group level and come from the 2000 Decennial Census. Each column contains a different sample of block groups: Column (1) presents statistics for all block groups in Chicago; Column (2) presents statistics for block groups where public housing demolition occurred from 1993-1998; Column (3) shows block groups where no displaced public housing residents resettled; Column (4) presents statistics for block groups where displaced public housing residents resettled in the year after demolition. Block groups are identified as having resettlement based on tracking addresses in social assistance case files. Note that Census statistics are not publicly available at the individual block-level.

Table 2.2: Summary Statistics for Displaced Residents and Individuals in Resettlement Areas

	(1) Displaced (All)	(2) Displaced Males	(3) Non-Resettlement Comparison Group, Adults	(4) Diff., Within Estimate	(5) Non-Resettlement Comparison Group, Children (Age 18 or Less)	(6) Diff., Within Estimate
Demographics						
Age	32.99	33.224	39.165	-0.419**	12.119	-0.025
				[0.205]		[0.033]
Male (=1)	0.208	1.000	0.269	-0.010***	0.486	-0.002
				[0.003]		[0.005]
Black (=1)	0.986	0.985	0.93	0.014***	0.946	0.007***
				[0.002]		[0.003]
Past Arrest History						
# Violent Arrests	0.445	1.302	0.342	0.006	0.005	0.001
				[0.010]		[0.001]
# Property Arrests	0.616	1.5	0.514	-0.024	0.004	0.002
				[0.018]		[0.001]
# Drug Arrests	0.324	0.847	0.244	-0.008	0.005	0.001
				[0.008]		[0.001]
# Other Arrests	0.504	1.252	0.393	0.001	0.009	0.000
				[0.010]		[0.002]
N (Individuals)	6,098	1,265	70,866	119,855	37,726	63,394

Notes: This table presents results from Equation 2.1 where the dependent variable is a demographic of past criminal arrest characteristic measured before resettlement. The specification includes fixed effects for "reference groups". Reference groups are defined as a specific resettlement block and all non-resettlement blocks that are within one-quarter of a mile of this resettlement block. Under this definition, all individuals living in non-resettlement blocks may be members of multiple reference groups. The data is structured such that these children have one observation per reference group. Standard errors are computed via a pairwise bootstrap procedure (Reps=500) and clustered at the reference group level.

Table 2.3: Effect of Resettlement on Crime

	(1) Non-Resettlement Comparison Group, Children (Age 18 or Less)	(2) Diff., Within Estimate, No Controls	(3) Diff., Within Estimate, Controls
Panel A.			
# Violent Arrests	0.545	-0.001	-0.003
		[0.012]	[0.011]
# Property Arrests	0.265	0.019**	0.019**
		[0.008]	[0.008]
# Drug Arrests	0.95	0.013	0.01
		[0.023]	[0.022]
Panel B.			
Ever Violent Arrest (=1)	0.271	-0.002	-0.003
		[0.004]	[0.004]
Ever Property Arrest (=1)	0.162	0.008**	0.008**
		[0.003]	[0.003]
Ever Drug Arrest (=1)	0.253	0.008*	0.007*
		[0.004]	[0.004]
N (Individuals)	38,304	63,282	63,282

Notes: This table presents results from Equation 2.1 where the dependent variable is a measure of total arrests or ever being arrested computed during in my sample of post-resettlement years. Crime measures are based on Illinois State Police data which is comprehensive up to the first quarter of 2012. The specification includes fixed effects for "reference groups". Reference groups are defined as a specific resettlement block and all non-resettlement blocks that are within one-quarter of a mile of this resettlement block. Under this definition, all individuals living in non-resettlement blocks may be members of multiple reference groups. The data is structured such that these children have one observation per reference group. Standard errors are computed via a pairwise bootstrap procedure (Reps=500) and clustered at the reference group level.

Table 2.4: Effect of Resettlement on Crime, by Subgroup

Panel A. Effects by Sex

	(1)	(2)	(3)	(4)
	Males		Females	
	Non-Resettlement Comparison Group, Children (Age 18 or Less)	Diff., Within Estimate, Controls	Non-Resettlement Comparison Group, Children (Age 18 or Less)	Diff., Within Estimate, Controls
# Violent Arrests	0.822	-0.011	0.283	0.011
		[0.018]		[0.010]
# Property Arrests	0.319	0.031**	0.213	0.009
		[0.012]		[0.010]
# Drug Arrests	1.751	0.002	0.191	0.031**
		[0.039]		[0.016]
N (Individuals)	18,713	30,845	19,591	32,437

Panel B. Effects by Baseline Age of Children

	(1)	(2)	(3)	(4)
	Younger Children (Age < 13)		Adolescents (13 - 18)	
	Non-Resettlement Comparison Group, Children (Age 18 or Less)	Diff., Within Estimate, Controls	Non-Resettlement Comparison Group, Children (Age 18 or Less)	Diff., Within Estimate, Controls
# Violent Arrests	0.531	0.006	0.564	-0.01
		[0.016]		[0.014]
# Property Arrests	0.264	0.013	0.266	0.027**
		[0.010]		[0.012]
# Drug Arrests	0.837	-0.021	1.09	0.058*
		[0.027]		[0.032]
N (Individuals)	21,170	34,743	17,134	28,539

Notes: This table presents results from an augmented version of Equation 2.1 where the dependent variable is a measure of total arrests. Panel A provides results which include interactions between an indicator for living on a resettlement block and indicators for the sex of the individual. Panel B provides results which include interactions between an indicator for living on a resettlement block and the age group for the child based on age at the time of resettlement. Crime measures are based on Illinois State Police data which is comprehensive up to the first quarter of 2012. The specification includes fixed effects for "reference groups". Reference groups are defined as a specific resettlement block and all non-resettlement blocks that are within one-quarter of a mile of this resettlement block. Under this definition, all individuals living in non-resettlement blocks (the comparison group) may be members of multiple reference groups. The data is structured such that these children have one observation per reference group. Standard errors are computed via a pairwise bootstrap procedure (Reps=500) and clustered at the reference group level.

Table 2.5: Effects of Resettlement of Different Types of Displaced Households

	(1)	(2)	(3)	(4)	(5)
	Non-Resettlement Comparison Group, Children (Age 18 or Less)	Diff., Lives Near Displaced HH With Young Males, Within Estimate	Diff., Lives Near Displaced HH With Young Females (No Males), Within Estimate	Diff., Lives Near Displaced HH With No Young Persons, Within Estimate	N (Individuals)
# Violent Arrests	0.545	-0.013	0.013	-0.01	63,282
		[0.015]	[0.017]	[0.012]	
# Property Arrests	0.265	0.027	0.021	0.013	63,282
		[0.032]	[0.018]	[0.010]	
# Drug Arrests	0.950	0.008	0.021	0.002	63,282
		[0.039]	[0.042]	[0.036]	

Notes: This table presents results from an augmented version of Equation 2.1 where the dependent variable is a measure of total arrests. Column (2) presents the coefficient for an indicator for living on a (treated) block with displaced households which has young (age 13 to 25) males. Column (3) presents the coefficient from an indicator for living on a (treated) block with displaced households which have young females (and no males) while Column (4) presents the coefficient from an indicator for living on a (treated) block with displaced households that do not have any young members. Crime measures are based on Illinois State Police data which is comprehensive up to the first quarter of 2012. The specification includes fixed effects for "reference groups". Reference groups are defined as a specific resettlement block and all non-resettlement blocks that are within one-quarter of a mile of this resettlement block. Under this definition, all individuals living in non-resettlement blocks (the comparison group) may be members of multiple reference groups. The data is structured such that these children have one observation per reference group. Standard errors are computed via a pairwise bootstrap procedure (Reps=500) and clustered at the reference group level.

CHAPTER III

Peers and Motivation at Work: Evidence from an Experiment in Malawi

From a work with Lasse Brune and Jason Kerwin

3.1 Introduction

Social scientists and policymakers have a long-standing interest in understanding how peers shape an individual's behavior. A key question is whether peers affect productivity in the workplace. The answer to this question has particular importance for determining the optimal allocation of labor and designing firm incentives.

While an emerging literature provides compelling evidence that peer effects exist in workplace settings, the mechanisms behind these peer effects are less clear.[1] Several studies show worker effort is sensitive to social pressure that arises in settings where there are externalities from effort due to joint production and team compensation (Mas and Moretti, 2009; Gould and Winter, 2009; Kaur, Kremer and Mullainathan, 2010; Bandiera, Barankay and Rasul, 2010; Babcock et al., 2015; Cornelissen, Dustmann and Schönberg, 2013).[2] Yet few studies test whether peer effects on productivity may also arise from mechanisms such as motivation or norms – channels not directly controlled by firms.

This paper provides new evidence on the mechanisms that drive workplace peer effects by conducting a unique field experiment with an agricultural firm. We partner with a tea estate in Malawi to randomly allocate about 1,000 piece-rate workers to different locations on tea fields. Each worker is assigned a specific plot area to pick tea leaves each day, and

[1] See Herbst and Mas (2015) for additional discussion and a list of previous studies on peer effects in the workplace.

[2] Another well-studied channel for workplace peer effects is knowledge spillovers (i.e., learning). Notable studies testing for this form of peer effect include the following: Waldinger (2012); Azoulay, Zivin and Wang (2010); Jackson and Bruegmann (2009); Guryan, Kroft and Notowidigdo (2009).

our design creates exogenous, within-worker variation in the composition of plot neighbors. We focus on estimating the effect of the average of peers' ability (permanent productivity) on workers' output.

Importantly, several aspects of this setting allow us to test for the existence of social influences on worker's performance that are unrelated to spillovers in the production process or in the compensation scheme. Unlike much of the previous work examining peer effects, workers in our setting are paid piece rates, and there is no cooperation in the process of collecting tea. Hence, any impact of peers on productivity in our setting is likely to be due to learning or psychological mechanisms related to "motivation" (e.g., self-control or norms).[3]

We find that a worker's daily volume of tea collected is affected by the average ability (i.e. permanent productivity) of his or her coworkers that are located nearby. Increasing the average ability of coworkers by 10 percent raises a tea worker's productivity by about 0.5 percent. In terms of the previous literature, Mas and Moretti (2009) and Falk and Ichino (2006) find effects that are about twice as large in very different settings.

