Public Housing, Housing Vouchers, and Student Achievement: Evidence from Public Housing Demolitions in Chicago

By BRIAN A. JACOB*

This paper utilizes a plausibly exogenous source of variation in housing assistance generated by public housing demolitions in Chicago to examine the impact of high-rise public housing on student outcomes. I find that children in households affected by the demolitions do no better or worse than their peers on a wide variety of achievement measures. Because the majority of households that leave high-rise public housing in response to the demolitions move to neighborhoods and schools that closely resemble those they left, the zero effect of the demolitions may be interpreted as the independent impact of public housing. (*JEL* I28, I38, J18)

Over the past half century, the federal government has taken on a considerable role in providing housing assistance to low-income individuals. The real costs of housing assistance have increased substantially over the past two decades, from $7.5 billion in 1977 to over $26 billion in 1997. Outlays per unit nearly doubled over this period, from $2,980 to $5,490. The number of households assisted has also risen during this period, from 3.2 million to 5.7 million (U.S. House of Representatives, 1998). In fact, in the early 1990's, the federal government spent nearly as much on housing assistance as it did on programs such as AFDC, SSI, and Food Stamps, and considerably more than it spent on EITC and JTPA (Rebecca Blank, 1997).

At the same time, high-rise public housing developments are one of the most striking symbols of urban decay and ghetto poverty in our society. Many critics argue that public housing has fostered racial and economic segregation (Douglas Massey and Nancy Denton, 1993), increased crime and juvenile delinquency (Oscar Newman, 1972), and reduced educational and employment outcomes for children and adults (Paul Ong, 1998). Recent accounts have vividly portrayed the poverty, violence, and despair endemic in inner-city public housing (Alex Kotlowitz, 1991).

As a result of these factors, there has been a national shift toward providing low-income renters with vouchers to purchase housing on the private market, rather than providing assistance through housing projects. Between 1977 and 1997, the number of households receiving housing vouchers increased from 162,000 to over 1.4 million, which comprised over one-third of all low-income renters served by HUD. In 1996, Congress passed Section 202 of the Omnibus Consolidated Rescissions and Appropriations Act (Section 202) that required housing authorities to conduct a viability assessment of their public housing stock. If the costs of rehabilitation and maintenance for a particular unit exceed the cost of providing the families in that unit with a rent subsidy for the private housing market for a period of 20 years, then the housing authority must remove the unit from its stock. Roughly 91,000 units across 35 public housing authorities are scheduled for demolition as a result of this legislation. In

* John F. Kennedy School of Government, Harvard University, 79 John F. Kennedy Street, Cambridge, MA 02138 (e-mail: brian_jacob@harvard.edu). I would like to thank the Consortium on Chicago School Research, the Chicago Public Schools, and the Chicago Housing Authority for providing the data used in this study, and Ed Yong and Lee Burr for assistance interpreting data from the Housing Authority. I gratefully acknowledge the many helpful suggestions of Julie Cullen, Thomas DeLeire, Mark Duggan, Sue Dynarski, Carolyn Hill, Robin Jacob, Larry Katz, Jeff Kling, Robert LaLonde, Lars Lefgren, Steven Levitt, Jens Ludwig, Susan Mayer, Sudhir Venkatesh, Aaron Yelowitz, and two anonymous referees. This research was partially supported by grants from the Milton Fund and the Joint Center on Poverty Research at the University of Chicago/Northwestern University. Any remaining errors are my own. The data used in this study is available on the AER Web site (http://www.aeaweb.org/aer/contents/).

Chicago alone, 19,000 units in 17 developments failed the viability test [Chicago Housing Authority (CHA), 1998].

Despite these substantial policy changes, we know relatively little about the impact of public housing on the well-being of poor families, let alone the impact of demolishing the traditional high-rises and shifting families to voucher-based assistance. Some evidence in the early seventies suggested that public housing fostered social dysfunction (Newman, 1972), but recent work suggests that public housing participation does not have an adverse effect on long-run labor market outcomes (Philip Oreopoulos, 2003) and that children in public housing may actually have better educational outcomes than low-income children who do not live in public housing (Janet Currie and Aaron Yelowitz, 2000).

While some studies have compared low-income families receiving public housing to those receiving voucher-based or no housing assistance, most fail to account for selection.[1] Studies of two housing mobility programs provide more compelling evidence regarding the relative benefits of voucher-based assistance.[2] Since 1976 the Gautreaux program in Chicago has provided more than 7,000 families with vouchers to leave public housing in a context where the relocation destination was determined in a quasi-random fashion.[3] Comparisons of households that moved within the city ("urban movers") to those who moved to the suburbs ("suburban movers") suggest that moving to low-poverty neighborhoods substantially increases youth educational attainment and labor market outcomes as well as modestly improving mothers' employment rates (Julie Kaufman and Rosenbaum, 1992; Rosenbaum, 1995).

A recent randomized housing mobility program, Moving to Opportunity (MTO), provides additional evidence on neighborhood effects.[4] An interim evaluation of MTO finds few positive effects of moving to a low-poverty neighborhood after four to seven years (Larry Orr et al., 2003). The offer of a housing voucher had no effect on adult earnings, employment, or receipt of public assistance, although there is some indication that it had a positive effect on certain health outcomes. Adults in the treatment groups, for example, experienced a significant decrease in obesity and improvement on some measures of mental health. Children in the treatment groups showed no improvement on a wide range of educational performance measures that included achievement scores, self-reported performance in school, and college attendance. There were, however, striking gender differences in the effects on a variety of behavioral and health outcomes among children, with girls appearing to benefit from a move to lower-poverty neighborhoods and boys suffering from such moves. For example, girls in the treatment group experienced a reduction in stress and depression as well as a decrease in arrest rates for violent crime while boys experienced an increase in self-reported behavior problems along with a rise in arrests for property crimes.[5]

[1] See, for example, Sandra Newman and Ann Schnare (1993) and Margaret Austin Turner (1998).

[2] The earliest evidence on housing vouchers comes from the Experimental Housing Allowance Program (EHAP), a massive, federally funded social experiment during the 1970's. In a comprehensive review of EHAP findings, Raymond Struyk and Marc Bendick (1981) concluded that housing allowances neither increased mobility nor affected racial or economic segregation. Unfortunately, this experiment did not provide direct evidence on outcomes such as education, employment, crime, or health.

