American Economic Review 2018, 108(10): 3028–3056
https://doi.org/10.1257/aer.20161352

Moved to Opportunity: The Long-Run Effects of Public Housing Demolition on Children[†]

By Eric Chyn*

This paper provides new evidence on the effects of moving out of disadvantaged neighborhoods on the long-run outcomes of children. I study public housing demolitions in Chicago, which forced low-income households to relocate to less disadvantaged neighborhoods using housing vouchers. Specifically, I compare young adult outcomes of displaced children to their peers who lived in nearby public housing that was not demolished. Displaced children are more likely to be employed and earn more in young adulthood. I also find that displaced children have fewer violent crime arrests. Children displaced at young ages have lower high school dropout rates. (*JEL* H75, I38, J13, R23, R38)

Over the past three decades, cities across the United States have spent more than $6 billion on initiatives to demolish public housing in high-poverty areas and provide tens of thousands of displaced residents with housing vouchers (US Government Accountability Office 2007). One justification for this policy is the idea that former public housing residents would benefit from relocating to less disadvantaged neighborhoods. In particular, policymakers and academics have hoped that outcomes of children would improve. Theory suggests that children's odds of success are higher if they move to neighborhoods where most adults are employed and their peers are less likely to engage in criminal activity (Wilson 1987; Sampson and Groves 1989; Massey and Denton 1993).

Despite this substantial change in housing assistance policy, there is relatively little research studying the causal effects of public housing demolition on former

*Department of Economics, University of Virginia, PO Box 400182, Charlottesville, VA 22904 (email: ericchyn@virgina.edu). This paper was accepted to the *AER* under the guidance of Hilary Hoynes, Coeditor. I would like to thank four anonymous referees for detailed comments and suggestions. I am also grateful to Martha Bailey, John Bound, Charles Brown, John DiNardo, Susan Dynarski, Michael Eriksen, Brian Jacob, Jens Ludwig, Jeff Smith, Michael Mueller-Smith, Ed Olsen, Mel Stephens, James Sullivan and Justin Wolfers for their feedback. In addition, this paper benefited from comments from Lasse Brune, Deanna Chyn, Sarah Johnston, Max Kapustin, Jason Kerwin, Johannes Norling, Bryan Stuart, Isaac Sorkin, and seminar participants at University of Michigan, Vanderbilt University, the Sixth Workshop of the Centre for Research Active Labour Market Policy Effects, University of Notre Dame, the W.E. Upjohn Institute, University of Oregon, University of Virginia, University of Texas at Austin, UC Riverside, and the NBER 2016 Summer Institute (Children's Workshop). I acknowledge fellowship support from a NICHD training grant to the Population Studies Center at the University of Michigan (T32 HD007339). I would like to thank Christine Devitt, Brian Jacob, and Robert Goerge for their assistance in obtaining and interpreting the data used in this study. All opinions and any errors are my own. The author declares that he has no relevant or material financial interests that relate to the research described in this paper.

[†]Go to https://doi.org/10.1257/aer.20161352 to visit the article page for additional materials and author disclosure statement.

public housing residents.[1] Jacob (2004) studies academic outcomes for displaced children in the first few years after demolition. His results suggest there was little, if any, short-run impact on academic achievement. However, these findings may not be a good guide for understanding longer-run impacts. Recent studies of several childhood interventions and policies suggest long-run effects may occur due to improvements in noncognitive skills (Garces, Thomas, and Currie 2002; Chetty et al. 2011; Heckman, Pinto, and Savelyev 2013; Carrell, Hoekstra, and Kuka 2016).

This paper sheds new light on the consequences of public housing demolition by providing the first (to the best of my knowledge) causal estimates of the long-run impacts on children. Specifically, I study the case of Chicago where the housing authority began reducing its stock of public housing during the 1990s. Importantly, the authority targeted some 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. My research design compares the young adult outcomes of displaced and non-displaced children from the same public housing development. Because these two groups of children and their households were similar before the demolition, differences in later-life outcomes can be attributed to neighborhood relocation.

Novel administrative data from Illinois make 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. 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 21 percent lower poverty and 42 percent less violent crime relative to non-displaced households.

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 in higher annual earnings: an increase of 16 percent relative to their non-displaced counterparts. In addition to studying labor supply, I find that displaced children have 14 percent fewer arrests for violent crimes in the years following demolition. I also show that children displaced at younger ages are less likely to drop out from high school.

This evidence sheds light on the costs and benefits of demolition of public housing, which has been a major focus of US housing policy. In addition to complementing the short-run analysis provided by Jacob (2004), the positive impacts on displaced children detected in this paper relate to research studying the effect of demolition on neighborhood-level outcomes. Aliprantis and Hartley (2015) and Sandler (2017) use event-study approaches to show that areas near public housing benefited from demolition because crime rates decreased significantly.

More broadly, the results in this paper also contribute to the literature on the effects of neighborhood conditions on children. Recent analysis of the Moving to Opportunity (MTO) experiment shows that voluntary voucher-based relocation programs can have positive benefits for children who were young (below age 13)

[1] See Collinson, Ellen, and Ludwig (2015) for further discussion of US housing policy changes and the decline of project-based housing assistance in recent decades.

when their families moved (Chetty, Hendren, and Katz 2016).[2] The findings in this paper suggest these positive benefits of neighborhood change are not limited to the type of households that volunteered for the MTO experiment. In addition, this paper provides evidence on childhood exposure effects of neighborhood poverty. Similar to Chetty, Hendren, and Katz (2016) and Chetty and Hendren (2016a), I find that there are significant and positive impacts on earnings measured at age 26 for younger displaced children (who moved during ages 7 to 12). These effects are notably larger than the corresponding effects for older children (who moved during ages 13 to 18). At the same time, this subgroup analysis shows that older children experience detectable and positive impacts from relocating from high-poverty neighborhoods.[3]

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 earns $45,000 more in their lifetime ($12,000 in present value).[4,5] 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.

I. History of Public Housing Demolition in Chicago

During the 1990s, Chicago had the third largest public housing system in the United States, providing services to nearly five percent of the city's population (Popkin et al. 2000). The Chicago Housing Authority (CHA) owned and managed 17 housing developments (also known as "projects") that provided homes specifically for families with children. Each project consisted of a collection of apartment buildings built in close proximity. Many of these buildings were large high-rise structures with 75 to 150 housing units.

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.[6] The vast majority of public housing residents during this period in Chicago were African American, and a large share was single-parent, female-headed households.

[2] Note that Sanbonmatsu et al. (2011) also analyzed children who participated in MTO and found no detectable impact on labor market outcomes. The most recent MTO analysis by Chetty, Hendren, and Katz (2016) focuses on younger (below age 13) children who were not old enough to have completed their education at the time that data were collected for Sanbonmatsu et al. (2011).

[3] Chetty and Hendren (2016a) find no detectable impact of relocation for older children in the MTO sample. In Section VIII, I discuss possible explanations for why older children displaced by demolition benefited from moving from high-poverty neighborhoods.

[4] This estimate is based on the assumption that the 16 percent treatment effect on earnings remains constant over the life cycle. See Section IX for a detailed discussion.

[5] This calculation of the benefits from relocating may represent an upper bound since the poverty rate at public housing projects in Chicago was relatively high.

[6] For example, more than 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.

The demolition of public housing in Chicago during the 1990s began as a reaction to serious housing management problems. 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 address these maintenance issues effectively 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.[7] Although the city wanted to eventually eliminate much of the housing stock, funding limitations dictated that only a few demolitions occurred in the 1990s.[8]

As mentioned, the first demolitions in Chicago stemmed from a variety of events and circumstances that were sometimes unforeseen.[9] 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.[10] Similarly, harsh winter weather damaged several buildings in the Henry Horner Homes project, prompting the CHA to close these buildings for demolition.[11] When not reacting to specific crises, the CHA generally sought to close buildings that had the most severe maintenance issues.[12]

When the CHA selected a building for demolition, it provided Section 8 housing vouchers to displaced residents which allowed recipients to rent housing on the private market.[13] Alternatively, the CHA provided residents of affected buildings 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.[14]

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 fortieth 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. Finally, the transition to

[7] 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.

[8] As part of a long-run plan to eliminate high-rise public housing, the CHA would construct new mixed income housing. In practice, the effort to build new housing proceeded slowly and occurred many years after demolition (Hunt 2009).

[9] The analysis in this paper pertains only to building demolitions that displaced individuals in 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 corresponds to the period studied in Jacob (2004).

[10] Melita Marie Garza, "9 High-Rises at Taylor Homes Slated to Close," *Chicago Tribune*, September 10, 1999.