In addition to our main estimates on the effects of mean peer productivity, we also test for non-linear peer effects. We find notable heterogeneity in the effects on productivity, which is consistent with the broader peer effects literature despite the markedly different contexts (Sacerdote, 2001; Falk and Ichino, 2006; Carrell, Fullerton and West, 2009; Mas and Moretti, 2009; Carrell, Sacerdote and West, 2013; Cornelissen, Dustmann and Schönberg, 2013). The least able workers are the most responsive to the average ability of their peers, while there is no detectable impact of peer productivity on higher-ability workers. This pattern of heterogeneity is notable because it suggests that there is potential for aggregate gains for the firm from sorting workers to ensure that the able workers are near higher ability peers.

These estimates differ from Bandiera, Barankay and Rasul (2010) who studied peer effects among U.K. fruit-farm piece-rate employees who are friends. They find evidence that having a higher ability friend nearby increases a workers' productivity. Similarly, workers have lower productivity when they work near lower-ability friends. They argue that workers' desire to socialize with their friends is the driver of this pattern of peer effects between friends.[4] In contrast, we measure peer effects from all co-workers, not just for friends. While we find similar conformism patterns (for all but the highest productivity workers),

[3] In our setting, it is also possible that workers may synchronize their productivity and effort in an attempt to socialize with other workers nearby. We discuss and provide evidence against this hypothesis in greater detail in our analysis.

[4] Park (2015) also provides evidence that manufacturing workers value socializing with their friends despite such interactions resulting in earnings losses. In this case, peer effects due to socializing are associated with compensating differentials: workers forgo monetary compensation to enjoy non-pecuniary benefits of socializing with their friends.

our analysis suggests that socialization is unlikely to be an important mechanism in our setting. Using data on workers' social networks, we find that friends have no detectable impact on productivity while there are positive and significant effects for non-friends.

To learn more about the mechanisms driving the peer effects in our setting, we conduct a choice experiment to measure demand for working next to specific peers. We conduct a survey of a subset of employees in the season following our first experiment and implement an incentivized choice experiment. We find that 71 percent of respondents state they would prefer having a high-productivity co-worker nearby if re-assignment were possible. When asked for the main reason for their choices in an open-ended question 79 percent workers state that the higher-productivity peers provide motivation. Moreover, workers are willing to pay for faster peers. We give workers the option to exchange all or part of the respondent gift that they receive for taking the survey (two bars of soap) in order to be re-assigned next to a high-productivity co-worker. Among those who stated a preference for being reassigned next to a fast plucker, 71 percent are willing to give up one bar and 55 percent are willing to give up two bars of soap - equivalent to 18 percent of average daily earnings. Our choice experiment also allows workers to pay to work next to any other worker of their choice, but almost none of them do so. This indicates that workers place value specifically on having high-ability coworkers, rather than on other traits that are correlated with ability.

Overall, our analysis contributes to the literature by providing evidence that workplace peer effects may be driven by a particular class of psychological mechanisms. As discussed, our setting rules out the possibility that the effects are driven by externalities in the production process or the terms of the financial contract. Supplementary analysis also suggests that learning does not drive the estimates in our study because there no evidence that peer effects vary by experience of workers. Further, the choice experiment that we conduct shows that workers in our sample are willing to pay to work near high-productivity peers, a pattern that is inconsistent with standard models of rank preferences, last-place aversion, shame or reputational concerns (Tincani, 2015; Kuziemko et al., 2014; Kandel and Lazear, 1992; Breza and Chandrasekhar, 2015). Overall, our results are consistent with models of contagious enthusiasm or limited self-control (Mas and Moretti, 2009; Kaur, Kremer and Mullainathan, 2014).

3.2 Background

To conduct our study, we partner with Lujeri Tea Estates, a large agricultural firm in Malawi. Our sample is a group of roughly 1,000 employees who hand-pick ("pluck") leaves from tea bushes (hereafter, we refer to these workers as pluckers). Workers temporarily store plucked leaves in baskets and empty their baskets at a central weighing station. There is no explicit cooperation involved in this process, and tea pluckers are paid a constant piece rate for each kilogram of plucked tea.

Production at the firm is organized by assigning workers to "gangs" which are each managed by a supervisor. The size of a gang is typically around 45 pluckers, but the sizes range from 29 on the low end to 76 on the high end. Each gang is responsible for plucking tea from a pre-determined set of fields over the course of a harvesting "cycle" (7 to 12 calendar days). In our analysis sample, there are 78 fields for the 22 gangs we study.

On each tea field for a gang, the supervisor assigns workers to pluck tea from a specific set of plots (between 1 to 3 per day depending on the characteristics of the plan and day of the week). Each field has between 30 and 120 plots, and workers must pluck on their assigned plots before moving on to other plots.[5,6] At the completion of a harvesting cycle, the gang returns back to the initial field for a new round of plucking – unlike with other crops that are harvested once or a few times, tea bushes grow continuously throughout the season.

Figure 3.1 illustrates the general assignment of workers to plots on a given field and the rotation of workers throughout the harvesting cycle. Panel A shows that each worker is assigned two contingent plots (blue squares).[7] The example highlights three workers who are colored red, green and yellow. The illustration shows that workers B and C are the immediate plot neighbors of workers A. Panel B provides an illustration showing how workers change assignments across fields covered during a 6-working-day harvesting cycle. On each day of the harvesting cycle a given worker has an assigned set of plots for that day's specific field. Across days in the harvesting cycle, a worker will have different neighbors. In the example, the three hypothetical workers are sometimes separated as shown for Cycle Days 3, 5 and 6. On these days, the workers will have different plot neighbors.

3.3 Random Assignment of Workplace Peers

We designed our experimental intervention to randomly assign workers to plots on tea fields in order to generate random variation in exposure to workplace peers. To implement this, we obtained the roster of workers in each gang and a "plucking program" for each gang. The plucking program is a predetermined list of which field (or fields) a gang works during

[5] Pluckers are expected to finish their assigned plots in accordance with the cycle schedule. Pluckers who finish their plots can ask for additional plots that are not assigned to other workers. If a worker is absent on a given day, the supervisor will assign the plot to other workers for the day.

[6] Fixed plot assignment is done so that workers internalize the negative effects of over- and under-plucking bushes on their plots.

[7] Note that in reality the fields and plots are often not sized as evenly-sized rectangles.

each day of its cycle and the number of pluckers that should be assigned to each field. In the simplest case, there is one field on each cycle day with all the pluckers working on it.[8] We use this information to generate randomly ordered lists of pluckers for each day of a gang's harvesting cycle. On cycle days where a gang works on multiple fields, we also randomly determine which workers are on each field.

These randomized lists are used to determine the order in which pluckers are assigned to plots on each field. The random assignment takes advantage of the usual assignment process in which pluckers stand in a queue and are assigned to plots in the order in which they are standing. The supervisor does the assignments by "snaking" back and forth across the field and taking the next plucker from the queue for each plot. Our random assignment scheme alters this system by giving the supervisors a randomly-ordered list to use in this snake pattern.[9] Each gang supervisor was responsible for assigning workers using the randomly generated list of worker assignments in February 2015. We verified compliance with these assignments by having our project managers visit each gang in the week after randomization. As a result of our intervention, workers are randomly assigned to plots within a field for different cycle days as illustrated in Panel B of Figure 3.1.

3.4 Data

To study the impact of workplace peers, we use three main sources of data. First, we rely on administrative data from the firm on worker productivity. Productivity is defined as kilograms of tea plucked per day and is electronically recorded by the firm for the purpose of paying employees. As a result, it is measured with minimal error. This data on worker productivity is available from December 2014 to April 2015 (the beginning and end of the main tea harvest season, respectively). Second, we hired project staff to record information on the plot neighbors assigned to each worker as a result of the randomized assignment that we implemented. Third, we collected survey data to obtain measures of worker characteristics such as background demographics and social networks.

[8] Many gangs have more complicated schedules, spending multiple cycle days on some fields, and splitting the gang across more than one field on certain days of their cycle.

[9] An exception to our randomization is the first work day ("Cycle Day 1") in a gang's cycle. We intentionally did not randomize work assignments in this data. On these days, supervisors assigned workers using the usual method, in which the plots are still assigned using the snaking pattern across the field, but the order of the pluckers comes from the order in which they stand in the queue. We use this non-random assignment on the first work day to test for endogenous assignment and sorting of workers to locations on a field in Section 3.5.

3.4.1 Main Analysis Sample

Our study centers on 1,046 pluckers who worked during the main season after we implemented our randomized work assignments in February 2015. Table 3.1 provides some summary statistics based on the survey and administrative data. The average age for workers is about 37 years and about 43 percent of the sample is female. Only 7 percent of workers are new and average experience is nearly 8 years. Over the course of our study period, the average daily output for each worker is 69 kilograms of plucked tea leaves.

As recorded in our data on work assignments, workers have on average about 5 assigned neighbors on any given day of work. We measure ability as the estimated permanent productivity for each worker in our sample (see Section 3.5 for details). Our study focuses on studying how working alongside peers of different ability affects daily output. To provide a sense of the variation in neighbor ability (measured as the logged average of co-worker ability), Table 3.1 shows that the standard deviation of co-worker ability is about twice the mean.

3.5 Empirical Strategy

The main question in this paper is whether working in close proximity to higher-ability co-workers increases productivity in our sample of tea pluckers. To address this question, we estimate the following linear model of peer effects for the productivity of worker i:

$$y_{ift} = \mu_i + \beta\overline{Ability}_{-ift} + \delta_t + \lambda_f + \epsilon_{ift} \tag{3.1}$$

where y_{ift} is the (logged) total kilograms of tea plucked on field f and date t. The key variable in Equation 3.1 is $\overline{Ability}_{-ift}$ which is the mean of ability of all co-workers who are assigned to work adjacent to the plots that worker i is assigned. The model also includes date and field fixed, δ_t and λ_f, to control for variation in harvest conditions over the course of the season and across the tea estate. Finally, we also control for time-invariant determinants of productivity – such as the worker's own plucking ability – by including individual-level fixed effects μ_i.