[3] The Gautreaux program resulted from a lawsuit brought against the CHA and HUD in the early 1970's that charged the agencies with violating the civil rights of tenants by pursuing racially discriminatory housing practices. As part of a settlement in the Gautreaux case, HUD and CHA established a program that provided public housing residents with Section 8 housing vouchers to move to private housing in Chicago or nearby suburbs. Counselors offered families units as they became available regardless of their location preference, and few families turned down this initial offer since it would have entailed a substantial wait for and uncertainty regarding availability of another unit (James Rosenbaum, 1995; Stephanie DeLuca and Rosenbaum, 2002).

[4] Motivated in part by the Gautreaux findings, HUD sponsored a mobility experiment (MTO) that randomly assigned low-income families living in public housing in five cities (Chicago, New York, Los Angeles, Boston, and Baltimore) to one of three groups: (i) an experimental group (MTO group) which received housing subsidies and search assistance to move to private-market housing in tracts with poverty rates below 10 percent; (ii) a comparison group that received Section 8 housing vouchers with no constraint on relocation choice (Section 8 group); and (iii) a control group that received no special assistance.

[5] Preliminary evaluations of MTO only several years after implementation, particularly studies focusing on the Boston and Baltimore sites, found more positive effects. For more detail, see John Goering et al. (1999), Jens Ludwig et al. (2000, 2001), Lawrence Katz et al. (2001), Emily Rosenbaum

A recent series of public housing closings in Chicago provides an excellent opportunity to study the effect of high-rise public housing in general as well as the impact of providing families with housing vouchers. During the 1990's, the CHA closed over 7,400 units of public housing in 12 developments. Residents in the affected buildings were offered Section 8 housing vouchers to move to private housing anywhere in the metropolitan area. The families affected by the closings did not volunteer to move, they were not provided with additional counseling or support, and they were not required to relocate to any particular area.

In this paper, I examine the impact of recent high-rise public housing closings in Chicago on student achievement. To measure educational outcomes over time, I combine administrative data from the CHA and the Chicago Public Schools (ChiPS). I match students to housing developments through home addresses in school records and determine building closure from occupancy data provided by the CHA. To estimate the impact of public housing demolition, I compare students living in CHA units slated for closure with peers living in units in the same project that were not closed. To the extent that these groups were identical prior to closure, any subsequent differences in educational achievement can be attributed to the closure. While the CHA targeted projects for redevelopment on the basis of vacancy rates and physical deterioration, the *timing* of building closures *within* a project is plausibly uncorrelated with unobservable student characteristics.

I find that the demolitions led to a small increase in the drop-out rate among older children (i.e., youth aged 14 and older at the time of the closure announcement), but had no impact on the academic achievement of younger children on a wide variety of outcome measures. This was true across various subgroups and did not appear to change substantially over time. In terms of relocation outcomes, I find that children affected by the closures were considerably less likely to be living in high-rise public housing in subsequent years but were still living in high-poverty neighborhoods and attending schools identical to those of the control students. Indeed, even those students who did move to substantially better neighborhoods did not end up in significantly better schools.

These results suggest that high-rise public housing does not have an independent impact on student achievement.[6] This finding contradicts some of the earlier literature on public housing participation, but is consistent with the recent work by Currie and Yelowitz (2000) and Oreopoulos (2003). The finding that low-income families tend to relocate close to their original neighborhood is consistent with the MTO studies (John Goering et al., 1999). While in contrast to some of the initial results from MTO (Ludwig et al., 2000), the finding here that children whose families were offered Section 8 vouchers did not show improved academic achievement is quite consistent with the most recent MTO results. One important difference between this study and both Gautreaux and MTO is that the families in the two housing mobility experiments all volunteered for the program whereas the households in this study were forced to relocate. In general, the results presented here suggest that the current HUD policy initiatives to eliminate high-rise public housing will not necessarily lead to the benefit documented in Gautreaux and early MTO studies.

The remainder of this paper is structured as follows. Section I describes the institutional factors leading to the demolitions and provides a conceptual framework for understanding the impact of the demolitions. Section II outlines the empirical strategy, while Section III describes the data. Sections IV and V present the findings, and Section VI concludes.

I. Background

A. *Public Housing in Chicago During the 1990's*

The Chicago Housing Authority (CHA) was organized in 1937 to provide temporary housing for people unable to obtain "decent, safe and

and Laura Harris (2001), and Tama Leventhal and Jeanne Brooks-Gunn (2003).

[6] Below I argue that the drop-out effect for older children was largely a result of the initial disruption generated by the relocation. For younger children, it is possible that benefits associated with the relocation opportunity may have been offset by negative effects of the move itself, although the results in Table 3 suggest that this is not the case.

sanitary dwellings" in the private market. Today public housing residents represent roughly 4.7 percent of the city's population, making the CHA the third largest housing authority in the nation. The largest component of the CHA consists of 17 federally funded developments that primarily serve families with children, including 28,335 units with 50,526 residents (CHA, 2000a).

The public housing closings that took place in Chicago during the past decade were not part of a unified plan, but rather the result of a variety of events and initiatives, some purposeful and others unforeseen.[7] Reasons for the closures range from a desire on the part of the CHA to remove or rehabilitate unsafe buildings to initiatives of private developers to build new market rate and mixed-income housing in gentrifying neighborhoods. Some of the earliest building closures and demolitions stemmed from a court case filed on behalf of residents in the Henry Horner Homes. In 1991, the National Center on Poverty Law filed suit against the CHA on behalf of the Henry Horner Mothers' Guild and individual residents, claiming that the CHA and HUD violated the United States Housing Act by failing to maintain the Horner developments as "decent, safe, sanitary and otherwise habitable." The consent decree signed by the CHA in 1995 called for a comprehensive revitalization of Horner, which entailed the demolition of a many mid- and high-rise buildings (James Zagel, 1995).

While many of the closures and demolitions have taken place as part of a federally funded redevelopment initiative known as the HOPE VI program, the most widely publicized closures have been in response to crises. The Cabrini-Green development, plagued by gang violence in the 1980's, attained national notoriety as a result of the shooting death of a seven-year-old boy, Dantrell Davis, on his way to school on October 13, 1992. This spurred the mayor and the CHA to vacate several of the Cabrini high-rises, which were later demolished in 1995 (Christine Hawes, 1992; Matthew Nickerson, 1992). In January 1999, pipes burst in several of the Robert Taylor high-rises, causing flooding and leaving residents in those buildings without heat in the middle of a major snowstorm. CHA was forced to evacuate over a hundred families in four buildings, placing them temporarily in local hotels and churches before permanently relocating them (Melita Marie Garza, 1999a, b; Bechetta Jackson and Garza, 1999).