[11] Melita Marie Garza, "CHA Evacuates High-Rise Units without Heat," *Chicago Tribune*, January 12, 1999.

[12] An additional motivation for building closure was related to criminal activity. For example, snipers located on the roof of a Cabrini Green building shot seven-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 (Christine Hawes, "Now Things Move Quickly at Cabrini," *Chicago Tribune*, October 16, 1992). I exclude projects and buildings where these crime-based closures occurred from the analysis in this paper.

[13] Prior to 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).

[14] The CHA provided few support services such as housing counseling during the period of my study. Later CHA initiatives included relocation support for residents who were displaced by building demolitions that occurred during the 2000s (Popkin et al. 2012).

vouchers from public housing should not mechanically affect the income of assisted households because the program and rent rules for vouchers and project-based assistance were similar.[15]

II. 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 who shape norms and social identity (Wilson 1987). Living in a less disadvantaged neighborhood can also expose a child to higher-income peers who, upon reaching working age, can provide job information and referrals.[16] 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.

Although these mechanisms suggest children should benefit from moving to low-poverty neighborhoods, empirical studies provide mixed support for this prediction. For example, an 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).[17] Chetty and Hendren (2016a) use a quasi-experimental approach and find that children benefit from moving to better neighborhoods in terms of earnings, college attendance, fertility, and marriage patterns. 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 (2016) find no evidence of benefits for children who were older when their family moved to a low-poverty area through MTO. In relation to the present paper, the latter results are the most relevant comparison given the paper's similar focus on distressed, high-crime public housing in major US cities.[18] Yet, the results from MTO may differ from the demolition context for two reasons. First, relocation due to demolition may represent a distinct type of treatment. Second, the households displaced by demolition did not volunteer to relocate.

[15] Online Appendix Section A3 provides a list of the rules for housing vouchers.

[16] In addition, living in a lower-poverty area 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.

[17] 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 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) write that it is unclear whether the Gautreaux setting approximated random assignment because there are statistically significant differences in baseline characteristics between the set of participants that moved to the suburbs or within Chicago.

[18] There are notable differences between public housing neighborhoods in the United States and Canada (Oreopoulos 2003, 2008, 2012). For instance, Oreopoulos (2012, p. 208) 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."

Another way in which demolition and relocation may affect children is through changes in the quality of schooling. Several 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; Fryer and Katz 2013; Chetty, Friedman, and Rockoff 2014). Yet, studies of housing assistance programs suggest families typically do not use vouchers to relocate to areas with access to better schools. The MTO evaluation found little impact on child school quality although households moved to areas with notably lower poverty (Sanbonmatsu et al. 2006). Jacob (2004), who studied the short-run impacts of public housing demolition, found little difference in school quality for displaced and non-displaced children.

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

III. 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 CHA and social assistance (i.e., TANF/AFDC, Food Stamps, and Medicaid) case files (1994–1997) from the Illinois Department of Human Services (IDHS) to create a sample of children who lived in public housing and were affected by demolition during the 1990s.[19] I obtain information on baseline (prior to displacement due to demolition) characteristics and long-run outcomes by merging the sample of children with unemployment insurance wage records (1995–2009) from the Illinois Department of Employment Security (IDES), comprehensive arrest records (up to 2009) from the Illinois State Police (ISP), and IDHS assistance files (1989–2009). In complementary research, Chyn, Jacob, and Ludwig (2017) also link the sample of displaced and non-displaced children to records from the Chicago Public Schools (CPS) and the National Student Clearinghouse (NSC) to analyze schooling outcomes.[20] These schooling results are reproduced in Section VI of this paper. For further details on all data sources and sample construction, see the online Appendix.

A. *Sample of Public Housing Buildings*

My analysis focuses on a subset of public housing projects and buildings listed in CHA building inventory records. Specifically, I examine non-senior-citizen projects that experienced demolitions during the initial wave of housing demolitions

[19] Authorities in Chicago continued to demolish buildings throughout the 2000s, but I am unable to study later demolitions due to data limitations.

[20] The data from CPS contain information on high school graduation status.

associated with HOPE VI grants in 1995–1998.[21] I restrict attention to high-rise buildings, defined as having 75 units or more. In general, low- and mid-rise buildings did not experience the same type of abrupt demolition as high-rises. Finally, I exclude projects where evidence suggests that building demolition was correlated with unobserved tenant characteristics. Specifically, I exclude the Cabrini Green and Henry Horner projects.[22]

The final sample contains 53 high-rise buildings located in 7 projects. I obtain the date when a building was closed from Jacob (2004), which determined the closure year by examining CHA administrative data on building occupancy supplemented by qualitative sources.[23] During my study period, there were 20 demolished (treated) buildings and 33 comparison (control) group buildings that did not close during the 1995–2000 period.[24]

B. *Linking Households to the Public Housing System*

To create the analysis sample, I rely on social assistance records that provide exact street addresses for welfare recipients. Specifically, I include welfare recipients who have a street address matching a building in the public housing project sample in the year prior to building closure for demolition. By focusing on addresses in the year *before* building closure, the sample definition is unrelated to any impact that displacement due to demolition has on public assistance participation. Overall, the assistance data contain 5,676 adult recipients who lived in public housing in the year before building closure. Since the sample of public housing buildings contains 7,770 individual apartments, this suggests that the assistance sample covers at least 73 percent of the households living in the demolition sample of buildings (assuming there are no vacant apartments).[25]

Finally, I focus on children who were age 7 to 18 in the year of demolition. With this sample, I observe adult (age $>$ 18) outcomes for at least 3 years and at most 14 years for each child. This age restriction also allows me to compare my results directly to the analysis of children in the final impact evaluation from MTO.[26] The final sample comprises 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 displacement 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 displacement date. For example, residents of

[21] Note that I exclude high-rise projects that did not have 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 identifying the impact of demolition.

[22] Online Appendix Table A3 shows that the main results are robust to including children from the Cabrini-Green and Henry Horner projects in the analysis.

[23] Online Appendix Section A4 provides details on how the year of building closure before demolition is determined.

[24] See online Appendix Section A5 for further details on treatment and control buildings.

[25] 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. Only 15 percent of households living in Chicago public housing had an employed member, and the average CHA household annual income was $6,936 (Popkin et al. 2000).

[26] In online Appendix Table A14, I show that the 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.

projects that had buildings closed for demolition in 1995 will have 14 observations in their panel. I merge this panel with administrative data on labor market outcomes, social assistance receipt, and criminal arrests.

IV. Empirical Approach

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. 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 buildings can be used as a comparison group that holds constant characteristics specific to residents at Robert Taylor Homes. The former is the treatment group that was displaced from public housing. To the extent that displaced and non-displaced individuals were 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 and relocation on outcome Y for children,

$$Y_{it} = \alpha + \beta D_{b(i)} + \psi_{p(i)} + \epsilon_{it}, \tag{1}$$

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 term $\psi_{p(i)}$ is a set of project fixed effects. The dummy variable $D_{b(i)}$ takes a value of 1 if an individual lived in a building slated for demolition. Hence, β represents the net impact of relocation due to demolition on children's outcomes. Since displaced households received vouchers to replace their public housing assistance, the estimates of β capture effects of changing the form of housing assistance.[27] For all analysis using equation (1), I cluster standard errors at the building level.

In addition, I estimate two augmented versions of equation (1). First, I estimate a model that includes interactions for the sex of children. This analysis is motivated by a large body of previous empirical work that documents significant heterogeneity by sex. For example, many of the benefits detected in the MTO evaluations were found for girls but not for boys (Kling, Ludwig, and Katz 2005). 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 explore how treatment effects evolve over time by estimating a model that interacts an individual's age in the year that an outcome is measured and the treatment indicator.[28] This specification provides estimates of the difference in outcomes between

[27] This contrasts with analysis by Jacob, Kapustin, and Ludwig (2015) which studied the effects of receiving housing vouchers for households that were not previously receiving housing assistance. Andersson et al. (2016) also study a parameter that differs from the present paper by estimating the impact of each year spent living in voucher or project-based housing.