To measure ability in our sample of tea pluckers, we rely on an approach pioneered by Mas and Moretti (2009) which uses estimates of worker fixed effects as a measure of ability ("permanent productivity").[10] Specifically, we use the plucking data and estimate:

$$y_{ift} = \mu_i + \mathbf{M}_{ift}\gamma' + \delta_t + \lambda_f + \tau_{ift} \tag{3.2}$$

where the term $\mathbf{M}_{ift}$ is a vector of dummy variables which indicate whether worker j is working next to worker i in the field f on date t.[11] The idea is that the vector γ contains a set of parameters that absorb any possible peer effects and allows us to obtain unbiased estimates of worker fixed effects μ_i, under the assumption that each individual worker can have any effect on his or her coworkers.[12] For Equation 3.1, we use these estimates to define $\overline{Ability}_{-ift} = \overline{\widehat{\mu}}_{ift}$ as our measure of peer influence.[13]

In models of peer effects such as Equation 3.1, the key assumption for identification of β is that there is no correlation between the average ability of one's peers and the unobserved determinants of individual productivity: $cov(\overline{Ability}_{-ift}, \epsilon_{ift}) = 0$. One way this assumption could be violated is if supervisors assign workers with higher ability to work on particularly productive areas of a field that are physically close together. Our intervention eliminates this possibility by randomly assigning workers to plots within a field and makes it possible to purge estimates β of any endogenous sorting effects.

Table 3.2 shows that this random assignment is key for producing causal estimates of peer effects by presenting results from a series of regressions of worker' own ability on the mean of their co-workers ability.[14] Column (1) shows that the there is a positive correlation between own ability and peer ability on the sample of plucking days that correspond to "Cycle Day 1" of each gang's work cycle - these are days that we explicitly did not randomize workers and which gang supervisors implemented plot assignments through the status quo system. In line with our random assignment intervention, the results in Columns (3) and (4) show that this correlation does not exist for the remainder of the sample which supports the identifying

[10] Similar approaches to estimating ability as permanent productivity have been used in (Bandiera, Barankay and Rasul, 2010) and (Park, 2015).

[11] be clear, the set of possible co-workers is based on the gang for worker i so that $\mathbf{M}_{ift}$ is a vector of $J_i - 1$ dummy variables where J_i is the total number of pluckers in i's gang.

[12] One additional assumption for identification is that the form of any coworker peer effects is additively separable across workers.

[13] Since $\overline{Ability}_{-ift}$ is based on estimated quantities, the correct standard errors for Equation (1) need to adjust for this additional source of sampling variability. In practice, we find that the standard errors change little when we use a repeated-split sample approach to estimate worker's permanent productivity (discussed in Section 3.6) so our main results report the naive standard errors.

[14] Note that we follow the recommendation by Guryan, Kroft and Notowidigdo (2009) and include the leave-one-out mean in our test of random assignment. The inclusion of this term corrects for bias inherent in typical tests for random assignment when peer groups are small. Intuitively, this bias arises from the fact that individuals cannot be their own peers which generates a negative correlation between an individual's own characteristics and the characteristics of peers.

assumption in our linear-in-means model.[15]

Finally, in addition to producing estimates from Equation 3.1, we are also interested in testing whether there are non-linear peer effects in terms of a worker's own-ability. Specifically, we estimate the following more general model of peer effects:

$$y_{ift} = \mu_i + \sum_{q=1}^{q=4} \theta_q (D_i^q \times \overline{Ability}_{-ift}) + \delta_t + \lambda_f + \epsilon_{ift} \tag{3.3}$$

where the terms D_i^q are indicators which equal one if a person is in the q quartile of the distribution of worker ability. Previous research in education settings has used this type of specification and found evidence of notable heterogeneity in peer effects across the distribution of student ability (Hoxby and Weingarth, 2005; Carrell, Fullerton and West, 2009; Imberman, Kugler and Sacerdote, 2012; Carrell, Sacerdote and West, 2013).

3.5.1 Main Results

To test whether the average ability of co-workers affects productivity, Table 3.3 reports estimates from Equation 3.1. Column (1) shows that there is a positive and significant effect of the mean ability of peers on worker productivity. Specifically, a 10 percent increase in mean ability of peers is associated with a 0.5 percent increase in the daily amount of kilograms of tea plucked for each worker. Column (2) shows that our estimates are essentially unchanged when we condition on date-by-location fixed effects. Relative to the literature, these estimates are about half the size of estimates produced in a lab setting by Falk and Ichino (2006) and studying supermarket cashiers by Mas and Moretti (2009).

In addition to exploring peer effects in a linear-in-means model, we also test for the existence of peer effects that are vary across workers with different ability. Importantly, the existence of non-linear effects implies that there would be aggregate gains from selectively choosing workers' peer groups. Table 3.4 sheds light on this in our sample by presenting results from Equation 3.3.[16] We see that there is notable heterogeneity in the estimates for workers of different ability. Workers in the first quartile of the distribution of ability have the strongest effects: for a 10 percent increase in average coworker ability, the lowest ability worker increases his output by nearly 1 percent. The remaining estimates show there are no detectable effects of peers on any other ability-type of workers. Despite the fact that these

[15] Column (2) shows that there is positive but not statistically significant correlation between own ability and peer ability using the sample of all cycle days. As expected, the magnitude of this positive correlation falls notably in the results shown in Columns (3) and (4) where Cycle Day 1 is excluded.

[16] If peer effects were constant across individuals, then re-assigning a high-ability peer from one group to another would have equal and offsetting effects.

estimates are somewhat noisy, Figure 3.2 shows that the point the smallest point estimates are consistently decreasing in magnitude for higher ability workers.[17] In terms of policy, these results suggest that low ability workers benefit the most from having high-quality co-workers nearby.

3.6 Robustness

We construct an alternative measure of individual ability (permanent productivity) to show that our peer effect estimates are robust. Specifically, we use a repeated-split sample approach to estimate both the fixed effects for worker productivity and the effect of the mean ability of peers on own-productivity. This process is iterative and proceeds as follows. We begin by randomly splitting our sample of plucking data into two mutually exclusive, exhaustive subsamples by date. Each subsample contains 50 percent of the dates that we observe in the full sample. Using one of random subsamples, we estimate worker's fixed effects (ability) using Equation 3.2. Next, we merge these worker fixed effect estimates onto the held-out random sample of plucking data. We use this hold-out sample to estimate Equation 3.1 to obtain an estimate the effect of the average ability of peers.

The repeated-split sample approach allows us to address the concern that estimating Equation 3.2 using the entire sample of plucking data could be biased by common shocks that are not absorbed by date and field fixed effects. For example, two workers who both work on a part of a field that experiences a particularly high-growth day will both have idiosyncratically higher daily productivity which would push the estimates of $\overline{\overline{\mu}}_i$ for each worker upward in Equation 3.2; conversely, a negative shock will push the estimates downward. This is problematic because these shocks that bias estimates of worker ability will also be contained in the error term ϵ_{ift} in our model of daily worker output in Equation 3.1. these common shocks will cause violations of the assumption that $cov(\overline{Ability}_{-ift}, \epsilon_{ift}) = 0$.

Column (2) in Table 3.5 shows that the repeated-split sample approach generates a similar finding on worker peer effects relative to our main estimates. Specifically, the mean of 750 split sample estimates for β is about 0.06 which is slightly larger than the point estimate we obtained in Table 3.3. The standard deviation of the distribution of our estimates is 0.02, which shows that our split-sample approach is relatively precise. Finally, it worth noting that using the standard deviation of the repeated-split sample estimates provides us with a measure of the standard error of β that takes into account the uncertainty due to the two-step nature of our estimation strategy. This estimate is comparable to those we find

[17] Note that we can reject the null hypothesis of equal effects for the highest and lowest ability workers (p-value=0.01).

through the simple clustered standard error estimates in our main specifications.

3.7 Discussion and Interpretation

The evidence presented thus far shows that co-worker ability has an impact on productivity. A range of mechanisms could generate positive peer effects in general, but our setting allows us to rule out two of these immediately. First, unlike in many previously studied settings, our context rules out the possibility of peer effects being driven by externalities in the production process since there is no cooperation and no need for workers to coordinate. Second, the fact that workers receive piece-rates also rules out that our peer effects are driven by the firm's compensation scheme. With this in mind, this section proceeds to consider three other types of mechanisms that could be driving our estimates of peer effects.

One potential mechanism to explain our findings is learning ("knowledge spillovers"). It is conceivable that plot neighbors learn from observing each other work, thereby generating the positive effects that we observe.[18] To explore this possibility, we test whether peer effects in our setting are heterogeneous with respect to workers' past experience. Under the learning hypothesis, we would expect the effects of average peer ability would be largest for workers who have no prior or relatively less experience.

Table 3.6 presents results from augmented versions of Equation 3.1 in which we add measures of worker experience. The results in Column (1) replicate the estimate from our baseline specification for the sample of workers for whom we have self-reported experience. Column (2) builds on our main specification by adding an interaction between a dummy indicating status as a new worker (no prior experience) and our measure of peer ability. The estimate for this interaction is significant and implies that the effect of higher-ability peers is actually negative for new workers. As an alternative test for heterogeneity in peer effects by experience, we create dummies based on the quartiles of worker experience observed in our sample. We interact these dummies with our measure of average peer ability and present the results for these terms in Column (3). Although the results for this specification are not precise, the point estimates for the least experience and most experienced workers are remarkably similar which is not consistent with the idea that learning drives peer effects in our setting.

Another leading mechanism for explaining workplace peer effects is socialization between workers. In a relatively similar setting, Bandiera, Barankay and Rasul (2010) detect evidence of peer effects among friends who pick fruit at a large agricultural firm in the UK. Their

[18] Among previous studies testing for the existence of knowledge spillovers, Jackson and Bruegmann (2009) find evidence of knowledge spillovers among teachers while Waldinger (2012) finds no evidence among university scientists.

analysis suggests that socialization drives their estimates. When slow fruit pickers work near friends who are typically fast, they work harder to catch up. Similarly, relatively fast pickers slow down for their slower friends.