Families that were required to relocate were given the option to either (1) transfer to another unit within their current development, (2) transfer to a unit in another CHA development (contingent on availability), or (3) receive a Section 8 voucher. If a family chose the Section 8 option, the CHA paid for moving expenses as well as the cost of transferring telephone, electricity, and other utilities.

While providing an opportunity to leave public housing, the building closures that took place in Chicago during the 1990's differed considerably from the randomized housing mobility experiments such as Gautreaux and MTO. First, participants in the earlier housing mobility experiments were not only volunteers, but were likely a select group of public housing residents since they had to meet certain requirements in order to participate in the program.[8] Second, Chicago families were not required to relocate to low-poverty neighborhoods, as was the case for the experimental groups in Gautreaux and MTO.[9] Finally, the Chicago families received considerably fewer support services than the experimental families in Gautreaux and MTO.[10]

B. *Conceptual Framework*

The relocation opportunity provided by the demolitions might influence educational out-

[7] In contrast, the more recent building closures and demolitions in Chicago have been carefully laid out in advance as part of the "plan for transformation" in Chicago's public housing system (see CHA, 2000b).

[8] For example, the Gautreaux program only selected families with four or fewer children, who paid their rent regularly, had some source of income (usually AFDC) and met acceptable housekeeping standards.

[9] Certain groups in the mobility experiments—the urban movers in Gautreaux and the Section 8 control group in MTO—were not required to move to low-poverty neighborhoods. There is some evidence that these groups benefited relative to nonmovers though not as much as the families who moved to low-poverty neighborhoods.

[10] The average cost to counsel each experimental family in Gautreaux and MTO was $555 and $1,455, respectively (Goering et al., 1999). While I was not able to obtain comparable figures for the Chicago relocations, CHA officials indicated that the cost was substantially less than standard housing mobility programs.

comes through several different mechanisms. First, the relocation opportunity may allow families to move to lower-poverty neighborhoods. There are a number of theories linking residential location to individual outcomes, emphasizing factors such as peer effects, social networks, and local public goods (Christopher Jencks and Susan Mayer, 1990; Jeanne Brooks-Gunn et al., 1997). There is an extensive literature documenting the association between neighborhood or school characteristics and individual outcomes (Ingrid Gould Ellen and Margery Austin Turner, 1997; Paul Jargowsky, 1997). Quasi-random and experimental housing mobility programs such as Gautreaux and MTO provide more compelling evidence of some neighborhood effects on certain outcomes (Ludwig et al., 2000, 2001; Katz et al., 2001).

Second, relocation may raise the quality of a child's school. While there is some controversy over which (if any) observable school or teacher characteristics are associated with student achievement, there is strong evidence that some schools and teachers are more effective than others (Eric Hanushek, 1986, 1992; Steven Rivkin et al., 2002; Jonah Rockoff, 2002). Even a modest change in neighborhood quality might be associated with substantial improvements in school performance if the move leads to a change in the school attendance area.[11]

Third, regardless of the distance of the move, Section 8 relocation allows households to leave public housing, which may influence educational outcomes. Sociological theory based on the notion of "defensible space" contends that the physical characteristics of high-rise public housing (i.e., centralized elevator banks, long corridors and multiple entries) foster criminal behavior and other social problems, although the evidence to support this hypothesis is mixed (Newman, 1972; Newman and Karen Franck, 1980; Dennis Roncek et al., 1981; John Farley, 1982; Terence Dunworth and Aaron Saiger, 1994; Harold Holzman, 1996). In fact, one study in Chicago found a relationship between public housing, increased criminal behavior, and decreased employment rates in census tracts with high-rise developments, but found no such relationship in tracts with low-rise developments (Mark Condon, 1991). Alternatively, public housing may provide benefits that are not available to low-income families in private housing, including adequate quality housing, greater access to social services and a close network of friends and family. Several recent studies that address the endogeneity of public housing participation suggest that public housing has either a zero or even small positive effect on outcomes (Currie and Yelowitz, 2000; Newman and Joseph Harkness, 2000, 2002; David Reingold et al., 2001; Oreopoulos, 2003).

Finally, while living in private housing in better neighborhoods and attending better schools may increase academic achievement, some evidence suggests that the disruption of the move itself may have a negative impact on school performance, particularly in the short run. There is a substantial literature that documents the negative association between school mobility and student achievement (Gary Ingersoll et al., 1989; Karl Alexander et al., 1994; David Kerbow, 1996). It is important to note that the analysis here involves *forced* relocations as opposed to those in Gautreaux and MTO in which relocation was voluntary.[12]

II. The Empirical Strategy

The building closures described above provide an opportunity to examine two related questions. First, this natural experiment allows one to plausibly estimate the net causal effect of public housing demolitions on the educational outcomes of children. These reduced-form estimates (often referred to as "Intent-to-Treat" or ITT effects) of public housing closures are useful from a policy perspective insofar as they shed light on the potential impact of the ongoing wave of public housing demolitions taking place across the country as a result of the recent federal legislation. Second, the demolitions may also identify a parameter of more general interest—namely, the effect of living in public housing. Recall that families in demolished buildings were offered Section 8 housing

[11] This is only one mechanism that could generate nonlinear neighborhood effects.

[12] In one of the few to examine forced relocation, David Varady and Carol Walker (2000) found that the majority of households chose to remain in the same neighborhood and thus continued to live in racially segregated neighborhoods.

vouchers, allowing them to leave public housing. To the extent that the demolitions were unrelated to unobservable child or family characteristics, one can use the demolitions as an instrumental variable to estimate the effect of utilizing a housing voucher to rent a unit on the private market. As discussed below, these estimates are akin to the "Treatment-on-the-Treated" (TOT) effects that have been estimated in housing mobility studies such as MTO.

A. *Estimating the Reduced-Form Effect of Public Housing Demolitions*

If public housing demolitions were randomly assigned to families, one could estimate the net effect of the demolitions by simply comparing the outcomes of children living in buildings slated for demolition with those of all other children. As Table 1 shows, however, the demolitions were far from randomly assigned—students whose buildings were closed or demolished were systematically different from their peers. For example, over 90 percent of students in public housing receive free lunch compared with 72 percent of the entire student population. Public housing students live in census tracts with poverty rates roughly three times higher than other students, attend schools with substantially fewer high-performing peers, score 12 percentage points lower on standardized math and reading exams, and miss 60 percent more days in high school than other students.