[28] Specifically, I estimate $Y_{it} = \sum_{j=19}^{J} \alpha_j D_{i,b} \, \mathbf{1}(age_{i,t} = j) + X_i'\theta + \psi_p + \delta_t + \epsilon_{it}$. Here j indexes the age for an individual in year t, and J is the maximum age for an individual in the sample. The term δ_t is a year fixed effect, and

displaced and non-displaced children for each age at which outcomes are measured in the sample. I separately estimate this model on the samples of children who were young (age 7 to 12) and relatively old (ages 13 to 18) at the time of relocation. This approach is based on Chetty, Hendren, and Katz (2016), which found notable benefits of relocation for children who were young (less than age 13) when their families moved in a long-run analysis of the MTO demonstration. More broadly, several recent studies have found additional evidence that effects of relocation vary based on the age at which children move (Chetty and Hendren 2016a, b).[29]

Estimates of β have a causal interpretation if the CHA's selection of buildings for demolition was unrelated to resident characteristics. The historical evidence suggests this condition is plausible because 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 restricted the ability of households to sort into different buildings. Recall that most families spent years on the public housing waiting list and accepted the first unit that became available. Finally, Section IVB provides empirical support for this assumption that building demolition was unrelated to resident characteristics by showing that displaced and non-displaced residents in my sample have similar observed characteristics in the year prior to building closure for demolition.

A. *The Effect of Moving Out of Project-Based Public Housing*

As an alternative to the reduced-form analysis of demolition and relocation, I estimate the causal impact of living in public housing by using building demolition as an instrumental variable. These results represent the "dose effect" of spending an additional year in high-rise public housing, which is a parameter that may be of interest to policymakers. For this analysis, I use the following two-stage least squares system (2SLS),

$$P_i = \gamma + \tau D_{b(i)} + \psi_{p(i)} + \eta_{it}, \tag{2}$$

$$Y_{it} = \pi + \theta P_i + \psi_{p(i)} + \epsilon_{it}, \tag{3}$$

where the dependent variable in the first stage P_i is the number of years spent living in project-based public housing (including the period prior to demolition). The usual concern in this setting is that ordinary least squares (OLS) estimates of equation (3) will be biased because households selectively participate in public housing. The demolition setting allows me to address this issue by providing a source of variation in the number of years spent living in public housing that is plausibly uncorrelated with a child's unobserved characteristics.

X_i' is a set of individual level controls, including the main effects for age.

[29] The question of whether the timing of treatment matters is also considered in studies of nonhousing assistance programs. For example, Hoynes, Schanzenbach, and Almond (2016) find children exposed to Food Stamps when very young (in utero through early childhood) benefit notably.

TABLE 1—COMPARISON OF DISPLACED AND NON-DISPLACED CHILDREN AND ADULTS AT BASELINE (*Prior to Demolition*)

	All children		Male children		Female children		Adults	
	Control mean (1)	Difference: treated–control, within estimate (2)	Control mean (3)	Difference: treated–control, within estimate (4)	Control mean (5)	Difference: treated–control, within estimate (6)	Control mean (7)	Difference: treated–control, within estimate (8)
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)
Teen mom (= 1)†							0.371	−0.018
								(0.024)
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)
School outcomes								
Enrolled (= 1)	0.948	0.003	0.946	−0.009	0.949	0.014		
		(0.015)		(0.017)		(0.016)		
Reading score (SD)	−0.443	0.024	−0.477	−0.045	−0.410	0.074		
		(0.074)		(0.087)		(0.074)		
Math score (SD)	−0.449	0.048	−0.509	0.007	−0.393	0.073		
		(0.061)		(0.077)		(0.065)		
Economic activity								
Employed (= 1)							0.173	0.006
								(0.016)
Earnings‡							$1,493.75	−$45.91
								(193.358)
Observations (individuals)		5,250		2,547		2,703		4,331

Notes: Children are age 7 to 18 at baseline while adults are over age 18. The control mean statistics in columns 1, 3, 5, and 7 refer to the averages for non-displaced individuals. For each outcome (row), I compute the difference between displaced (treated) and non-displaced individuals using equation (1). This difference is reported in columns 2, 4, 6, and 8. Standard errors are presented below each estimate and are clustered at the building level. †Outcome is only defined for women. ‡Data on employment begin in the first quarter of 1995. For individuals who experience displacement in 1995, I use this quarter of earnings (scaled to an annual figure) to measure earnings prior to displacement because this quarter precedes demolition. See text for details on data sources.

B. *Comparing Treated and Control Individuals 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 comparison group (control) buildings. Specifically, I examine characteristics measured in the (baseline) year prior to building closure for demolition.

Table 1 compares children living in treated and control buildings by estimating a regression model where the dependent variable is a child characteristic measured in the year before demolition. 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 comparison group 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 0. The table shows that the mean difference is never statistically different from 0 for all measures of past criminal activity and demographics in my sample.[30] In addition, Table 1 shows there is no difference in schooling outcomes before demolition. Columns 3–6 similarly show no detectable difference between displaced and non-displaced youth by sex.

I also test for differences in the pre-demolition (baseline) characteristics of adult (age $>$ 18) public housing residents. Table 1 examines baseline characteristics of adults in columns 7 and 8. Similar to the analysis for children, column 8 reports the difference between displaced and non-displaced adults. Reassuringly, adults living in demolition buildings do not appear statistically different in terms of past criminal or labor market activity. Adults in treated buildings are almost one year older, but the magnitude of this difference is small relative to the mean adult age.[31]

C. *Testing for Attrition and Spatial Spillovers*

Administrative data allow me to avoid many concerns over sample attrition and missing data. 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. However, a concern is that my estimates will be biased if displaced children are more likely to move out of state. In this case, the 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.[32]

To address concerns over attrition, I follow Grogger (2013) and use terminal runs of zeros to measure permanent out-of-jurisdiction attrition. The idea is that attrition has a distinctive pattern: when individuals move out of state, all of their subsequent entries in administrative panel data from their original location are zeros. Online Appendix Section A2 uses this measure of attrition and tests 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.

[30] The juvenile arrest data come from the Illinois State Police (ISP). Prior to 1998, the arrest data for juveniles in the ISP data are 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.

[31] In additional tests, I examined move-out rates for residents of demolished and comparison group buildings using addresses measured in the social assistance case files. I find no evidence that families are systematically moving out of demolished buildings in the years leading up to demolition.

[32] There are at least two reasons attrition due moving may not be much of a concern for the analysis. First, an examination of National Student Clearinghouse data shows that 3.5 percent of the demolition sample ever attends a two- or four-year out-of-state institution. Moreover, there is no detectable difference between displaced and non-displaced children in the out-of-state attendance rate. Further details on analysis of schooling outcomes are provided in Section VI. Second, Jacob, Kapustin, and Ludwig (2015) provide evidence that children from low-income households in Chicago have very low rates of mobility out of Illinois. Specifically, they study children in Chicago whose households lived in private market housing and won a housing voucher lottery. In their analysis, they track children using detailed address data from National Change of Address (NCOA) registry and national credit bureau checks. They find 95 percent of children and their households were still living in Illinois after nearly ten years.

A final concern for the empirical results is spatial spillovers stemming from demolition. In other words, the control group of non-displaced children could be affected by the demolition of neighboring buildings and the relocation of their peers. This would bias estimates upward if non-displaced children are worse off due to demolition. To test for the existence of spillovers, I augment equation (1) with additional indicators for living in a comparison group building that is immediately adjacent to a demolition building. The omitted group in this case are children living in comparison group buildings located farther away from a demolished building. This specification tests for spillovers on comparison group buildings under the assumption that social interactions between buildings within a project decrease with distance. Online Appendix Table A2 presents the results of this specification and shows that there is no evidence of this type of spillover for labor market and welfare outcomes.

There also could be general spillovers at the project level. For example, demolition may have reduced social cohesion in the comparison group buildings that were not demolished from 1995 to 1998. The literature on the effects of demolition on neighborhood-level crime provides insight on this type of general spillover. As noted in the introduction, Sandler (2017) and Aliprantis and Hartley (2015) show Chicago's demolitions reduced crime in areas around public housing. To the extent that reductions in neighborhood crime have positive impacts on long-run child outcomes, this would tend to improve outcomes for children in the comparison group buildings. In this way, the existing literature provides evidence of positive spillovers on the comparison group, which would bias my estimates toward zero.

V. Main Results

A. *Effects of Demolition on Household Location*

Housing vouchers increase housing location choice (relative to project-based housing assistance), and this increased choice may be important if families live in public housing located in disadvantaged urban areas. For public housing in my Chicago sample, the Census tract 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 US population had income below the poverty line (Bishaw 2014).