Using data on social networks, we provide evidence that suggests the peer effects in our sample are not driven by the desire to socially interact. Specifically, we use self-reported friendship between pluckers to identify when workers are plucking on plots near their friends. We then compute the average ability of nearby co-workers who are friends. Similarly, we calculate the average ability of co-workers who are not friends. On the average day in our sample, a worker has about three plot neighbors that are friends. We use these two separate measures of average co-worker ability in our basic linear-in-means specification and report the results in Column (3) of Table 3.7. The point estimates show that there is an effect of working near higher ability non-friends while there is no detectable impact for the average ability of friends.[19]

This pattern of results suggests that our peer effects are not driven by either learning or socialization. A final prominent possibility that we consider is that the peer effects we measure might operate through motivation and "contagious enthusiasm". A key prediction of this mechanism is that exposure to faster coworkers is beneficial, and thus workers should be willing to pay for higher-ability peers. To test this prediction, we conducted a supplementary survey and choice experiment for a subset of tea workers during the 2015-2016 harvesting season. In this experiment, we asked workers whether they wanted higher-ability peers, and whether they would be willing to give up part of the compensation that they received for taking part in the survey (workers were each given two bars of soap as a token of thanks for taking the survey).

Panel A of Table 3.8 reports that 71 percent of workers would like to be assigned next to a fast (high ability) peer in their gang. Further, Panel B shows that these workers seeking re-assignment are willing to pay for these peers: 71 percent of workers who want a fast peer would be willing to give up one bar of soap while 55 percent would be willing to give up two bars of soap. When asked for the main reason for their choices in an open-ended question, 83 percent workers state that faster peers provide motivation. Only 15 percent state learning as a reason for wanting higher-ability peers.

The results from our willingness to pay experiment strongly suggest that motivation is the key driver of the peer effects we measure in this study. They rule out a range of other potential mechanisms posited in the literature, such as shame or a desire to avoid being last (Kandel and Lazear, 1992; Kuziemko et al., 2014). Since workers are willing to pay for faster

[19] While these point estimates appear quite different, we cannot reject a test of the null hypothesis that the effects of non-friends and friends are equal (p-value = 0.22).

peers, shame-type mechanisms can only be the operative mechanism inasmuch as it serves as a commitment device, inducing workers to reach a higher level of effort that they truly would like to achieve.

3.8 Conclusion

This paper provides evidence on workplace peer effects by conducting an experimental intervention with an agricultural firm in Malawi from February 2015 to April 2015. We randomly assigned tea pluckers to plot assignments on fields and use this variation in peer composition to examine the effect of mean coworker ability (permanent productivity) on workers' output. In addition, our analysis also explores whether the effect of peer ability varies by workers' own ability of workers.

Using administrative data on daily productivity, we find that the average of coworker ability has a positive and significant effect: increasing the average of coworker ability by 10 percent increases own-productivity by about 0.5 percent. Further, supplementary analysis suggests that these peer effects are non-linear, with the lowest ability workers being the most responsive to working near higher ability peers. This finding is notable because it implies that re-sorting workers on the basis of ability could generate gains in aggregate productivity, which is only possible if peer effects are non-linear.

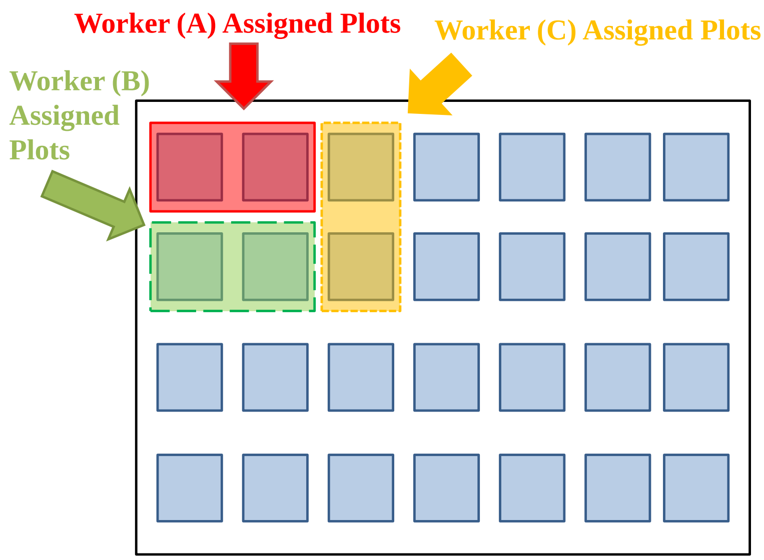
To shed light on the mechanisms driving our peer effect estimates, we conducted a choice experiment in the next harvesting season that allowed workers to choose new coworkers as plot neighbors. In this experiment, we find that 71 percent of workers wanted to be assigned to a fast (high-ability) coworker. Moreover, workers were willing to pay for faster coworkers: 55 percent of workers were willing to give up two bars of soap (worth 18 percent of daily wages) that we had given them as a gift for survey participation. In open-ended follow-up questions, 83 percent of workers state that working near faster peers motivates them.

This analysis provides strong evidence that workplace peer effects in our setting stem from the effect that peers have on motivation. In additional analysis, we do not find evidence that the effects we detect are driven by learning or worker socialization. Finally, the fact that workers receive piece rates and there is no cooperation in tea plucking rules out that the effects in our setting stem from production externalities or incentives of the firm's compensation scheme.

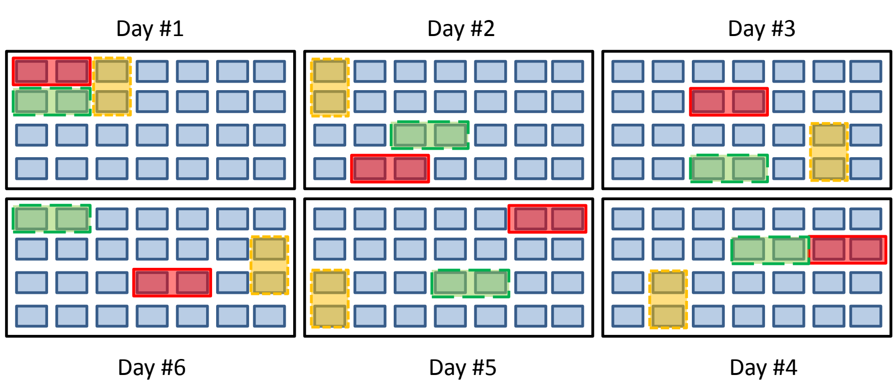
3.9 Figures and Tables

Figure 3.1: Tea Worker Field Assignment Illustrations

(a) Hypothetical Assignment for Three Tea Workers

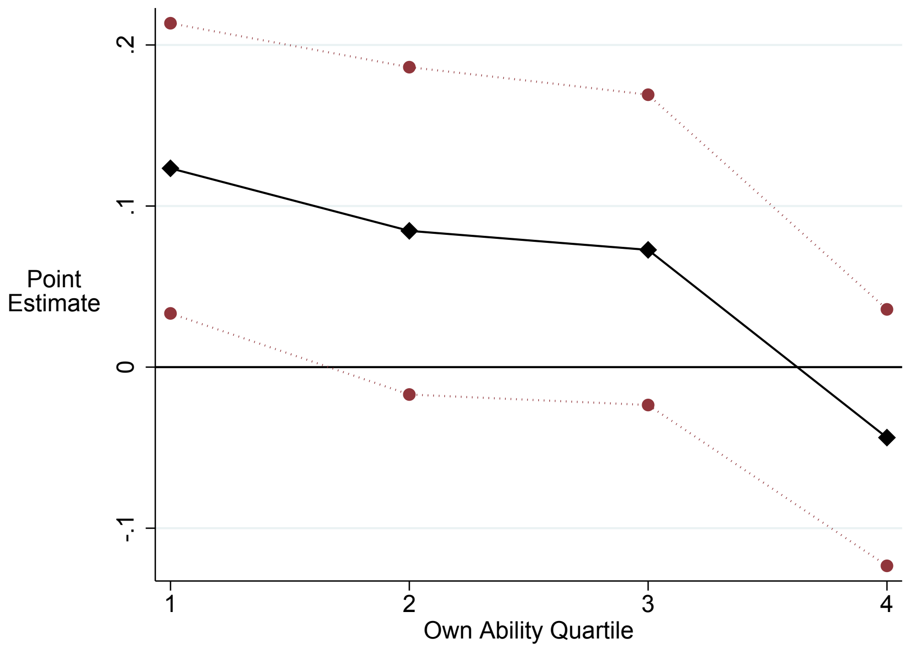


(b) Plot Assignments Change Over Days in Harvesting Cycle



Notes: The two panels illustrate work assignments for tea workers at the Lujeri Tea Estates in Malawi. Panel A shows how three workers would be assigned two plots each. For our analysis, all workers A, B and C would be neighboring co-workers. Panel B shows how plot work assignments change over the course of a harvest cycle that lasts 6 calendar days and visits distinctly different fields. On some days and fields, workers A, B and C are neighbors. Yet, there are also cases where they are not neighbors: for example, on Day #3, #5 and #6, workers A, B and C are not assigned to work in neighboring plots.

Figure 3.2: Non-Linear Peer Effect Estimates



Notes: This figure presents results from a regression of daily output (the log of kilograms of tea plucked) on the mean ability of physically nearby co-workers interacted with dummies for the worker's own ability level. The underlying data is a panel at the worker and day level. All specifications control for individual fixed effects. The results parallel Column (1) of Table 3.4, and control separately for date and location fixed effects. The measure of daily output comes from administrative data obtained from Lujeri Tea Estates. Information on neighbors was collected by staff for this project. 95% confidence intervals are shown with dashed lines; standard errors are clustered at the worker level.

Table 3.1: Summary Statistics, Lujeri Worker Sample

	(1) Average	(2) Std. Deviation	(3) 10th Percentile	(4) 90th Percentile	(5) Obs (N)
Age	37.43	10.64	25.00	52.00	944
Female (=1)	0.43	0.50	0.00	1.00	944
Married (=1)	0.63	0.48	0.00	1.00	944
New Worker (=1)	0.07	0.26	0.00	0.00	944
Experience (Yrs.)	7.72	8.31	0.08	15.50	944
Ability (Estimate)	0.03	0.20	-0.22	0.30	1,046
# Neighbors	4.69	1.82	2.00	8.00	35,460
Log(Mean Peer Ability)	0.05	0.10	-0.07	0.16	35,460
Output (kgs.)	69.41	36.17	27.00	118.00	35,460

Notes: This table presents descriptive statistics based on survey data we collected for a sample of tea pluckers at the Lujeri Tea Estates in Malawi.