Instead, I compare students who were living in buildings slated for demolition immediately prior to the closure announcement with students living in buildings in the *same project* that were not slated for demolition. Let y be some outcome, D be an indicator for living in a building subject to demolition, and $\mathbf{X}$ be a vector of individual background variables (including an intercept). The demolition impact is thus captured by the π_1 coefficient in the ordinary least-squares (OLS) regression

$$(1) \qquad y_{ijt} = D_{ijt}\pi_1 + \boldsymbol{\gamma}_{\mathbf{j}} + \boldsymbol{\delta}_{\mathbf{t}} + \varepsilon_{1ijt}$$

where i, j, and t index individuals, projects, and time, respectively, γ and δ are fixed effects for project and year, respectively, and ε is a stochastic error term.

The key assumption required for consistent estimation of π_1 is that the demolitions were not correlated with unobserved factors that may influence student achievement directly—i.e., $\text{Cov}(D_{ijt}, \varepsilon_{1ijt}) = 0$. In the following section, I present evidence suggesting that this assumption is met here. Students in demolished and non-demolished buildings appear identical on a wide range of observable characteristics immediately prior to the closure announcement. In addition, I discuss the history of the building closures, along with the process of tenant assignment and transfer in the CHA, all of which suggest that the demolitions examined in this paper were plausibly uncorrelated with other unobservable student characteristics.[13] However, including additional covariates ($\mathbf{X}$) allows one to satisfy a slightly weaker condition, $\text{Cov}(D_{ijt}, \varepsilon_{1ijt}|X_{ijt}) = 0$, and reduces the residual variation in (1), thereby increasing the precision of the demolition estimate. For this reason, the main estimates presented in the paper will come from the following specification:

$$(2) \quad y_{ijt} = D_{ijt}\pi_2 + X_{ijt}\beta_2 + \boldsymbol{\gamma}_{\mathbf{j}} + \boldsymbol{\delta}_{\mathbf{t}} + \varepsilon_{2ijt}$$

An additional concern arises from the fact that the primary outcome measures—e.g., test scores, grades, and retention—are not observed for students who drop out or otherwise leave the ChiPS. If this sample attrition is correlated with the demolitions, it may bias the achievement estimates. Hence, an additional assumption required for consistent estimation of π_2 is that $\text{Cov}(M^y_{ijt}, \varepsilon_{2ijt}|X_{ijt}) = 0$ where M^y_{ijt} is a binary indicator of whether outcome y is missing for individual i at time t. In Section V, I show that the demolitions have no statistically significant effect on transferring out of the ChiPS, being enrolled in school, or having valid achievement data, suggesting that the achievement estimates are not biased by differential sample attrition.

Assuming that the above assumptions are

[13] These estimates also assume that the demolitions did not have any impact on the educational outcomes of control students. While some observers have suggested that the demolitions may have improved the living environment in the remaining buildings within the public housing development, such effects (if they exist) are likely to be an order of magnitude smaller than the direct effect of the demolition on students in the treatment buildings.

TABLE 1—A COMPARISON OF STUDENTS IN THE CHICAGO PUBLIC SCHOOLS (ChiPS)

	ChiPS students enrolled in 1995			Analysis sample (year prior to closure announcement)		
Dependent variable	All students (1)	Living in public housing (2)	Living in public housing developments with closures (3)	Control mean (s.d.) (4)	Demo mean (s.d.) (5)	Difference: Demo − Control (s.e.) (6)
Male	0.51	0.50	0.50	0.508	0.499	−0.014
	(0.50)	(0.50)	(0.50)	(0.500)	(0.500)	(0.012)
Black	0.55	0.96	1.00	—	—	—
	(0.50)	(0.20)	(0.06)			
Living with at least one parent	0.863	0.911	0.910	0.920	0.910	−0.003
	(0.344)	(0.285)	(0.286)	(0.272)	(0.286)	(0.011)
Living in foster care	0.037	0.047	0.050	0.046	0.053	0.004
	(0.190)	(0.211)	(0.217)	(0.210)	(0.225)	(0.008)
In special education	0.118	0.135	0.133	0.114	0.116	−0.003
	(0.322)	(0.342)	(0.339)	(0.318)	(0.320)	(0.009)
Free lunch	0.719	0.907	0.913	0.883	0.879	−0.009
	(0.450)	(0.290)	(0.282)	(0.322)	(0.326)	(0.009)
Age	11.3	10.4	10.8	10.34	10.04	−0.323**
	(3.9)	(3.9)	(3.9)	(4.02)	(4.01)	(0.143)
Old for grade	0.07	0.10	0.10	0.197	0.200	0.013
	(0.25)	(0.30)	(0.31)	(0.410)	(0.400)	(0.012)
Math score (percentile rank)	34.5	24.7	23.7	25.1	27.5	−0.73
	(28.0)	(24.0)	(23.8)	(24.2)	(24.6)	(1.53)
Reading score (percentile rank)	32.8	23.5	22.2	22.8	24.6	−0.40
	(25.1)	(20.7)	(20.2)	(20.4)	(20.8)	(1.07)
Math gain in prior year	−1.1	−2.8	−2.3	0.02	−0.37	0.13
	(16.8)	(17.8)	(17.6)	(18.60)	(18.73)	(0.98)
Reading gain in prior year	−0.2	−2.1	−2.0	0.02	−0.15	0.14
	(17.5)	(17.5)	(17.3)	(17.83)	(18.38)	(0.80)
GPA	1.88	1.42	1.36	1.458	1.538	0.056
	(1.02)	(0.93)	(0.94)	(0.908)	(0.919)	(0.044)
Absences (per course)	18.5	28.8	31.4	23.7	20.2	−0.458
	(17.5)	(20.7)	(21.2)	(16.8)	(15.4)	(0.690)
Course credits	22.8	22.5	22.5	23.0	23.3	0.105
	(2.8)	(3.3)	(3.3)	(3.5)	(3.8)	(0.217)
Moved in past year	0.074	0.049	0.047	0.114	0.109	−0.016
	(0.262)	(0.216)	(0.211)	(0.313)	(0.312)	(0.016)
Changed schools in past year	0.212	0.195	0.188	0.169	0.174	0.007
	(0.409)	(0.396)	(0.391)	(0.374)	(0.379)	(0.011)
Census-tract poverty rate	0.27	0.72	0.82	0.844	0.825	0.004
	(0.21)	(0.21)	(0.11)	(0.100)	(0.118)	(0.012)
Percent school peers meeting national norms in math	0.28	0.17	0.16	0.168	0.198	−0.005
	(0.17)	(0.11)	(0.11)	(0.103)	(0.117)	(0.015)
Number of observations	416,104	28,996	18,484	7,030	3,526	10,556