I test whether displaced public housing residents moved to lower-poverty neighborhoods using address information from social assistance case records. The primary concern with this analysis of post-demolition location is that address data are only available if a household received social assistance such as AFDC/TANF, Food Stamps, or Medicaid. Hence, this analysis may be biased if demolition has an impact on a household's participation in social assistance programs.[33] Reassuringly,

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

Table 2—Impact of Demolition on Household Neighborhood Characteristics

	3 years after demolition		8 years after demolition	
	Control mean (1)	Difference: treated–control, within estimate (2)	Control mean (3)	Difference: treated–control, within estimate (4)
HH has address (= 1)	0.777	0.014	0.656	0.011
		(0.021)		(0.020)
Only HHs with address				
Tract characteristics:				
Black (percent)	94.897	−2.801	90.042	−1.055
		(1.125)		(1.257)
Below poverty (percent)	64.208	−14.264	40.858	−2.771
		(2.729)		(2.353)
Violent crime rate	68.855	−29.522	30.801	−2.371
		(5.807)		(4.714)
Observations (HHs)		2,767		2,767
Observations (HHs with address)		2,162		1,824

Notes: 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 are reported in columns 2 and 4 as computed from a regression specified in equation (1). This analysis follows households regardless of whether a child is still present. Robust standard errors are clustered at the public housing building level. See Section III for further description of data sources.

the first row in Table 2 shows that there is no detectable difference in the likelihood that displaced households with children (age 7 to 18) are observed in the social assistance data with an address in the third or eighth year after demolition and relocation.[34]

The last two rows of Table 2 show that displaced (treated) households moved to better quality neighborhoods relative to their non-displaced (control) peers. Column 2 shows that three years after displacement the treated households with children lived in Census tracts that have 21 percent lower poverty relative to control households. Their neighborhoods also had less crime: treated households lived in neighborhoods with about 29 (42 percent) fewer violent crimes per 10,000 residents. Overall, these effects on neighborhood relocation are similar to those reported in Jacob (2004), who relied on an alternative administrative source to examine changes in children's location.[35]

These results also show that differences in neighborhood conditions became smaller over time. In terms of neighborhood characteristics measured eight years after demolition, column 3 of Table 2 shows that there is much less contrast between displaced and non-displaced households. Online Appendix Figure A1 provides further details on differences over time by plotting treatment effects on neighborhood characteristics in each post-displacement year. Across neighborhood characteristics,

[34] Panel A of online Appendix Figure A2 provides estimates of the difference in the probability that a household has an address in each year after demolition and relocation. These results show that there is never any detectable difference in the probability of observing an address for displaced households.

[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.

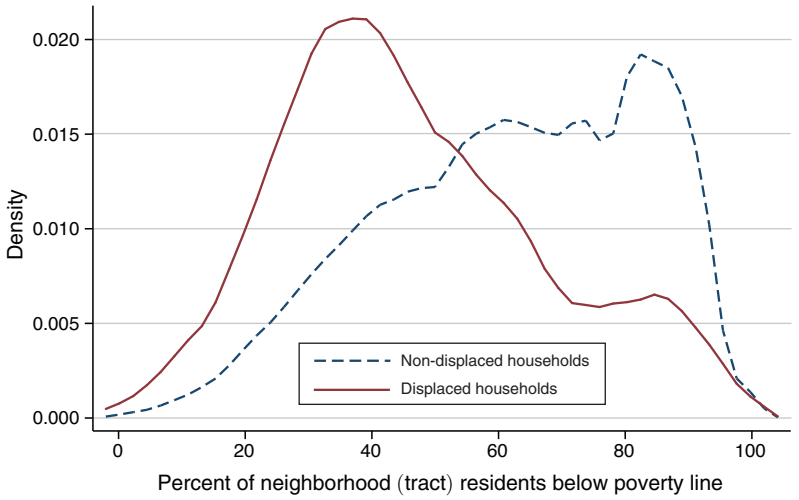

FIGURE 1. DENSITY OF NEIGHBORHOOD POVERTY AFTER DEMOLITION

Notes: The figure shows statistics for the duration-weighted average poverty rate for each household in the sample ($N = 2{,}767$). I compute the average over all locations for the household regardless of whether a child is still present.

the difference is largest in the first year after displacement and attenuates notably over time. This pattern occurs because control households gradually move from public housing. This is reflected in panel B of online Appendix Figure A2 which shows differences in the probability of living in public housing in the post-demolition years. Eight years after displacement, there are no detectable differences between displaced and non-displaced households.[36]

To characterize the cumulative impact of demolition on neighborhood conditions, Figure 1 presents densities of duration-weighted neighborhood (Census tract) poverty rates.[37] Specifically, the poverty rates in the figure are averages over all the locations at which a household lived since displacement. 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 the comparison group 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 who were randomly selected to be part of the unrestricted (Section 8) treatment group (Kling, Ludwig, and Katz 2005; Sanbonmatsu et al. 2011).

[36] It does not appear that mobility of the comparison group children over time is due to subsequent public housing demolition in Chicago that occurred throughout the 2000s. Based on my analysis of residency in the post-demolition period, it appears that most comparison children moved from their baseline address well before demolition of that building.

[37] When a household does not have an active social assistance case in a given year, no address is observed. For the duration weighting, I consider only years in which an address (and poverty rate) is observed for a given household.

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

	Control mean (1)	Difference: treated–control, within estimate (2)
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)
Observations		35,382
Individuals		5,246

Notes: The control mean statistic in column 1 refers to averages for non-displaced individuals. The mean difference between displaced and non-displaced children is reported in column 2 and is computed from a regression specified in equation (1). Robust standard errors are clustered at the public housing building level. All monetary values are in 2012 dollars. See Section III for further description of data sources.

B. *Effects on Labor Market Activity*

Table 3 examines the impact of demolition on children's adult labor market outcomes by presenting results from equation (1). The point estimates 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 young adult working years (age > 18). On average, children who were displaced are 4 percentage points (9 percent) more likely to be employed and earn $600 (16 percent) more annually.[38] Although I do not directly measure hours worked, Table 3 also shows that the probability of earning more than $14,000, the equivalent of working full-time (35 hours a week) at $8 per hour for 50 weeks, increases by 1.3 percentage points (13 percent). Overall, these results show that demolition and relocation is strongly associated with better adult labor market outcomes for children. Online Appendix Tables A3–A5 show that the main results are not sensitive to the inclusion of controls.

Table 4 presents estimates from a modified version of equation (1) that allows treatment effects to vary by sex.[39] The point estimates show that the positive impact detected for the full sample is driven mainly by girls. Relative to their 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 estimates for all outcomes are positive.[40]

[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, I consider their earnings to be zero. All monetary values are in 2012 dollars.

[39] Online Appendix Figure A3 and Tables A6 and A7 further explore heterogeneity by estimating quantile treatment effects. The pattern of the point estimates shows a notable degree of heterogeneity in the earnings response, with the treatment effects generally increasing for higher quantiles.

[40] Clampet-Lundquist et al. (2011) discuss several hypotheses to explain why relocation programs may generate larger benefits for girls.

TABLE 4—IMPACT OF DEMOLITION ON ADULT LABOR OUTCOMES OF CHILDREN BY SEX

	Males		Females	
	Control mean (1)	Difference: treated–control, within estimate (2)	Control mean (3)	Difference: treated–control, within estimate (4)
Employed (= 1)	0.325	0.017	0.505	0.066
		(0.019)		(0.014)
Employed FT (= 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)	$9,055.43	$552.21	$8,739.53	$609.26
		(439.299)		(274.111)
Observations		16,876		18,506
Individuals		2,546		2,700

Notes: 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 specified in equation (1). FT stands for full-time. Robust standard errors are clustered at the public housing building level. See Section III for further description of data sources.

I also explore how the effects vary based on the age at which children were displaced (treated) by public housing demolition. To do this, I divide the sample into children who were young (ages 7 to 12) and old (ages 13 to 18) at baseline. Online Appendix Table A8 shows there are positive and significant impacts for both younger and older children. For example, the impacts on earnings for younger and older children are $583 and $588, respectively. This latter result differs from Chetty, Hendren, and Katz (2016) which tends to find negative estimates of the impact of relocation for MTO children who were older (age 13 to 18) when their families moved.

One concern with this approach of estimating effects for subgroups based on age of relocation is that earnings steeply rise in the mid-to-late twenties as children complete education and enter the labor force (Haider and Solon 2006). This is an important consideration given that the youngest children who relocated are followed only until they are age 26. This data limitation implies that the analysis for younger children may understate the impact of relocation if treatment effects of relocation rise with age, as shown in Chetty, Hendren, and Katz (2016).