Table 3.2: Balance Test: Comparing Own and Peer Ability

	Dependent Variable: Log(Own-Ability)			
	(1)	(2)	(3)	(4)
Log(Mean Peer Ability)	0.129*	0.0415	0.0201	0.00518
	(0.0737)	(0.0283)	(0.0308)	(0.00849)
Leave-One-Out Mean	-1.455***	-1.552***	-1.595***	-43.27***
	(0.404)	(0.390)	(0.394)	(0.672)
Cycle Day 1	Yes	Yes	No	No
Remaining Cycle Days	No	Yes	Yes	Yes
Worker Fixed Effects	No	No	No	Yes
Date Fixed Effects	No	No	No	Yes
Observations	9,531	45,058	35,460	35,460
R2	0.022	0.024	0.026	0.927

Notes: This table presents results from a regression of our measure of a worker's own ability on the mean ability of physically nearby co-workers. The underlying data is a panel at the worker and day level. All specifications control for individual fixed effects. The results in Columns (1) are from the sample of "Cycle Day 1" days which did not have random assignment of workers to plot assignments at the tea estate. Column (2) presents results using the full sample of all dates and cycle dates in our data. Columns (3) and (4) use the sample of all non Cycle 1 days – this is the sample for which we randomly assigned workers to locations on fields. The measure of daily output comes from administrative data obtained from Lujeri Tea Estates. Information on neighbors was collected by staff for this project. Standard errors are clustered at the worker level.

Table 3.3: Effects of Workplace Peers, Linear Model

	Dependent Variable: Log of Daily Output	
	(1)	(2)
Log(Mean Peer Ability)	0.0560**	0.0497**
	(0.0238)	(0.0210)
Worker Fixed Effects	Yes	Yes
Date Fixed Effects	Yes	No
Location (Field) Fixed Effects	Yes	No
Date by Location Fixed Effects	No	Yes
Observations	35,460	35,460
R2	0.416	0.736

Notes: This table presents results from a regression of daily output (the log of kilograms of tea plucked) on the mean ability of physically nearby co-workers. The underlying data is a panel at the worker and day level. All specifications control for individual fixed effects. The results in Columns (1) and (2) use two different approaches to control for date and location effects. The measure of daily output comes from administrative data obtained from Lujeri Tea Estates. Information on neighbors was collected by staff for this project. Standard errors are clustered at the worker level.

Table 3.4: Effects of Workplace Peers, Non-Linear Model

	Dependent Variable: Log of Daily Output	
	(1)	(2)
Log(Mean Peer Ability) X Ability Quartile 1	0.123***	0.0926**
	(0.0460)	(0.0377)
Log(Mean Peer Ability) X Ability Quartile 2	0.0846	0.0679
	(0.0518)	(0.0500)
Log(Mean Peer Ability) X Ability Quartile 3	0.0728	0.0391
	(0.0491)	(0.0427)
Log(Mean Peer Ability) X Ability Quartile 4	-0.0438	0.00350
	(0.0406)	(0.0356)
Worker Fixed Effects	Yes	Yes
Date Fixed Effects	Yes	No
Location (Field) Fixed Effects	Yes	No
Date by Location Fixed Effects	No	Yes
Observations	35,460	35,460
R2	0.416	0.736

Notes: This table presents results from a regression of daily output (the log of kilograms of tea plucked) on the mean ability of physically nearby co-workers interacted with dummies for the worker's own ability level. The underlying data is a panel at the worker and day level. All specifications control for individual fixed effects. The results in Columns (1) and (2) use two different approaches to control for date and location effects. The measure of daily output comes from administrative data obtained from Lujeri Tea Estates. Information on neighbors was collected by staff for this project. Standard errors are clustered at the worker level.

Table 3.5: Effects of Workplace Peers, Split Sample Estimates of Linear Model

	Dependent Variable: Log of Daily Output
	(1)
Log(Mean Peer Ability)	0.065***
	(0.021)
Worker Fixed Effects	Yes
Date Fixed Effects	Yes
Location (Field) Fixed Effects	Yes
Date by Location Fixed Effects	No
Split Sample Reps	750

Notes: This table presents results from a split-sample estimation approach to estimating the effect of mean co-worker ability on daily output. As described in Section 3.6, we proceed in two steps. First, we randomly split our sample of plucking data into two mutually exclusive, exhaustive subsamples. Each subsample contains 50 percent of the dates that we observe in the full sample. Using one of random subsamples, we estimate worker's fixed effects (ability) using Equation 3.2 from the text. Second, we merge these worker fixed effect estimates onto the held-out random sample of plucking data. We use this hold-out sample to estimate Equation 2.1 to obtain an estimate the effect of the average ability of peers. We repeat this process 750 times to generate a sample of estimates for effect of mean ability of coworkers and present this in Column (1) above.

Table 3.6: Peer Effects by Experience Level

	Dependent Variable: Log of Daily Output		
	(1)	(2)	(3)
Log(Mean Peer Ability)	0.0585**	0.0692***	
	(0.0256)	(0.0268)	
New Worker (=1) X Log(Mean Peer Ability)		-0.158*	
		(0.0819)	
Quartile 1 Exp. X Log(Mean Peer Ability)			0.0693
			(0.0528)
Quartile 2 Exp. X Log(Mean Peer Ability)			0.0366
			(0.0526)
Quartile 3 Exp. X Log(Mean Peer Ability)			0.0675
			(0.0473)
Quartile 4 Exp. X Log(Mean Peer Ability)			0.0632
			(0.0527)
Worker Fixed Effects	Yes	Yes	Yes
Date Fixed Effects	Yes	Yes	Yes
Location (Field) Fixed Effects	Yes	Yes	Yes
Date by Location Fixed Effects	No	No	No
Observations	32,921	32,921	32,921
R2	0.411	0.411	0.411

Notes: This table presents results from a regression of daily output (kilograms of tea plucked) on the mean ability of physically nearby co-workers. The underlying data is a panel at the worker and day level. All specifications control for individual fixed effects. The results in Column (2) and (3) are from specifications that include additional interactions based on the worker's self-reported experience at Lujeri. The measure of daily output comes from administrative data obtained from Lujeri Tea Estates; information on neighbors was collected by staff for this project; and, measures of experience are based on our survey data collection. Standard errors are clustered at the worker level.

Table 3.7: Effects of Friends and Non-Friends in the Workplace

	Dependent Variable: Log of Daily Output		
	(1)	(2)	(3)
Log(Mean Peer Ability), Non-Friends	0.052***		0.052***
	(0.020)		(0.020)
Any Non-friends (=1)	0.017		0.012
	(0.037)		(0.037)
Log(Mean Peer Ability), Friends		0.006	0.007
		(0.032)	(0.032)
Any Friends (=1)		-0.006	-0.007
		(0.007)	(0.007)
Worker Fixed Effects	Yes	Yes	Yes
Date Fixed Effects	Yes	Yes	Yes
Location (Field) Fixed Effects	Yes	Yes	Yes
Date by Location Fixed Effects	No	No	No
Observations	35,445	35,445	35,445
R2	0.415	0.415	0.415

Notes: This table presents results from a regression of daily output (the log of kilograms of tea plucked) on measures of the mean ability of nearby co-workers who are friends and non-friends. The underlying data is a panel at the worker and day level. All specifications control for individual fixed effects. The results in Column (2) and (3) are from specifications that include additional interactions based on the worker's self-reported experience at Lujeri. The measure of daily output comes from administrative data obtained from Lujeri Tea Estates; information on neighbors was collected by staff for this project; and, measures of experience are based on our survey data collection. Standard errors are clustered at the worker level.

Table 3.8: Willingness to Pay for Fast Peers

	(1) Pct.	(2) Obs.
Panel A. All Survey Respondents		
Who do you want to be re-assigned next to?		
A fast plucker in your gang	0.71	724
A slow plucker in your gang	0.05	724
Any person of your choosing	0.11	724
No-reassignment	0.14	724
Panel B. Respondents who want to be next to fast pluckers		
If you could switch to be near a fast plucker…		
…would you be willing to give up 1 bar of soap?	0.71	515
…would you be willing to give up 2 bar of soap?	0.55	515

Notes: This table presents statistics from survey data that we collected for tea pluckers at the Lujeri Tea Estates. For the choice experiment, respondents were given a gift of two bars of soap (18 percent of average daily wages) and asked if they would be willing to give up soap in exchange for being re-assigned.

APPENDIX A

Appendix for Moved to Opportunity: The Long-Run Effect of Public Housing Demolition on Labor Market Outcomes of Children

A.1 Detailed Analysis of Differential Attrition

As explained in Section 1.5.1, 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 δ_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, β 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. $Y_{i,t+j} = 0 \ \forall j \quad \in \{1, \ldots, T-t\}$, where Y is an administrative data outcome and T denotes the last unit of time in the data). For a single outcome k, 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 1.1 where the outcome is imputed attrition $a^k_{i,t}$. 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.

A1 Detailed Description of Sample Definition

As stated in Section 1.4, 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.

A2 Program Rules for Housing Vouchers

A2.1 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.

A2.2 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:

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

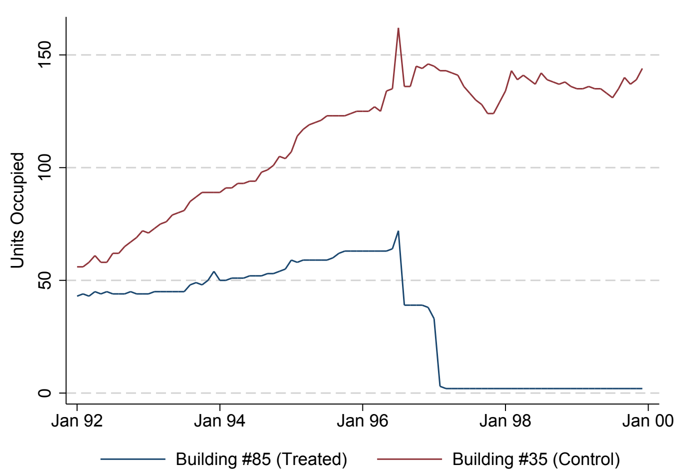
Note that TANF benefits are included when determining program eligibility and the family's rent contribution, while the value of other forms of government assistance such as foodstamps, EITC and Medicaid are not. Earnings by children younger than age 18 or payments received for foster children are also 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.