Notes: In columns (1)–(3), public housing includes all low-, medium-, and high-rise developments. For these columns, standard deviations (s.d.) are presented in parentheses below the group means. Note that the district used a different form of the achievement exam in 1995, which may explain the systemwide declines in performance from 1994 to 1995 seen in columns (1)–(3). Columns (4)–(6) present results from the analysis sample ($n = 10{,}556$), but varies somewhat across rows because certain outcomes are only available for elementary or secondary students. Columns (4) and (5) show the control and treatment (i.e., demolition) group means in the year prior to the closure announcement with the standard deviations in parentheses. Column (6) shows the difference between treatments and controls in this year. The differences are estimated from a regression model that includes fixed effects for housing development by year and, for individual achievement measures, fixed effects for grade. Eicker-White robust standard errors clustered by public housing building are shown in parentheses.

** Significant at the 5-percent level.

met, the coefficient π_2 will reflect the causal impact of public housing demolitions on educational outcomes. In a program evaluation context, this parameter is generally referred to as the "Intent-to-Treat" (ITT) effect. In housing mobility studies such as MTO, for example, the ITT refers to the difference in outcomes between those offered a housing voucher (i.e., the treatment group) and a control group that was not offered a voucher. Here the ITT effect sheds light on the potential impact of the future demolitions in Chicago and similar building closures taking place in urban areas throughout the country as a result of the recent federal legislation. From a program evaluation perspective, therefore, π_2 is the key parameter of interest.

B. *Estimating the Effect of Living in Public Housing*

The ITT captures the effect of being *offered* the treatment.[14] In many cases, however, researchers and policy makers are interested in understanding the effect of *receiving* the treatment. For this reason, housing mobility studies such as MTO generally also report estimates of the "Treatment-on-the-Treated" (TOT) effect, which measures the average effect of the treatment on those in the treatment group who actually receive the treatment (i.e., actually move with a voucher). In its simplest form, the TOT can be estimated by simply dividing the ITT effect by the proportion receiving the treatment (Howard Bloom, 1984), though researchers often employ an instrumental variables (IV) strategy that uses treatment assignment as an instrument in order to gain greater statistical precision (Katz et al., 2001). It is worthwhile noting that the TOT is still a "net" effect in the sense that it does not identify which of the mechanisms (e.g., neighborhood poverty, peers, school quality, etc.) is driving the effect of using a voucher.

Because I do not have direct information on voucher use in my data, I cannot precisely replicate the TOT estimates generated in most housing mobility studies. Instead, following earlier work that examines the effect of public housing participation (Currie and Yelowitz, 2000; Oreopoulos, 2003), I use demolition as an instrument to estimate the causal impact of living in public housing (relative to subsidized, Section 8, private housing) on educational outcomes. Consider the following equation that relates some educational outcome (y) to an indicator for living in public housing (P) and a vector of background characteristics ($\mathbf{X}$)

$$(3) \quad y_{ijt} = P_{ijt}\beta_P + X_{ijt}\beta_3 + \boldsymbol{\gamma}_\mathbf{j} + \boldsymbol{\delta}_\mathbf{t} + \varepsilon_{3ijt}$$

where, as before, $\boldsymbol{\gamma}_j$ reflects fixed effects for initial development and $\boldsymbol{\delta}_t$ reflects fixed effects for the announcement year. Here we specify P as a binary variable, taking on a value of one if the student were living in public housing in year t and zero otherwise.

Typically, we are concerned that OLS estimates of β_P will be biased because living in public housing is likely correlated with unobserved family or child characteristics that also influence academic achievement. However, because residents in demolished buildings were offered housing vouchers that provide rent subsidies for private housing, the demolitions may serve as an instrument for public housing participation in our sample. This can be captured in the following first-stage equation:

$$(4) \quad P_{ijt} = D_{ijt}\pi_4 + X_{ijt}\beta_4 + \boldsymbol{\gamma}_\mathbf{j} + \boldsymbol{\delta}_\mathbf{t} + \varepsilon_{4ijt}$$

where D is an indicator for whether a student was living in a building slated for demolition at the time of the closure announcement. The coefficient π_4 in equation (4) measures the effect of the demolition on likelihood that a child will be living in public housing in year t.

In order for the demolition (D) to be a valid instrument for public housing participation, it must be correlated with the endogenous regressor (i.e., participation in public housing, P), but not directly correlated with the outcome of interest (i.e., student achievement, y). Formally, this implies $\text{Cov}(D_{ijt}, \varepsilon_{3ijt}) = 0$.[15] If these assumptions are met, the 2SLS estimate of β_P in

[14] Because not all of those families who are offered vouchers utilize them, the ITT represents the average of the causal effect for those who took the voucher and those who did not.

[15] In the context of estimating a standard TOT effect, this assumption is generally expressed as the requirement that the treatment must not have any impact on the "never-takers" (i.e., those who were assigned to a treatment group, but chose not to receive the treatment) (Katz et al., 2001).

equation (3) measures the effect of living in public housing in year *t* on educational outcomes in the same year.

Several points should be kept in mind when interpreting this estimate. First, this coefficient essentially reflects the impact of living in public housing *relative* to subsidized, Section 8, private housing (as opposed to receiving no housing subsidy at all).[16] Second, the assumptions required to interpret this coefficient as the independent causal effect of living in public housing per se (as distinct from neighborhood or school effects) are considerably stronger than those normally employed to estimate the TOT effect in a housing mobility study. Specifically, in order to interpret the 2SLS estimate of β_P as the independent causal effect of living in public housing, one must assume that the mobility, neighborhood, school, and relocation pathways described earlier have a zero effect. In Sections V and VI, I show that many (though not all) of these pathways do appear to have zero effects and, more importantly, in the case where there is a nonzero effect, the direction of the bias allows for a useful bounding of the estimates.

Finally, because some educational outcomes such as standardized test scores may be heavily dependent on a student's cumulative schooling (or life) experience, one can parameterize equations (3) and (4) so that *P* reflects the number of years the student lived in public housing since the beginning of the year in which the closure was announced rather than a simple binary indicator for living in public housing in year *t*. In these specifications, referred to as (3′) and (4′) respectively, the vector of student characteristics, **X**, includes preannouncement measures of student achievement. Thus, the coefficient π_4 in equation (4′) represents the effect of the demolition on the number of years a student lived in public housing between the year of the closure announcement and year *t*. The 2SLS estimate of β_P in equation (3′) measures the effect of an additional year in public housing on the student's achievement gain from the year prior to the closure announcement to year *t*.