To better test whether the effects of relocation vary based on the age at which children move, I examine the trajectory of impacts on employment and earnings by estimating a specification which interacts an individual's age at the time an outcome is measured (hereafter, age of measurement) and the treatment indicator.[41] Figure 2 examines the evolution of treatment effects on earnings separately for young (age 7 to 12) and older children (age 13 to 18). This analysis reveals two important findings. First, the treatment effects for older children (diamond-shaped) are always positive and show little trend over time. Second, the analysis of younger children reveals that there is an increase in the size of the treatment effect at older

[41] For example, I am able to observe adult earnings data for children who were young (age 7 to 12) at the time of relocation from ages 19 to 26. Hence, I create interactions for each age from 19 to 26 and interact this with the treatment indicator, which allows me to compare displaced and non-displaced children over different adult ages.

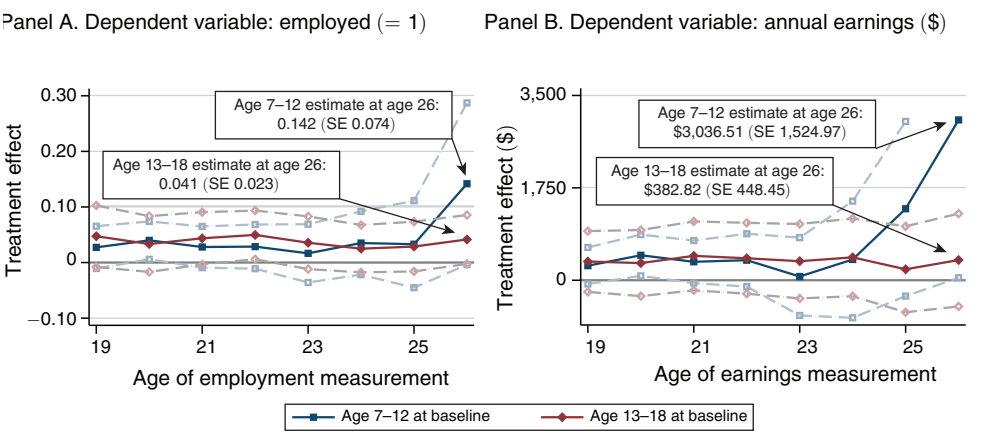

FIGURE 2. IMPACT ON EMPLOYMENT AND EARNINGS BY AGE OF MEASUREMENT

Notes: Each point on the figure is an estimate from the following model:

$$Y_{it} = \sum_{j=19}^{26} \alpha_j D_{i,b} \, \mathbf{1}(age_{i,t} = j) + X_i'\theta + \psi_p + \delta_t + \epsilon_{it},$$

where i, t, b, and p index individuals, years, buildings, and projects, respectively. See Section IV for further details.

ages. When earnings are measured at age 26, panel B shows the effect is \$3,036 (p-value $= 0.05$) for children displaced at young ages. For earnings, a test of the hypothesis that the effects of relocation at age 26 for younger children are the same as for older children is rejected with $p < 0.10$.[42] Overall, this evidence is consistent with recent research showing that the magnitude of benefits from relocating to better neighborhoods depends on the length of exposure to such environments (Chetty, Hendren, and Katz 2016; Chetty and Hendren 2016b).[43]

C. *Effects on Social Assistance and Crime*

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 US 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). Using assistance records linked to the sample for post-demolition years up to 2009, I test whether there is any impact of relocation on participation in different types of public assistance.

[42] Figure 2 confirms that the results in online Appendix Table A8 mask substantively large effects for younger children by pooling large impacts at age 26 with relatively small treatment effects at earlier ages. At the same time, the estimates for older children in online Appendix Table A8 are also slightly larger than the results in Figure 2 because treatment effects grow when these children enter their thirties.

[43] In terms of the impact of demolition on neighborhood characteristics, estimates for households with younger children (age 7 to 12) are slightly larger in magnitude compared to those for households with older (age 13 to 18) children. Online Appendix Table A9 shows demolition and relocation reduced neighborhood (Census tract) poverty rates (measured three years after displacement) by 15 and 12.5 percentage points for households with young and older children, respectively.

TABLE 5—IMPACT OF DEMOLITION ON CRIME OF CHILDREN

	All		Males		Females	
	Control mean (1)	Difference: treated–control, within estimate (2)	Control mean (3)	Difference: treated–control, within estimate (4)	Control mean (5)	Difference: treated–control, within estimate (6)
Number of arrests						
Violent	0.072	−0.010	0.106	−0.017	0.039	−0.004
		(0.004)		(0.006)		(0.005)
Property	0.034	0.006	0.041	0.009	0.028	0.003
		(0.003)		(0.006)		(0.003)
Drug	0.103	−0.005	0.193	−0.016	0.018	0.005
		(0.011)		(0.018)		(0.008)
Other	0.154	−0.25	0.268	−0.037	0.046	−0.014
		(0.011)		(0.015)		(0.008)
Observations		56,629		27,246		29,383
Individuals		5,250		2,547		2,703

Notes: The control mean statistic in column 1 refers to averages for non-displaced individuals. The mean difference between displaced and non-displaced children in columns 2, 4, and 6 are computed from the regression specified in equation (1). Robust standard errors are clustered at the public housing building level.

Online Appendix Table A10 presents results from equation (1) in column 2, which shows no detectable impact of demolition and relocation on utilization of AFDC/TANF, Food Stamps, or Medicaid services across years. The estimates in column 2 are generally very small (less than or equal to 0.01), and the confidence intervals generally rule out effects larger than negative or positive 3 percentage points. Columns 4 and 6 present the effects on social assistance by sex, which show no 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) children 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 Food Stamps ($25,000), TANF ($7,000), and Medicaid ($26,000).

I also use data on arrests to explore the effects of relocation due to public housing demolition on criminal behavior. This analysis is motivated by the fact that displaced households relocated to areas with notably lower rates of crime. Research using Danish data by Damm and Dustmann (2014) suggests this reduction in childhood exposure to crime should have long-run impacts on criminal behavior.

Table 5 reports results from estimating equation (1) in which the dependent variable is a measure of the annual number of arrests and the sample includes all years over the entire post-demolition period. The results in column 1 show youth who relocated have 14 percent fewer arrests for violent crimes. The results for males and females in columns 3 and 4 show this decrease is larger for males. These findings on violent crime are particularly striking in light of the fact that the relocation effects on arrests may be biased upward due to a higher probability of arrest in less

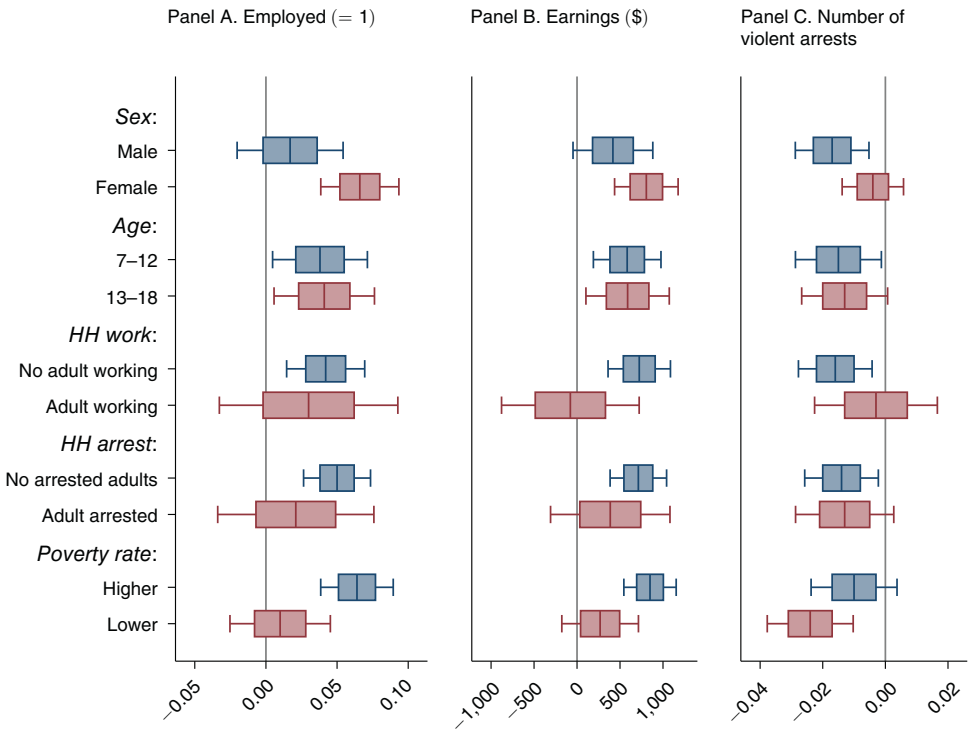

FIGURE 3. IMPACT OF DEMOLITION BY SUBGROUP

Notes: Rows present box and whisker plots for effects estimated separately for subgroups defined by baseline characteristics. See text for further details.

disadvantaged neighborhoods (Kling, Ludwig, and Katz 2005).[44,45] This variation across neighborhoods in the probability of arrest is important to keep in mind in light of the fact that column 2 of Table 5 shows that displaced children have more arrests for property crimes than their non-displaced peers. Interestingly, Kling, Ludwig, and Katz (2005) also observed an increase in property crime for boys whose household moved to a low-poverty neighborhood through the MTO program.