A3 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).

A4 CHAC 1997 Lottery Data and Summary Statistics

A4.1 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, Kapustin and Ludwig (2015). As mentioned in Section 1.9, 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, Kapustin and Ludwig (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 A4.2. 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 95 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 95 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.

A4.2 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, Kapustin and Ludwig (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 1.6.

A5 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, Liebman and Katz (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, Liebman and Katz, 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 π. Parents can use the voucher to lease private market housing in a new neighborhood that has high safety θ, where $\theta > \overline{q}$ and $\overline{q}$ 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.[1] Specifi-

[1] Note that the model is solved via backward induction whereby the parent forecasts consumption and

cally, the log-utility functional form implies that we can re-write the volunteering condition as:

$$2\ log\left(\frac{I-\theta}{I+q_i}\right) > c$$

$$\Rightarrow \underbrace{\frac{I+\theta}{exp^{.5c}}}_{\equiv\gamma} > q_i$$

In other words, parents with sufficiently poor perceptions of neighborhood safety – below γ – 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 $\overline{q}$ and is assumed to be the actual level of 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 σ^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

utility when they volunteer for MTO.

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 θ; (3) the mean of public housing neighborhood quality $\overline{q}$; (4) the variance in signals about neighborhood quality σ^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 θ to zero.

With these assumptions, I can use three moments from the data to solve for the three remaining parameters: $\overline{q}$, σ 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: $770 = E(Y_{1i} - Y_{0i}|\gamma < q_i) = .5\left(-\overline{q} + \sigma\phi(\alpha)/(1 - \Phi(\alpha))\right)$.[2] 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).

[2] 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.

A6 Estimating the Local Average Treatment Effect (LATE) of Moving for MTO

As discussed in Section 1.9, the Moving-to-Opportunity (MTO) studies are an important comparison for the findings in this paper. The MTO studies focus on reporting intention-to-treat (ITT) and treatment on the treated (TOT) effects of receiving a voucher offer. Specifically, these studies use the following estimating equation to estimate the ITT effects:

$$y_i = \alpha + \beta_E^{ITT} Exp_i + \beta_S^{ITT} S8_i + \gamma X_i + s_i\delta + \epsilon_i \tag{A1}$$

The MTO studies also estimate the TOT effects to understand the effect of changing neighborhoods through the intervention. Specifically, the TOT is calculated by dividing the ITT effect by the take-up rate for MTO vouchers. This is implemented using two-stage least squares where the endogenous variable in the first stage includes indicators for *taking*-up the experimental and Section 8 vouchers. The instruments are indicators for treatment group assignment (Kling, Ludwig and Katz, 2005).

While MTO focuses on the impact of voucher offers and take-up, I do not have data on voucher usage for the demolition sample.[3] Due to this data limitation, I focus on estimating the effect of the treatment of moving out of project-based housing. This parameter can be measured in both the demolition and MTO samples because I can use MTO treatment group assignment and living in a building set for demolition as instruments for the endogenous treatment of moving out of public housing.

The specific parameter that I calculate across samples is the local average treatment effect (LATE) of moving out of public housing which is calculated as:

$$LATE = \frac{\mathbb{E}(Y|Z=1) - \mathbb{E}(Y|Z=0)}{\mathbb{E}(D=1|Z=1) - \mathbb{E}(D=1|Z=0)} \tag{A2}$$

Note that the LATE in this context differs from the average treatment on the treated (ATET) – $\mathbb{E}(Y_{1i} - Y_{0i}|D_i = 1)$ – because there are always-takers in the MTO and demolition samples. Moreover, note that the LATE in this context differs from the TOT effects reported in MTO

[3] Recall that any household displaced by demolition received an offer for a Section 8 voucher. Alternatively, households could also request to move to another public housing project that had openings although this option is unlikely given the long waiting list for project-based public housing in Chicago.

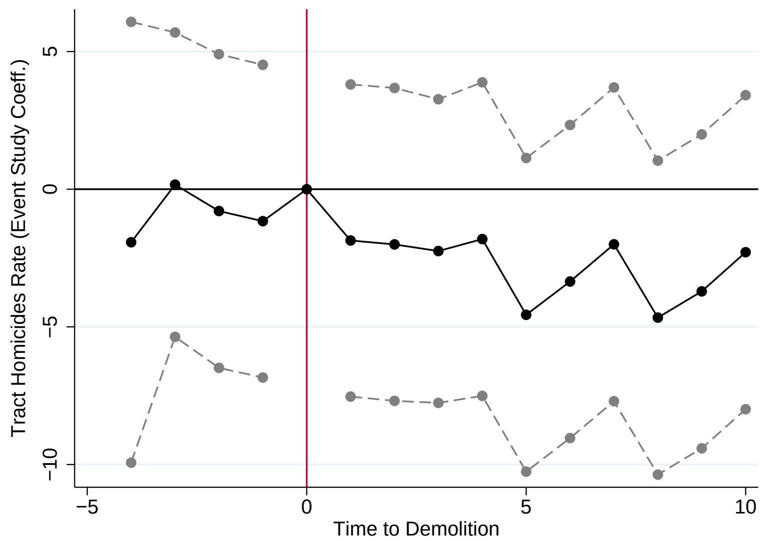
since the denominator is difference in rates of moving rather than the MTO voucher take-up rate.

Because I lack access to the MTO data, I must construct estimates of the LATE of moving using estimates published in the MTO studies. Specifically, I use estimates of the ITT effect for a given youth labor market outcome from Exhibit 5.8 (p. 154) of Sanbonmatsu et al. (2011) for the numerator in Equation A2. Note that the youth sample used for this estimate includes only "grown children" who were under age 18 at baseline and over age 20 as of December 31, 2007. Since randomization for MTO took place between 1994 and 1998, this implies that the youngest child in the grown children sample would have been 7 years old at baseline and this corresponds to the minimum age included in my demolition sample. For the denominator in A2, I use estimates of the impact of the MTO voucher offers on the probability of making any residential move out of public housing from Table IV of Katz, Kling and Liebman (2001). These mobility rates are measured using data from a follow-up survey sent to MTO households about 2 years after randomization.

As mentioned in Section 1.9, I construct confidence intervals for the LATE of moving out of public housing using a parametric bootstrap procedure (Efron and Tibshirani, 1994). Specifically, I create 10,000 draws of the ITT effect on youth labor market outcomes and first-stage estimates of moving estimates from normal distributions with means and standard deviations set equal to the point estimates and standard errors from the previously mentioned published MTO studies. This generates 10,000 realizations of the LATE based on the MTO data. The values of the 2.5^{th} and 97.5^{th} percentiles of the distribution of the LATE constitute the 95-percent confidence interval for the LATE of relocation that one would obtain for the MTO sample.

A7 Appendix Figures and Tables

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

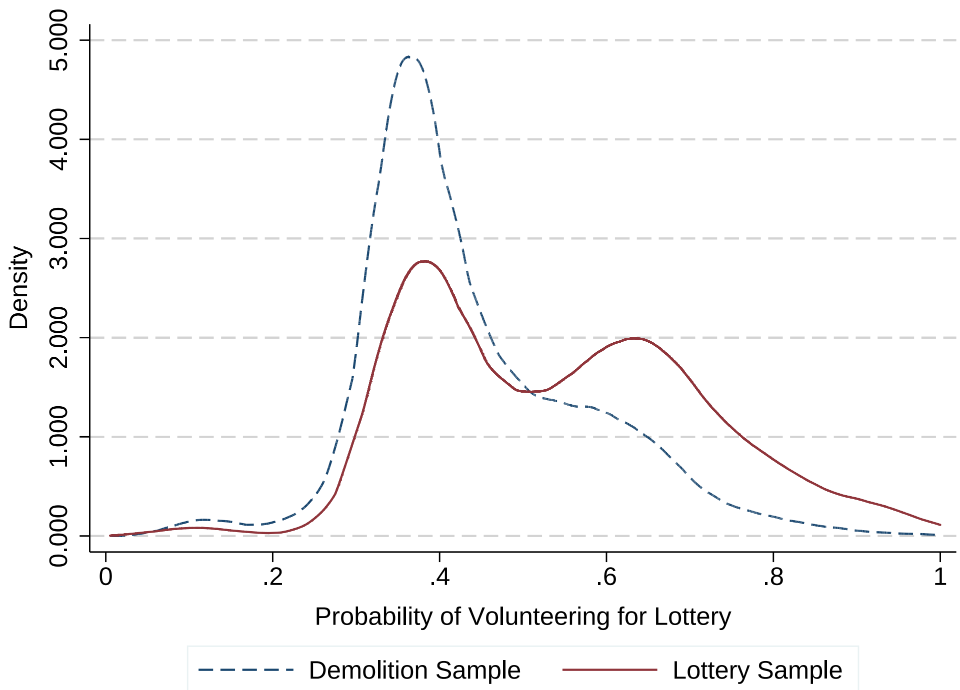


Notes: This figure plots event-study coefficients (solid black dots) from a regression of the tract-level homicide rate on time-dummies. Specifically, the figure plots a set of coefficients π_j and τ_j from the following specification:

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

where the dependent variable $h_{i,t}$ is homicide rate for tract i at year t. The terms μ_i and δ_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 δ_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. 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 (β')	Difference: Near–Far, Within Estimate (π)
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 1.2. As described in the text, the estimate β' is the difference in outcomes between displaced and non-displaced children who are part of the far group. Similarly, the estimate π 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)

Subgroup	(1) Fraction of All Children	(2) Employment Control Mean	(3) 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 ($)

Subgroup	(1) Fraction of All Children	(2) Earnings Control Mean	(3) 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, Krieger and Yekutieli (2006). Columns (1) and (2) repeat the results from Tables 1.3, 1.5 and 1.7 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)	(2)	(3)	(4)	(5)
	Lottery Sample	Demolition Sample	p-value Difference: Lottery–Demolition	Weighted Demolition Sample	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 1.9.4 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 1.1 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 1.7 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 *** $p < 0.01$.