[16] It is not possible to determine if this is strictly true because I cannot tell which families living in a private unit were receiving voucher-based assistance. Some of the families impacted by the demolitions may have left public housing and moved into unsubsidized private housing, most likely by moving in with friends or relatives. Footnote 36 discusses several reasons why families in the treatment group may not have made use of the Section 8 vouchers.

III. The Data

The data for this study are drawn from administrative records of the Chicago Public Schools (ChiPS) and the Chicago Housing Authority (CHA). Administrative data from the CHA list all public housing developments in the city, including building addresses and the number of units per building. Using student addresses in school records, I merge the ChiPS and CHA data to determine whether a student was living in public housing at a particular time.[17] The full data set is thus a nearly complete census of ChiPS students who lived in public housing during at least one semester between 1991–1992 and 2001–2002. This includes 94,666 students who lived in 2,180 buildings[18] in 36 developments during this period. Appendix Table A1 provides a more detailed discussion of the construction of the variables used in this analysis.

The sample used in this analysis is a subsample of the full data set. First, I restrict the analysis to "family developments" owned and operated by the CHA, thereby excluding individuals who live in senior-citizen developments or scattered-site public housing as well as those who live in private housing but receive Section 8 vouchers. Second, I consider only students living in high-rise buildings[19] in developments that experienced closings or demolitions over this period. I exclude developments that did not experience any building closings for two reasons: (1) Table 1 suggests that the children in these developments may differ systematically

[17] Records were matched by street address (including street number, direction, name, and type). Students with missing or inaccurate address information were not matched. However, generally less than 0.005 percent of students had missing or invalid address information in any given year. The CHA administrative files contained no missing address information.

[18] Note that a building may contain several different addresses. CHA defines a building as a structure with a continuous roof.

[19] Following the standard practice of the CHA, I define a high-rise as any building with at least 75 units.

from those in the developments that experienced closings; (2) Since I include fixed development effects in the statistical model, information on students in unaffected developments will not help estimate the treatment effect.[20] I exclude low- and mid-rise buildings because in general these buildings did not experience the same type of full-scale closure or demolition that high-rises did, but were rather vacated more slowly over a longer period of time. Third, I do not consider building closures that were announced prior to 1993 because the school records do not extend back far enough to track these students.[21] Fourth, I exclude cases where (a) there is good reason to believe that the demolition may have been correlated with unobservable tenant characteristics that also influence achievement (e.g., the three buildings in the Robert Taylor Homes commonly referred to as "The Hole" that were demolished in 1997 as a result of severe and persistent gang problems), or (b) students in demolished buildings appear substantially different on observable characteristics than students in nondemolished buildings. Fifth, since over 95 percent of public housing residents in Chicago are African-American, I have chosen to limit my analysis to these students. Finally, I drop observations that were missing demographic information (less than 1 percent of the sample).

To construct treatment and comparison groups, I first select a base group of students who were living in a public housing development in the year prior to notification of a building closure.[22] Students who were living in the buildings scheduled for closure comprise the treatment group while students in other stable buildings (defined as those buildings that were not closed between 1992 and 2002) serve as the comparison or control group. The final sample consists of 10,556 students in 73 buildings in 9 developments. The treatment group consists of 3,526 students living in 31 buildings slated for demolition. Appendix B describes the process used to determine the notification and closure dates and Appendix C provides more details on the construction of the sample.

A. *How Did Treatment and Control Students Compare Prior to the Closings?*

Columns (4)–(6) in Table 1 compare treatment and control students within housing development in the year prior to the closure announcement. If the demolitions were randomly assigned, then we would expect that the mean difference between treatment (i.e., demolition) and control groups would be zero, except for sampling error. Indeed, this condition appears to be largely true. In column (6), we see only 1 out of the 18 coefficients is significantly different from zero—children in treatment buildings were 0.32 years younger than their peers in control buildings. While the difference is statistically significant, it is quite small given the standard deviation of age in the sample is roughly 4.02. For all of the other characteristics, the coefficients are substantively small and statistically insignificant.[23]

Even in the absence of observable differences, however, the two groups may differ along unobservable dimensions. If the least motivated or capable families were more likely to live in buildings scheduled for demolition, for example, we might expect the treatment group children to have worse outcomes in the absence of the public housing closings, thus biasing the estimated demolition effect downward. In order

[20] Because I include fixed development effects, the treatment effect of closure is only estimated off of the variation within developments that experienced some treatment. While the other observations help identify the other coefficients in the model, the inclusion of these cases does not change the primary results so I have chosen to omit these observations.

[21] School records are available as far back as 1992, but in the case of some of the earliest building closures, it appears that families had begun vacating building prior to this date. Consequently, the number of students who report living in these buildings in 1992 or 1993 is extremely small.

[22] More specifically, if a building closure was announced in the 12-month period between November 1st in year one and October 31st in the following year, the "base group" consists of all students who were living in the development in October of year one.

[23] Note that the estimates for the achievement outcomes include a full set of fixed effects for current grade level to account for the age differences between treatment and control students. The results are identical if one includes age effects instead of grade effects. These effects were included primarily to account for potential differences in the achievement exams across grades.

for a comparison of treatment and control students to yield unbiased estimates of the treatment effect, either (a) families must have been randomly assigned to units within development (or in a manner uncorrelated with factors that may influence achievement), or (b) buildings within a project must have been selected for demolition for reasons uncorrelated with unobserved tenant characteristics that influence student achievement.

While it is impossible to completely rule out the possibility of unobservable differences, the processes of tenant assignment and recent building closures in Chicago suggest that such differences are unlikely. With roughly 30,000 families on the waiting list for CHA housing, waiting times of seven to eight years for public housing in Chicago are not uncommon. When families reach the top of the list, they are assigned units based on bedroom size and availability. Prospective tenants can reject an offer and place their name on a waiting list for a particular development, but this rarely occurs in practice because the site-specific waiting lists are often longer than the general CHA list. Because of the high demand for public housing services and the physical deterioration of many buildings, there are almost no transfers for reasons other than building closure or rehabilitation (Greg Russ, 2000).