D. *Effects by Subgroup*

I also examine additional types of heterogeneity in the response to demolition. The rows of Figure 3 summarize effects for various subgroups defined in terms of baseline characteristics. The estimated difference between displaced and non-displaced individuals is the center (line) in each box. The top and bottom of each box represent effects that are one standard error above and below the point estimate. The whiskers

[44] Specifically, consider the following equation relating arrests A, criminal behavior C, and the probability of arrest P: $A = C \times P$. Holding constant criminal behavior, we would expect that living in a less disadvantaged neighborhood, with better policing, would result in more arrests.

[45] Kling, Ludwig, and Katz (2005) provide evidence that quality of local policing is higher in the neighborhoods where MTO households relocated.

display the 95 percent confidence interval. The top rows reproduce the results by sex and baseline age for the sake of comparison.[46]

There are two main findings in this subgroup analysis. First, the point estimates for labor market outcomes are almost always positive, and estimates for the number of violent arrests are consistently negative. This pattern suggests that children benefit from relocation regardless of their background characteristics. Second, the results tend to suggest that treatment effects are larger for children from relatively more disadvantaged circumstances. Effects for children from households with no working adults are larger relative to effects for children from households that have at least one working adult. Children who lived at baseline in public housing projects with relatively higher poverty rates (above 70 percent) have larger improvements in both employment and earnings.[47]

E. *The Impact of Living in Public Housing on Labor Market Outcomes*

The reduced-form analysis shows that displaced children have notable improvements in labor market outcomes in young adulthood relative to their non-displaced peers. As discussed in Section IV, it is also possible to use demolition and displacement as an instrument for the number of years spent living in public housing. With this first stage, I can estimate the impact of each year spent living in public housing and being exposed to the associated poverty and crime in the surrounding area.

Table 6 provides results from estimating equations (2) and (3). Panel A shows that displaced children lived in public housing for about 2.6 fewer years relative to their non-displaced peers. This implies that demolition reduced the total time spent living in public housing (including pre-demolition years) by 36 percent. The second-stage estimates in panel B show that each additional year spent living in public housing reduces labor market participation and annual earnings by about 2 percentage points and $277, respectively. As a point of comparison, Chetty, Hendren, and Katz (2016) estimate that each year of reduced childhood exposure to a better neighborhood decreases the intention-to-treat (ITT) effect of experimental vouchers in the MTO demonstration by $364.[48]

VI. Mediating Mechanisms

Why does demolition have a large impact on the young adult labor market outcomes of children? In addition to the mechanisms described in Section II, 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, Online Appendix Table A11 explores whether there is any impact of demolition on labor market outcomes of parents.[49] Column 2 shows that the point estimates are never statistically different from zero. The effect on labor market participation is particularly small and represents less than a 1 percent impact ($= 0.004/0.489$).

[46] Note that online Appendix Table A8 reports the point estimates for labor market outcomes.

[47] Note that I cannot reject the equality of treatment effects across these different subgroups.

[48] Specifically, see Table 8 of Chetty, Hendren, and Katz (2016), which reports results from interacting treatment status (receiving a voucher) with the age at move.

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

Table 6—The Effect of Living in Public Housing

	Control mean (1)	Difference: treated–control, within est. (2)
Panel A. First stage		
Years with PH Address	6.84	−2.634
		(0.466)
Observations		5,250
Individuals		5,250

	Control mean (1)	Difference: treated–control, within est. (2)	2SLS (3)
Panel B. Labor market outcomes			
Employed (= 1)	0.419	0.04	−0.019
		(0.014)	(0.012)
Earnings	$3,713.00	$602.27	−$277.36
		(153.915)	(162.431)
Observations		35,382	35,382
Individuals		5,246	5,246

Notes: The control mean statistics in column 1 refer to averages for non-displaced children. The mean difference between displaced and non-displaced children in column 2 is computed from the regression specified in equation (1). The 2SLS results in column 3 are estimates obtained from estimating equations (2) and (3). See Section III for further description of data sources.

Overall, these results are consistent with the analysis of the MTO experiment that found no detectable impact of housing vouchers on adult outcomes (Sanbonmatsu et al. 2011).

Another possible explanation is that living in a neighborhood with less crime affects teenage criminal behavior thereby influencing adult labor market outcomes by reducing the likelihood of incarceration. Damm and Dustmann (2014) provide support for this idea using Danish data and showing that children living in areas with a higher share of youth criminals are more likely to commit crime when they grow older. Online Appendix Table A12 tests this idea by estimating equation (1) in which the outcome is the number of arrests per year, and I restrict the analysis to years when an individual is between 13 and 18 years old. The lack of detectable decreases in teenage arrests suggest the positive impact of relocation on labor supply does not arise from reductions in adolescent criminal behavior.

Finally, as discussed in Section II, demolition could change long-run child outcomes by affecting schooling. 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. He finds that displaced children are not enrolled in better quality schools after demolition, and there is no detectable impact on test scores or grades. To better understand the longer-run effects of demolition, Chyn, Jacob, and Ludwig (2017) revisit the effects of demolition and relocation on schooling outcomes. Their analysis is motivated by the fact that Jacob (2004) could not study effects on high school dropout or enrollment in higher education for children displaced at young ages.[50]

[50] Jacob (2004) had data on student outcomes up to the 2001–2002 school year. He could not analyze high school dropout rates for young displaced children because this group would still be attending school at the end of his sample.

Table 7—Impacts on High School Graduation and Postsecondary Schooling

	Control mean (1)	Difference: treated–control, within est. (2)
Panel A. Children age 7 to 12 at baseline		
HS dropout (= 1)	0.631	−0.051
		(0.029)
Attend two-year post-sec (= 1)	0.149	0.042
		(0.026)
Individuals		2,429
	Control mean (1)	Difference: treated–control, within est. (2)
Panel B. Children age 13 to 18 at baseline		
HS dropout (= 1)	0.636	−0.021
		(0.046)
Attend two-year post-sec (= 1)	0.095	0.008
		(0.022)
Individuals		1,685

Notes: This table reproduces analysis of the impact of demolition on long-run schooling outcomes from Chyn, Jacob, and Ludwig (2017). The control mean statistics in column 1 refers to averages for non-displaced individuals. The mean difference between displaced and non-displaced children reported in column 2 is estimated from equation (1).

Table 7 reproduces their results for impacts on the likelihood of dropping out of high school and attending a two-year postsecondary educational institution. Panel A shows that children displaced at young ages (age 7 to 12) are 5.1 percentage points (8 percent) less likely to drop out from high school. In addition, their analysis provides suggestive evidence of a positive impact on the probability that young displaced children attended a two-year postsecondary educational institution. In terms of high school dropout status and enrollment in higher education, panel B shows no detectable effects for children displaced at older ages. This latter result suggests the labor market benefits for older (age 13 to 18) children found in this paper are not due to improvements in schooling.

VII. Multiple Comparisons

One concern for my results is how to manage the risk of 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 recommended practices to adjust per-comparison *p*-values to account for multiple outcomes (Anderson 2008). To start, I specify a limited set of outcomes for my main, confirmatory analysis. Specifically, I focus on four outcomes: (i) labor market participation; (ii) annual earnings; (iii) use of social assistance (i.e., AFDC/TANF, Food Stamps, or Medicaid); and (iv) 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 adjusted *p*-values in column 4 of online Appendix Table A13 show that the main conclusions of my demolition

analysis do not change based on an examination of adjusted *p*-values to account for testing multiple outcomes.

VIII. Discussion and Comparison with Previous Studies

Studies of the MTO experiment provide some of the most credible evidence of the impacts of relocating children from disadvantaged neighborhoods. The MTO program provided housing vouchers to families from public housing in severely disadvantaged areas in five major US cities. The experiment randomly assigned each family to one of three groups: (i) a control group that received no vouchers through MTO; (ii) a treatment group that received standard Section 8 housing vouchers that could be used to subsidize private market housing; and (iii) an "experimental" 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).