BIBLIOGRAPHY

Akerlof, George A., and Rachel E. Kranton. 2000. "Economics and Identity." *The Quarterly Journal of Economics*, 115(3): 715–753.

Aliprantis, Dionissi, and Daniel Hartley. 2015. "Blowing it Up and Knocking it Down: The Local and City-wide Effects of Demolishing High Concentration Public Housing on Crime." *Journal of Urban Economics*, 88: 67–81.

Anderson, Michael L. 2008. "Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American Statistical Association*, 103(484): 1481–1495.

Angrist, Joshua D. 2004. "Treatment effect heterogeneity in theory and practice*." *The Economic Journal*, 114(494).

Angrist, Joshua D., Guido W. Imbens, and Donald B. Rubin. 1996. "Identification of Causal Effects Using Instrumental Variables." *Journal of the American Statistical Association*, 91(434): 444–455.

Azoulay, Pierre, Joshua S. Graff Zivin, and Jialan Wang. 2010. "Superstar Extinction." *The Quarterly Journal of Economics*, 125(2): 549–589.

Babcock, Philip, Kelly Bedard, Gary Charness, John Hartman, and Heather Royer. 2015. "Letting down the Team? Social Effects of Team Incentives." *Journal of the European Economic Association*, 13(5): 841–870.

Bandiera, Oriana, Iwan Barankay, and Imran Rasul. 2010. "Social Incentives in the Workplace." *The Review of Economic Studies*, 77(2): 417–458.

Bayer, Patrick, Randi Hjalmarsson, and David Pozen. 2009. "Building Criminal Capital behind Bars: Peer Effects in Juvenile Corrections." *The Quarterly Journal of Economics*, 124(1): 105–147.

Bayer, Patrick, Stephen L. Ross, and Giorgio Topa. 2008. "Place of Work and Place of Residence: Informal Hiring Networks and Labor Market Outcomes." *Journal of Political Economy*, 116(6): 1150–1196.

Benjamini, Yoav, Abba M. Krieger, and Daniel Yekutieli. 2006. "Adaptive linear step-up procedures that control the false discovery rate." *Biometrika*, 93(3): 491–507.

Bertrand, Marianne, Erzo F. P. Luttmer, and Sendhil Mullainathan. 2000. "Network Effects and Welfare Cultures." *The Quarterly Journal of Economics*, 115(3): 1019–1055.

Billings, Stephen B., David J. Deming, and Stephen L. Ross. 2016. "Partners in Crime: Schools, Neighborhoods and the Formation of Criminal Networks." National Bureau of Economic Research Working Paper 21962.

Bishaw, Alemayehu. 2014. "Changes in Areas With Concentrated Poverty: 2000 to 2010." *American Community Survey.*

Black, Sandra E., Paul J. Devereux, Katrine V. Løken, and Kjell G. Salvanes. 2014. "Care or Cash? The Effect of Child Care Subsidies on Student Performance." *Review of Economics and Statistics*, 96(5): 824–837.

Bovard, James. 2011. "Raising Hell in Subsidized Housing." *Wall Street Journal.*

Breza, Emily, and Arun G. Chandrasekhar. 2015. "Social Networks, Reputation and Commitment: Evidence From a Savings Monitors Experiment." National Bureau of Economic Research.

Brooks, Tricia, Joe Touschner, Samantha Artiga, Jessica Stephens, Alexandra Gates, and Jessica Stephens. 2015. "Modern Era Medicaid." Kaiser Family Foundation.

Carrell, Scott E., and Mark L. Hoekstra. 2010. "Externalities in the Classroom: How Children Exposed to Domestic Violence Affect Everyone's Kids." *American Economic Journal: Applied Economics*, 2(1): 211–228.

Carrell, Scott E., Bruce I. Sacerdote, and James E. West. 2013. "From Natural Variation to Optimal Policy? The Importance of Endogenous Peer Group Formation." *Econometrica*, 81(3): 855–882.

Carrell, Scott E., Richard L. Fullerton, and James E. West. 2009. "Does Your Cohort Matter? Measuring Peer Effects in College Achievement." *Journal of Labor Economics*, 27(3): 439–464.

Carr, Jillian, and Vijetha Koppa. 2016. "The Effect of Housing Vouchers on Crime: Evidence from a Lottery."

Case, Anne C., and Lawrence F. Katz. 1991. "The company you keep: The effects of family and neighborhood on disadvantaged youths." *National Bureau of Economic Research*, , (3705).

Census. 1990. "Poverty in the United States." Bureau of the Census.

Chetty, Raj, John N. Friedman, and Jonah E. Rockoff. 2014. "Measuring the Impacts of Teachers II: Teacher Value-Added and Student Outcomes in Adulthood." *American Economic Review*, 104(9): 2633–2679.

Chetty, Raj, John N. Friedman, Nathaniel Hilger, Emmanuel Saez, Diane Whitmore Schanzenbach, and Danny Yagan. 2011. "How Does Your Kindergarten Classroom Affect Your Earnings? Evidence from Project Star." *The Quarterly Journal of Economics*, 126(4): 1593–1660.

Chetty, Raj, Nathaniel Hendren, and Lawrence F. Katz. 2015. "The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment." National Bureau of Economic Research.

Chetty, Raj, Nathaniel Hendren, and Lawrence F. Katz. 2016. "The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment." *American Economic Review*, 106(4): 855–902.

Chyn, Eric. 2015. "Moved to Opportunity: The Long-Run Effect of Public Housing Demolition on Labor Market Outcomes of Children."

Collinson, Robert, Ingrid Gould Ellen, and Jens Ludwig. 2015. "Low-Income Housing Policy." National Bureau of Economic Research Working Paper 21071.

Cornelissen, Thomas, Christian Dustmann, and Uta Schönberg. 2013. "Peer Effects in the Workplace." Institute for the Study of Labor (IZA) IZA Discussion Paper 7617.

Coulton, Claudia J., Julian Chow, Edward C. Wang, and Marilyn Su. 1996. "Geographic Concentration of Affluence and Poverty in 100 Metropolitan Areas, 1990." *Urban Affairs Review*, 32(2): 186–216.

Damm, Anna Piil, and Christian Dustmann. 2014. "Does Growing Up in a High Crime Neighborhood Affect Youth Criminal Behavior?" *American Economic Review*, 104(6): 1806–1832.

Deming, David J. 2011. "Better Schools, Less Crime?" *The Quarterly Journal of Economics*, 126(4): 2063–2115.

DHS. 2014. "Illinois DHS – Supplemental Nutrition Assistance Program."

DHS. 2015. "Illinois DHS – Work Pays."

DiNardo, John, and David S. Lee. 2011. "Program Evaluation and Research Designs." In *Handbook of Labor Economics*. Vol. 4, 463–536. Elsevier.

Durlauf, Steven N. 2004. "Chapter 50 Neighborhood effects." In *Handbook of Regional and Urban Economics*. Vol. Volume 4 of *Cities and Geography*, , ed. J. Vernon Henderson and Jacques-François Thisse, 2173–2242. Elsevier.

Efron, Bradley, and R. J. Tibshirani. 1994. *An Introduction to the Bootstrap.* CRC Press.

Ellen, Ingrid Gould, and Margery Austin Turner. 1997. "Does Neighborhood Matter? Assessing Recent Evidence." *Housing Policy Debate*, 8(4): 833–866.

Falk, Armin, and Andrea Ichino. 2006. "Clean Evidence on Peer Effects." *Journal of Labor Economics*, 24(1): 39–57.

Finkel, Meryl, and Larry Buron. 2001. "Study on Section 8 Voucher Success Rates: Volume I: Quantitative Study of Success Rates in Metropolitan Areas." Abt Associates.

Fredriksson, Peter, Björn Öckert, and Hessel Oosterbeek. 2013. "Long-Term Effects of Class Size." *The Quarterly Journal of Economics*, 128(1): 249–285.

Freedman, Matthew, and Emily G. Owens. 2011. "Low-Income Housing Development and Crime." *Journal of Urban Economics*, 70(2–3): 115–131.

Fryer, Roland G., Jr., and Lawrence F. Katz. 2013. "Achieving Escape Velocity: Neighborhood and School Interventions to Reduce Persistent Inequality." *The American Economic Review*, 103(3): 232–237.

Galster, George, Dave E. Marcotte, Marv Mandell, Hal Wolman, and Nancy Augustine. 2007. "The Influence of Neighborhood Poverty During Childhood on Fertility, Education, and Earnings Outcomes." *Housing Studies*, 22(5): 723–751.

GAO. 1997. "Public Housing: Status of the HOPE VI Demonstration Program."

GAO. 2003. "Public Housing HOPE VI Resident Issues and Changes in Neighborhoods Surrounding Grant Sites."

GAO. 2007. "Public Housing: Information on the Financing, Oversight, and Effects of the HOPE VI Program."

Garza, Melita. 1999*a*. "9 High-rises At Taylor Homes Slated To Close." *Chicago Tribune*.

Garza, Melita. 1999*b*. "CHA Evacuates High-rise Units Without Heat." *Chicago Tribune*.

Goering, John, Joan Kraft, Feins Judith, Dennis McInnis, Mary Holin, and Huda Elhassan. 1999. "Moving to Opportunity for Fair Housing Demonstration Program: Current Status and Initial Findings." U.S. Department of Housing and Urban Development.

Gould, Eric D, and Eyal Winter. 2009. "Interactions between Workers and the Technology of Production: Evidence from Professional Baseball." *Review of Economics and Statistics*, 91(1): 188–200.

Grogger, Jeffrey. 2013. "Bounding the Effects of Social Experiments: Accounting for Attrition in Administrative Data." National Bureau of Economic Research Working Paper 18838.

Guryan, Jonathan, Kory Kroft, and Matthew J Notowidigdo. 2009. "Peer Effects in the Workplace: Evidence from Random Groupings in Professional Golf Tournaments." *American Economic Journal: Applied Economics*, 1(4): 34–68.

Hastings, Justine, Thomas Kane, and Douglas Staiger. 2006. "Heterogeneous preferences and the efficacy of public school choice." National Bureau of Economic Research Working Paper 12145.

Hawes, Christine. 1992. "Now Things Move Quickly At Cabrini." *Chicago Tribune*.