Closure decisions were clearly linked to the physical condition of the buildings, although the relationship was not always straightforward. For example, all of the Robert Taylor high-rises were built at the same time in the same style, are in similarly poor condition and all are slated for demolition over the next 15 years. However, the closures to date have been driven largely by chance events such as pipes bursting in one building rather than another. The comprehensive redevelopment plan devised for the Cabrini-Green development included plans for the demolition of high-rises in the North extension, starting on the East side of the development simply because it was adjacent to the wealthier business district. A final example involves the Wells Extension mid-rises. In the early nineties, the CHA intended to rehabilitate all ten of the buildings, but ran out of money after completing six so that the remaining four mid-rises had to be closed and demolished. According to tenants and CHA officials, there was no clear reason why the CHA chose to begin with those particular buildings. Finally, I have explicitly excluded cases where there is good reason to believe that the demolition may have been correlated with unobservable tenant characteristics that could influence achievement (e.g., the "Hole").

IV. The Reduced-Form Effects of Public Housing Closures

Table 2 shows the effect of the public housing closures on sample attrition along with a variety of relocation and educational outcomes three years after the initial notification. Note that if the students were randomly assigned to treatment or control buildings, then we would not expect to see any differences between the estimates in columns (2)–(4), which is generally the case.

The top panel addresses the issue of sample attrition. We see that attrition rate was identical for students from demolished and nondemolished buildings, with roughly 6.1 percent of children leaving the ChiPS for a private school or moving out of the district. Among children younger than 14 years of age at the time of the closure announcement (the sample for whom most achievement measures are available), treatment and comparison students were equally likely to be enrolled in school three years later. More importantly, there is no difference in the likelihood of having missing test score or transcript data across the two groups. Hence, for the remainder of the paper, the achievement estimates will focus only on these younger students.

It appears that the demolitions had a small, negative effect on the educational attainment of older children in public housing (i.e., those 14 years or older at the time of the closure announcement). These students in demolished buildings were roughly 4.4 percentage points (8.2 percent) more likely to have dropped out of school within three years than their peers in control buildings. In contrast, a comparison of educational outcomes among younger children indicates that the public housing closings had no statistically or economically significant impact on student achievement. The point estimates in column (4) suggest that the demolition effect was only 0.2 and 0.1 percentile points for

TABLE 2—THE IMPACT OF PUBLIC HOUSING CLOSURES THREE YEARS AFTER INITIAL NOTIFICATION

Dependent variable	Control mean (1)	Difference: Demo − Control (no controls) (2)	Difference: Demo − Control (controls) (3)	Difference: Demo − Control (controls) (4)
Sample attrition				
Left the ChiPS (i.e., transferred to a private school or moved out of the district)	0.061	0.000	0.000	0.000
	(0.239)	(0.002)	(0.006)	(0.006)
Enrolled in school (ages 3–13)	0.899	−0.011	−0.006	−0.007
	(0.277)	(0.007)	(0.007)	(0.007)
Missing transcript outcomes (ages 3–13)	0.823	0.000	0.000	0.000
	(0.382)	(0.006)	(0.006)	(0.006)
Missing test score outcomes (ages 3–13)	0.335	0.004	0.000	0.000
	(0.472)	(0.009)	(0.009)	(0.009)
Educational outcomes				
Dropped out (ages 14+)	0.538	0.048*	0.043**	0.044**
	(0.499)	(0.025)	(0.022)	(0.021)
Math score (percentile rank)	31.6	−0.474	−0.401	0.200
	(24.8)	(0.858)	(0.835)	(0.762)
Reading score (percentile rank)	27.6	−0.196	−0.159	0.099
	(21.0)	(0.246)	(0.730)	(0.680)
Old for grade	0.330	−0.005	−0.006	−0.005
	(0.470)	(0.014)	(0.012)	(0.012)
Absences (per course)	14.8	0.38	0.41	0.57
	(11.4)	(0.96)	(0.97)	(0.98)
Credits	25.4	0.49	0.42	0.35
	(4.7)	(0.28)	(0.28)	(0.28)
GPA	1.55	0.017	0.010	−0.017
	(0.93)	(0.069)	(0.067)	(0.064)
Relocation outcomes				
Living in public housing	0.615	−0.205**	−0.204**	−0.200**
	(0.449)	(0.012)	(0.012)	(0.012)
Years in public housing since closure announcement	2.19	−0.596**	−0.594**	−0.576**
	(1.07)	(0.032)	(0.032)	(0.032)
Miles from original residence	1.45	1.29**	1.29**	1.26**
	(2.56)	(0.08)	(0.08)	(0.08)
Census-tract poverty rate	0.676	−0.147**	−0.146**	−0.143**
	(0.269)	(0.008)	(0.008)	(0.008)
Changed schools since notification	0.576	0.172**	0.172**	0.170**
	(0.494)	(0.013)	(0.013)	(0.012)
Number of school moves[a]	1.45	0.01	0.01	0.01
	(0.69)	(0.03)	(0.03)	(0.02)
Percent school peers met norms in math	0.274	0.001	0.002	−0.002
	(0.112)	(0.003)	(0.003)	(0.003)
Controls for student demographics	—	No	Yes	Yes
Controls for prior achievement	—	No	No	Yes

Notes: The sample sizes for the attrition and drop-out estimates are 10,556 for the full sample, 7,635 for the younger children (age 3–13), and 2,251 for the older children (age 14–21). The education and relocation estimates are based on the sample of younger children still enrolled three years after the initial notification ($n = 6{,}681$), but the number of observations varies because of missing data and because some outcomes are only available for elementary or secondary students. Eicker-White robust standard errors that account for correlation within students are shown in parentheses. The estimates in column (2) include fixed effects for building, announcement year, and grade at announcement. The estimates in column (3) include the following additional controls: age, gender, living with at least one parent, living in foster care, special education, free lunch, and old for grade. The estimates in column (4) include all of the controls from columns (2) and (3) as well as controls for student achievement in the year prior to the closure notification, including math score, reading score, GPA, credits, and absences (imputed values and indicators for missing data are included for students without prior achievement measures).

[a] Conditional on having changed schools.

* Significant at the 10-percent level.

** Significant at the 5-percent level.

math and reading, respectively, which corresponds to effect sizes of roughly 0.01.[24] There was similarly little effect on high school outcomes. The point estimates for absences, course credits, and GPA were extremely small (generally less than 0.05 standard deviations) and never statistically significant.