As mentioned in Section V, one consistent finding in the MTO analysis and the results in this paper concerns the trajectory of treatment effects for children who move from disadvantaged areas. Chetty, Hendren, and Katz (2016) provide estimates of the long-run impacts on children who were younger than age 13 when their families moved through MTO. They find that the treatment effects of the experimental housing voucher are larger when children are in their late twenties. This finding is consistent with Figure 2, which shows the reduced-form impact of demolition and relocation on earnings is largest at age 26 for young displaced children.

At the same time, the positive impacts of relocation due to public housing demolition contrast with analysis of the MTO children whose household received a standard Section 8 housing voucher. Figure 4 compares estimates of the treatment-on-the-treated (TOT) effect of standard Section 8 housing vouchers in MTO and the reduced-form impacts of public housing demolition and relocation from Table 3 of this paper. The TOT estimate from MTO captures the impact of using a Section 8 housing voucher to relocate from public housing. Although I cannot estimate this parameter due to data limitations, the reduced-form impact of demolition represents an ITT effect of vouchers since displaced households received Section 8 voucher offers. In addition, one can interpret the ITT estimate in this paper as a TOT estimate of voucher use if all displaced households used a voucher and none of the non-displaced households did.

Panels A and B of Figure 4 show estimates and confidence intervals of the effects on adult labor market employment and earnings for children aged 7 to 18 at baseline in each study. Note that I use the MTO estimates as reported in Sanbonmatsu et al. (2011) because the age of children in their sample best aligns with the ages observed in the demolition sample.[51] The figure shows these MTO estimates for standard Section 8 vouchers are negative and fall outside of the 95 percent confidence interval around the demolition estimate.

Why might estimates of the impact of demolition differ from what one might expect based on the analysis of standard Section 8 vouchers in the MTO study? One

[51] Specifically, children affected by demolition are ages 21 to 32 at the end of my sample. Similarly, the sample used in Sanbonmatsu et al. (2011) examines "grown" children who were aged 21 to 31 by December 31, 2007.

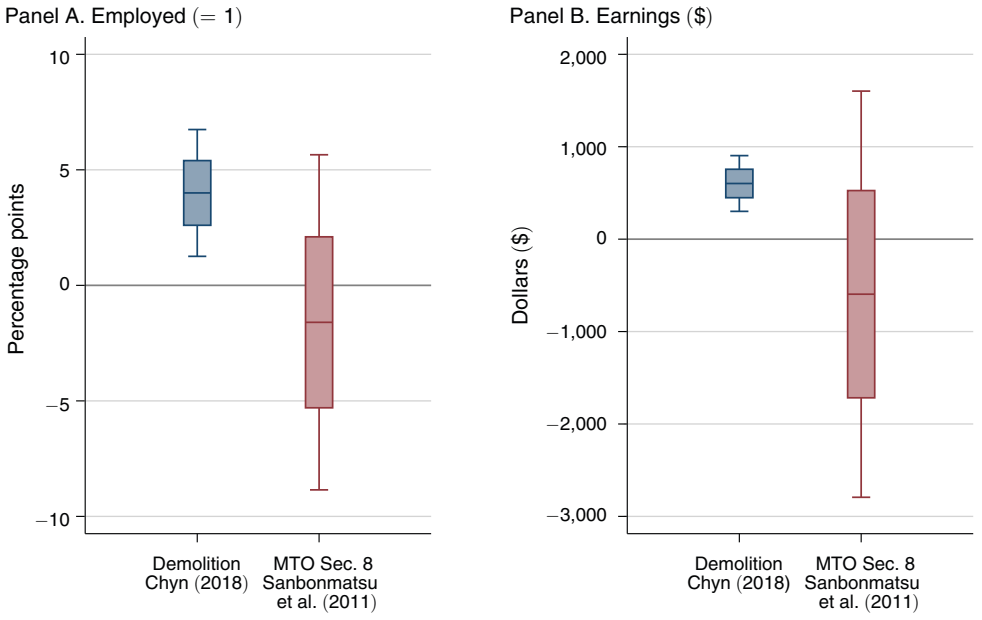

FIGURE 4. EFFECTS ON CHILDREN ACROSS STUDIES

Notes: Panel A shows box and whisker plots of the effects on adult labor market employment for children (age 7 to 18 at baseline) from different studies. Panel B similarly compares the effects on adult labor market earnings. See Section VIII for details.

possibility stems from the fact that public housing demolition had different effects on neighborhood quality relative to the MTO experiment. Prior to demolition, the average public housing household in my sample lived in a Census tract with a poverty rate of 83 percent. In contrast, MTO households lived in neighborhoods where the mean poverty rate (prior to randomization) was 56 percent (Orr et al. 2003). This difference in exposure to poverty may matter if there is a nonlinear relationship between neighborhood conditions and child outcomes. The results in Figure 3 support this idea. Children who relocated from public housing projects where the Census tract poverty rate was higher than 70 percent have much larger treatment effects on labor market outcomes relative to their peers who lived in public housing projects with lower poverty rates.

Another possible explanation is that there may be different effects of relocation for the type of household affected by public housing demolition. Estimates based on studying displaced households represent effects for a general population of low-income households because families had no ability to control whether demolition affected them. In contrast, the effects from the MTO evaluation speak to the effects of relocation for motivated households opting to participate in a voluntary relocation program. This distinction has importance given that 25 percent of eligible families at the five MTO sites volunteered (Goering et al. 1999). This could be important because research on other public programs suggests treatment effects vary widely across the population. For example, Kline and Walters (2016) find that Head Start generates larger test score gains for children who were the least likely to

enroll in the program. In addition, Bitler, Hoynes, and Domina (2014) study Head Start, finding significant variation in the effects across subgroups.

Finally, a third explanation for differences in treatment effects stems from the possibility that children in MTO (especially older youth) may have returned to their original neighborhood to visit friends after moving. Orr et al. (2003) report that 57 percent of youth age 12 to 19 in the experimental group spent time visiting a friend in their original neighborhood or lived in their original neighborhood in 2001 (four to seven years after randomization). In contrast, older children displaced by public housing demolition may not have had an incentive to return to their original neighborhood since their friends from the same building would have been forced to relocate.

IX. Cost-Benefit Analysis

This section uses results from Section V to provide back-of-the-envelope estimates of the benefits and costs of relocating youth from project-based public housing assistance using Section 8 vouchers.[52] These calculations help inform discussions of the optimal design of housing assistance. The US federal government currently spends $46 billion on housing assistance, and this fact underscores the need for a comparison of the relative efficiency of different housing assistance programs (Collinson, Ellen, and Ludwig 2015).[53]

To understand the earnings benefit of relocation, I use the reduced-form estimate from Table 3. Recall that these estimates reflect the impact of relocating using a Section 8 housing voucher relative to the counterfactual of living in project-based public housing. The results imply that replacing project-based assistance with vouchers increased youth earnings by about $602 (16 percent).

I follow Chetty, Hendren, and Katz (2016) to predict the impact of this effect on lifetime earnings. Specifically, I use the following assumptions: (i) the 16 percent increase in annual income is constant over the life cycle; (ii) the profile of income for demolition participants follows the US population average; (iii) the real wage growth rate is 0.5 percent; and (iv) the discount rate is 3 percent. Based on these assumptions, relocating youth using vouchers would increase pretax lifetime income by about $45,000 (present value of about $12,000).[54]

In terms of cost, studies suggest the cost of housing voucher programs is much lower than the cost of project-based housing assistance (Olsen 2014). This implies payments for moving expenses were the main direct cost of replacing project-based

[52] As discussed in Section V, the poverty rate was relatively high in the areas of Chicago where the CHA demolished public housing. This implies the estimates of the benefits and costs in this section may represent an upper bound.

[53] 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).

[54] The estimate of the impact on lifetime income is calculated as follows. First, I calculate the mean of individual pretax 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 US. This implies that the estimated effect of relocation on pretax undiscounted lifetime earnings is about $45,000 ($= 0.16 \times 0.16 \times \$1.75\text{m}$). All monetary values are in 2012 dollars.

assistance with Section 8 vouchers.[55] To the best of my knowledge, there is no record of moving payments provided by the CHA. However, I can use the schedule for federal payments used to reimburse individuals displaced by government projects (e.g., highway construction). The scheduled payment is set at $1,100 for a furnished four-bedroom apartment in Illinois.