Heckman, James, Justin L. Tobias, and Edward Vytlacil. 2001. "Four Parameters of Interest in the Evaluation of Social Programs." *Southern Economic Journal*, 68(2): 211–223.

Herbst, Daniel, and Alexandre Mas. 2015. "Peer Effects on Worker Output in the Laboratory Generalize to the Field." *Science (New York, N.Y.)*, 350(6260): 545–549.

Hoxby, Caroline M., and Gretchen Weingarth. 2005. "Taking Race Out of the Equation: School Reassignment and the Structure of Peer Effects." Working paper.

Imberman, Scott A, Adriana D Kugler, and Bruce I Sacerdote. 2012. "Katrina's Children: Evidence on the Structure of Peer Effects from Hurricane Evacuees." *American Economic Review*, 102(5): 2048–2082.

Jackson, C. Kirabo, and Elias Bruegmann. 2009. "Teaching Students and Teaching Each Other: The Importance of Peer Learning for Teachers." *American Economic Journal: Applied Economics*, 1(4): 85–108.

Jacob, B. A., M. Kapustin, and J. Ludwig. 2015. "The Impact of Housing Assistance on Child Outcomes: Evidence from a Randomized Housing Lottery." *The Quarterly Journal of Economics*, 130(1): 465–506.

Jacob, Brian A. 2004. "Public Housing, Housing Vouchers, and Student Achievement: Evidence from Public Housing Demolitions in Chicago." *The American Economic Review*, 94(1): 233–258.

Jacob, Brian A, and Jens Ludwig. 2012. "The Effects of Housing Assistance on Labor Supply: Evidence from a Voucher Lottery." *American Economic Review*, 102(1): 272–304.

Jacob, Brian, Max Kapustin, and Jens Ludwig. 2014. "Human Capital Effects of Anti-Poverty Programs: Evidence from a Randomized Housing Voucher Lottery."

Jacobson, Louis S., Robert J. LaLonde, and Daniel G. Sullivan. 1993. "Earnings Losses of Displaced Workers." *The American Economic Review*, 83(4): 685–709.

Jones, Damon. 2015. "The Economics of Exclusion Restrictions in IV Models." National Bureau of Economic Research Working Paper 21391.

Kandel, Eugene, and Edward P. Lazear. 1992. "Peer Pressure and Partnerships." *Journal of political Economy*, 801–817.

Katz, Lawrence F., Jeffrey R. Kling, and Jeffrey B. Liebman. 2001. "Moving to Opportunity in Boston: Early Results of a Randomized Mobility Experiment." *The Quarterly Journal of Economics*, 116(2): 607–654.

Kaur, Supreet, Michael Kremer, and Sendhil Mullainathan. 2010. "Self-Control and the Development of Work Arrangements." *American Economic Review*, 100(2): 624–628.

Kaur, Supreet, Michael Kremer, and Sendhil Mullainathan. 2014. "Self Control at Work." *Journal of Political Economy.*

Kline, Patrick, and Christopher Walters. 2015. "Evaluating Public Programs with Close Substitutes: The Case of Head Start." National Bureau of Economic Research Working Paper 21658.

Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz. 2001. "Bullets Don't Got No Name: Consequences of Fear in the Ghetto." Princeton University, Woodrow Wilson School of Public and International Affairs, Center for Health and Wellbeing. Working Paper 274.

Kling, Jeffrey R., Jens Ludwig, and Lawrence F. Katz. 2005. "Neighborhood Effects on Crime for Female and Male Youth: Evidence from a Randomized Housing Voucher Experiment." *The Quarterly Journal of Economics*, 120(1): 87–130.

Kuziemko, I., R. W. Buell, T. Reich, and M. I. Norton. 2014. "Last-Place Aversion: Evidence and Redistributive Implications." *The Quarterly Journal of Economics*, 129(1): 105–149.

Lochner, Lance, and Enrico Moretti. 2004. "The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports." *American Economic Review*, 94(1): 155–189.

Ludwig, Jens, and Jeffrey R. Kling. 2007. "Is Crime Contagious?" *The Journal of Law and Economics*, 50(3): 491–518.

Ludwig, Jens, Greg J. Duncan, and Paul Hirschfield. 2001. "Urban Poverty and Juvenile Crime: Evidence from a Randomized Housing-Mobility Experiment." *The Quarterly Journal of Economics*, 116(2): 655–679.

Manski, Charles F. 1993. "Identification of Endogenous Social Effects: The Reflection Problem." *The Review of Economic Studies*, 60(3): 531.

Mas, Alexandre, and Enrico Moretti. 2009. "Peers at Work." *American Economic Review*, 99(1): 112–145.

Massey, Douglas S., and Nancy A. Denton. 1993. *American Apartheid: Segregation and the Making of the Underclass.* Harvard University Press.

Moffitt, Robert. 2004. "Policy Interventions, Low-Level Equilibria, and Social Interactions." In *Social Dynamics.* , ed. Steven N. Durlauf and H. Peyton Young.

National Commission, (U.S.). 1992. "Final Report of the National Commission on Severely Distressed Public Housing." Washington, D.C.

Newman, Oscar. 1973. *Defensible Space: Crime Prevention Through Urban Design.* Collier Books.

Olsen, Edgar O. 2003. "Housing Programs for Low-Income Households." In *Means-Tested Transfer Programs in the United States. National Bureau of Economic Research Conference Report*, , ed. Robert Moffitt, 365–442. Chicago; London:The University of Chicago Press.

Olsen, Edgar O. 2014. "Alleviating Poverty through Housing Policy Reform." University of Southern California.

Oreopoulos, Philip. 2003. "The Long-Run Consequences of Living in a Poor Neighborhood." *The Quarterly Journal of Economics*, 118(4): 1533–1575.

Oreopoulos, Philip. 2008. "Neighbourhood effects in Canada: a critique." *Canadian public policy*, 34(2): 237–258.

Oreopoulos, Philip. 2012. "Moving Neighborhoods Versus Reforming Schools: A Canadian's Perspective." *Cityscape*, 207–212.

Orr, Larry L., Judith Feins, Robin Jacob, Erik Beecroft, Lisa Sanbonmatsu, Lawrence F. Katz, Jeffrey B. Liebman, and Jeffrey R. Kling. 2003. "Moving to Opportunity Interim Impacts Evaluation." U.S. Department of Housing and Urban Development.

Park, Sangyoon. 2015. "Socializing at Work: Evidence from a Field Experiment with Manufacturing Workers."

Pinto, Rodrigo. 2015. "Selection Bias in a Controlled Experiment: The Case of Moving to Opportunity."

Popkin, Susan J., Diane K. Levy, Laura E. Harris, Jennifer Comey, Mary K. Cunningham, and Larry F. Buron. 2004. "The HOPE VI Program: What about the residents?" *Housing Policy Debate*, 15(2): 385–414.

Popkin, Susan J., James E. Rosenbaum, and Patricia M. Meaden. 1993. "Labor market experiences of low-income black women in middle-class suburbs: Evidence from a survey of gautreaux program participants." *Journal of Policy Analysis and Management*, 12(3): 556–573.

Popkin, Susan J., Mary K. Cunningham, Erin Godfrey, Beata Bednarz, Alicia Lewis, Janet L. Smith, Anne Knepler, and Doug Schenkleberg. 2002. "CHA Relocation Counseling Assessment Final Report." Urban Institute.

Popkin, Susan J., Michael J. Rich, Leah Hendey, Chris Hayes, Joe Parilla, and George Galster. 2012. "Public Housing Transformation and Crime: Making the Case for Responsible Relocation." *Cityscape*, 14(3): 137–160.

Popkin, Susan J, Victoria Gwiasda, Lynn Olson, Dennis Rosenbaum, and Larry Buron. 2000. *The Hidden War: Crime and the Tragedy of Public Housing in Chicago.* New Brunswick, NJ:Rutgers University Press.

Rosenbaum, James E. 1995. "Changing the geography of opportunity by expanding residential choice: Lessons from the Gautreaux program." *Housing Policy Debate*, 6(1): 231–269.

Rosin, Hanna. 2008. "American Murder Mystery." *Atlantic Monthly*, 40–54.

Sacerdote, Bruce. 2001. "Peer Effects with Random Assignment: Results for Dartmouth Roommates." *The Quarterly Journal of Economics*, 116(2): 681–704.

Sampson, Robert J., and W. Byron Groves. 1989. "Community Structure and Crime: Testing Social-Disorganization Theory." *American Journal of Sociology*, 94(4): 774–802.

Sanbonmatsu, Lisa, Jeffrey R. Kling, Greg J. Duncan, and Jeanne Brooks-Gunn. 2006. "Neighborhoods and academic achievement results from the Moving to Opportunity experiment." *Journal of Human Resources*, 41(4): 649–691.

Sanbonmatsu, Lisa, Jens Ludwig, Lawrence F. Katz, Lisa A. Gennetian, Greg J. Duncan, Ronald C. Kessler, Emma Adam, Thomas McDade, and Stacy Lindau. 2011. "Moving to Opportunity for Fair Housing Demonststration Program: Final Impacts Evaluation." U.S. Department of Housing and Urban Development.

Shroder, Mark D., and Larry L. Orr. 2012. "Moving to Opportunity: Why, How, and What Next?" *Cityscape*, 31–56.

Stevenson, Megan. 2015. "Breaking Bad Social: Influence and the Path to Criminality in Juvenile Jails."

Tincani, Michela M. 2015. "Heterogeneous Peer Effects and Rank Concerns: Theory and Evidence."

Vale, Lawrence J., and Erin Graves. 2010. "The Chicago Housing Authority's Plan for Transformation: What Does the Research Show So Far." *Massachusetts Institute of Technology, Department of Urban Studies and Planning.*

Waldinger, Fabian. 2012. "Peer Effects in Science: Evidence from the Dismissal of Scientists in Nazi Germany." *The Review of Economic Studies*, 79(2): 838–861.

Walters, Christopher R. 2014. "The Demand for Effective Charter Schools." National Bureau of Economic Research Working Paper 20640.

Wilson, William J. 1987. *The Truly Disadvantaged: The Inner City, the Underclass, and Public Policy.* University of Chicago Press.

Zelon, Harvey. 1994. "Survey of Public Housing Residents: Crime and Crime Prevention in Public Housing." Research Triangle Institute, Research Triangle Park, NC.