Overall, this accounting suggests relocating children from public housing generates a high rate of return on investment, since the value of increased lifetime earnings is about $24,000 for a family with two children and the main cost comes from moving expenses which are most likely around $1,100 per family. Assuming there is a 10 percent increase in tax revenue for relocated children, this implies that the government would gain about $1,300 ($= \$24{,}000 \times 0.10 - \$1{,}100$) per family. Yet, 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, but there were detectable increases in other types of crime.[56]

X. Conclusion

The demolition of project-based public housing during the past three decades represents a dramatic shift in US housing assistance policy. During its peak in the early 1990s, local city authorities operated more than 1.4 million units of public housing across the country (Collinson, Ellen, and Ludwig 2015). Since this period, the stock of public housing has shrunk by about 300,000 units in part due to the federal HOPE VI program, which provided more than $6 billion to demolish severely distressed public housing units (US Government Accountability Office 2007).

This paper provides the first evidence on the long-run causal impacts of demolition and relocation for children who lived in severely distressed public housing. I exploit the initial wave of public housing demolitions that occurred in Chicago during the 1990s as a natural experiment. Households displaced by public housing demolition received Section 8 housing vouchers and relocated (on average) to less disadvantaged neighborhoods.

My analysis reveals that children displaced by public housing demolition have notably better labor market outcomes measured in early adulthood compared with their non-displaced peers. In line with evidence from other recent studies of relocation, I find that there are larger positive impacts for children who were young (age 7 to 12) when they moved (Chetty, Hendren, and Katz 2016; Chetty and Hendren 2016a, b). At age 26, the positive effect of relocation is $3,036 ($p$-value $= 0.05$) for children displaced at young ages. This estimate is substantially larger than the effect on earnings at age 26 for children displaced at older ages.

In addition, the analysis reveals positive impacts on criminal and schooling outcomes. Displaced children have fewer arrests for violent crimes than their

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

[56] Using different data and methodology relative to Aliprantis and Hartley (2015), Popkin et al. (2012) find that both violent and property crime increased in areas that received displaced public housing residents.

non-displaced peers. This impact is driven by a statistically significant decrease in arrests for males. In terms of education outcomes, the analysis shows that demolition and relocation reduced the likelihood of dropping out of high school for children who were young at the time of demolition.

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

REFERENCES

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): 1481–95.

Andersson, Fredrik, John C. Haltiwanger, Mark J. Kutzbach, Giordano E. Palloni, Henry O. Pollakowski, and Daniel H. Weinberg. 2016. "Childhood Housing and Adult Earnings: A Between-Siblings Analysis of Housing Vouchers and Public Housing." NBER Working Paper 22721.

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." *Quarterly Journal of Economics* 115 (3): 1019–55.

Bishaw, Alemayehu. 2014. *Changes in Areas with Concentrated Poverty: 2000 to 2010.* Suitland, MD: US Census Bureau.

Bitler, Marianne P., Hilary W. Hoynes, and Thurston Domina. 2014. "Experimental Evidence on Distributional Effects of Head Start." NBER Working Paper 20434.

Black, Sandra E., Paul J. Devereux, Katrine V. Loken, and Kjell G. Salvanes. 2014. "Care or Cash? The Effect of Child Care Subsidies on Student Performance." *Review of Economics and Statistics* 96 (5): 824–37.

Carrell, Scott E., Mark Hoekstra, and Elira Kuka. 2016. "The Long-Run Effects of Disruptive Peers." NBER Working Paper 22042.

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." *Quarterly Journal of Economics* 126 (4): 1593–660.

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): 2633–79.

Chetty, Raj, and Nathaniel Hendren. 2016a. "The Impacts of Neighborhoods on Intergenerational Mobility I: Childhood Exposure Effects." NBER Working Paper 23001.

Chetty, Raj, and Nathaniel Hendren. 2016b. "The Impacts of Neighborhoods on Intergenerational Mobility II: County-Level Estimates." NBER Working Paper 23002.

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. 2018. "Moved to Opportunity: The Long-Run Effects of Public Housing Demolition on Children: Dataset." *American Economic Review.* https://doi.org/10.1257/aer.20161352.

Chyn, Eric, Brian Jacob, and Jens Ludwig. 2017. "The Impact of Public Housing Demolition on Long-Run Education Outcomes." Unpublished.

Clampet-Lundquist, Susan, Kathryn Edin, Jeffrey R. Kling, and Greg Duncan. 2011. "Moving Teenagers out of High-Risk Neighborhoods: How Girls Fare Better than Boys." *American Journal of Sociology* 116 (4): 1154–89.

Collinson, Robert, Ingrid Gould Ellen, and Jens Ludwig. 2015. "Low-Income Housing Policy." NBER Working Paper 21071.

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): 1806–32.

Fryer, Roland G., Jr., and Lawrence F. Katz. 2013. "Achieving Escape Velocity: Neighborhood and School Interventions to Reduce Persistent Inequality." *American Economic Review* 103 (3): 232–37.

Garces, Eliana, Duncan Thomas, and Janet Currie. 2002. "Longer-Term Effects of Head Start." *American Economic Review* 92 (4): 999–1012.

Goering, John, Joan Kraft, Judith Feins, Debra McInnis, Mary Joel Holin, and Huda Elhassan. 1999. *Moving to Opportunity for Fair Housing Demonstration Program: Current Status and Initial Findings*. Washington, DC: US Department of Housing and Urban Development.

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

Haider, Steven, and Gary Solon. 2006. "Life-Cycle Variation in the Association between Current and Lifetime Earnings." *American Economic Review* 96 (4): 1308–20.

Heckman, James, Rodrigo Pinto, and Peter Savelyev. 2013. "Understanding the Mechanisms Through Which an Influential Early Childhood Program Boosted Adult Outcomes." *American Economic Review* 103 (6): 2052–86.

Hoynes, Hilary, Diane Whitmore Schanzenbach, and Douglas Almond. 2016. "Long-Run Impacts of Childhood Access to the Safety Net." *American Economic Review* 106 (4): 903–34.

Hunt, D. Bradford. 2009. *Blueprint for Disaster: The Unraveling of Chicago Public Housing*. Chicago: University of Chicago Press.

Jacob, Brian A. 2004. "Public Housing, Housing Vouchers, and Student Achievement: Evidence from Public Housing Demolitions in Chicago." *American Economic Review* 94 (1): 233–58.

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

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.

Kline, Patrick, and Christopher R. Walters. 2016. "Evaluating Public Programs with Close Substitutes: The Case of Head Start." *Quarterly Journal of Economics* 131 (4): 1795–848.

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." *Quarterly Journal of Economics* 120 (1): 87–130.

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

Newman, Oscar. 1973. *Defensible Space: Crime Prevention through Urban Design.* New York: Macmillan Company.

Olsen, Edgar O. 2014. "Alleviating Poverty through Housing Policy Reform." Unpublished.

Oreopoulos, Philip. 2003. "The Long-Run Consequences of Living in a Poor Neighborhood." *Quarterly Journal of Economics* 118 (4): 1533–75.

Oreopoulos, Philip. 2008. "Neighbourhood Effects in Canada: A Critique." *Canadian Public Policy* 34 (2): 237–58.

Oreopoulos, Philip. 2012. "Moving Neighborhoods versus Reforming Schools: A Canadian's Perspective." *Cityscape: A Journal of Policy Development and Research* 14 (2): 207–12.

Orr, Larry, Judith Feins, Robin Jacob, Erik Beecroft, Lisa Sanbonmatsu, Lawrence F. Katz, Jeffrey B. Liebman, and Jeffrey R. Kling. 2003. "Moving to Opportunity for Fair Housing Demonstration: Interim Impacts Evaluation." Washington, DC: US Department of Housing and Urban Development.

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

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: A Journal of Policy Development and Research* 14 (3): 137–60.

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): 556–73.

Rosenbaum, James E. 1995. "Changing the Geography of Opportunity by Expanding Residential Choice: Lessons from the Gautreaux Program." *Housing Policy Debate* 6 (1): 231–69.

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, Lawrence F. Katz, Jens Ludwig, Lisa A. Gennetian, Greg J. Duncan, Ronald C. Kessler, Emma Adam, Thomas W. McDade, and Stacy Lindau. 2011. *Moving to Opportunity for Fair Housing Demonstration Program: Final Impacts Evaluation*. Washington, DC: US Department of Housing and Urban Development.

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): 649–91.

Sandler, Danielle H. 2017. "Externalities of Public Housing: The Effect of Public Housing Demolitions on Local Crime." *Regional Science and Urban Economics* 62: 24–35.

Shroder, Mark D., and Larry L. Orr. 2012. "Moving to Opportunity: Why, How, and What Next?" *Cityscape: A Journal of Policy Development and Research* 14 (2): 31–56.

US Government Accountability Office. 2007. *Public Housing: Information on the Financing, Oversight, and Effects of the HOPE VI Program.* Washington, DC: US Government Accountability Office.

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