How might one explain these results? As discussed earlier, there are several pathways through which the demolitions could operate including a change in public housing participation, neighborhood poverty, or school quality. While I cannot tell exactly which families utilized Section 8 vouchers, school records allow me to determine where students were living and what schools they were attending. Using this information, the bottom panel explores the impact of the demolitions on a variety of relocation outcomes.

We see that students affected by the demolitions were significantly less likely to be living in public housing three years following the closure announcements. Interestingly, only 61 percent of control students were still living in public housing three years after the announcement, perhaps reflecting the healthy economy during the late 1990's or the anticipation of future demolitions. Still, living in a building slated for closure decreased the probability of living in public housing by roughly 20 percentage points (33 percent). While the difference is highly significant, the magnitude of the effect suggests that relatively few treatment families took a Section 8 voucher, choosing instead to transfer to another public housing unit. This finding is consistent with prior research on housing vouchers (Goering et al., 1995; John Hartung and Jeffrey Henig, 1997; Newman, 1997; Turner, 1998; Turner et al., 1998; Mary Cunningham et al., 1999; Paul Fischer, 1999, 2003) as well as with the results of recent housing mobility experiments (e.g., approximately 50 percent of families in the MTO program utilized a voucher when given the opportunity. See Goering et al., 1999).

While treatment students were more likely to move out of public housing, they relocated to neighborhoods relatively close to their original residence. After three years, these students were living on average only 1.25 miles further from their original residence than their peers in control buildings. While the average neighborhood poverty rate was somewhat lower for the treatment group (53 percent) than the comparison group (68 percent), it is important to note that treatment students were still living in very poor neighborhoods. Less than 3 percent of treatment students were living in low-poverty tracts (i.e., lower than 10-percent poverty), and only 30 percent were living in moderate-poverty tracts (i.e., 10- and 40-percent poverty). Moreover, conditional on living in public housing, there was little difference in the census-tract poverty rates of treatments versus controls. This suggests that the differences in neighborhood poverty are directly associated with leaving public housing—that is, families in demolished buildings did not take the opportunity to transfer to other public housing units in lower poverty neighborhoods.

As one would expect given the marginal changes in residential location, children from demolished buildings do not appear to be attending significantly different schools than their peers in nondemolished buildings. Treatment students were 17 percentage points (30 percent) more likely to have switched schools since the closure announcement. Among those students who had switched schools at least once, however, the treatment group did not change schools more often than the comparison group, suggesting that they did not experience ongoing disruption. Most importantly, three years after the closure notification, treatment and comparison students appear to be attending identical schools in terms of average peer achievement—i.e., both groups are attending schools in which approximately 27 percent of students met national norms in mathematics.[25]

A. *The Impact of Closures Over Time*

Insofar as the initial relocation and school changes were disruptive, one might expect the impact of the closures to vary over time. If there were initial negative effects followed by steady improvements, then one might speculate that the demolitions would result in improved achievement in the long run. To examine the effects of

[24] This is based on student-level standard deviations within grade level for all students in the ChiPS.

[25] These schools are similar along other dimensions as well (i.e., reading scores, racial composition, size, etc.).

TABLE 3—THE EFFECTS OF PUBLIC HOUSING CLOSURES OVER TIME

	Difference: Demo − Control				
	Year 1	Year 2	Year 3	Year 4	Year 5
Sample attrition (n = 7,483)					
Left the ChiPS (i.e., transferred to private school or moved out of the district)	0.002	−0.002	0.001	0.005	0.001
	(0.005)	(0.006)	(0.007)	(0.008)	(0.009)
Enrolled in school (ages 3–13; n = 5,603)	−0.001	−0.002	−0.007	−0.006	−0.005
	(0.006)	(0.008)	(0.009)	(0.011)	(0.011)
Missing test score outcomes (ages 3–13; n = 5,603)	0.031	0.030	0.000	0.016	0.004
	(0.011)	(0.012)	(0.011)	(0.012)	(0.011)
Educational outcomes (n = 3,889)					
Dropped out (age 14+, n = 1,678)	0.036	0.047*	0.067**	0.085**	0.074**
	(0.024)	(0.027)	(0.027)	(0.027)	(0.026)
Old for grade	−0.002	0.003	0.000	0.005	0.005
	(0.010)	(0.016)	(0.018)	(0.019)	(0.019)
Math score (percentile rank)	−0.041	−0.326	−0.079	0.364	0.489
	(1.110)	(1.142)	(1.104)	(1.131)	(1.154)
Relocation outcomes (n = 3,889)					
Living in public housing	−0.100**	−0.185**	−0.174**	−0.165**	−0.111**
	(0.016)	(0.020)	(0.020)	(0.020)	(0.019)
Census-tract poverty rate	−0.058**	−0.106**	−0.107**	−0.109**	−0.081**
	(0.009)	(0.010)	(0.011)	(0.011)	(0.011)
Changed schools since notification	0.157**	0.196**	0.192**	0.179**	0.123**
	(0.020)	(0.020)	(0.020)	(0.018)	(0.016)
Percent school peers met norms in math	−0.001	0.006	0.001	0.004	0.003
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Controls for student demographics	Yes	Yes	Yes	Yes	Yes
Controls for prior achievement	Yes	Yes	Yes	Yes	Yes

Notes: The sample for this analysis includes students in public housing developments that experienced closures before 1997. The top panel includes all students in the relevant buildings immediately prior to closure notification (n = 7,483). The achievement and relocation estimates in the bottom two panels include the subsample of younger children (3–13) who were still enrolled five years after notification (n = 3,889). The difference in sample size is due to students who graduated, dropped out, or left the system. The number of observations varies across the dependent variables because certain outcomes are only available for elementary or secondary students. Eicker-White robust standard errors that account for correlation within students are shown in parentheses. The controls included are the same as those in column (4) of Table 2.

* Significant at the 10-percent level.

** Significant at the 5-percent level.

public housing closings over time, Table 3 presents estimates of the treatment effects for the first five years after initial notification for a sample of students in buildings with closure notification in 1996 or earlier (n = 7,483). By restricting the sample in this way, composition changes are less likely to contaminate any trends. These results, however, are not directly comparable to the results presented in Table 2 since the latter included all students.[26]

The results shown in the top panel indicate that building closures did not have a significant impact at any point on the likelihood of students leaving the ChiPS. The second panel shows that the effect of closure on residential and school mobility rises in the first few years, but then levels off by year three. For example, one year following the notification treatment students were 10 percentage points less likely to be living in public housing than control students.[27] This difference rose to

[26] To test the sensitivity of these results to the most recent demolitions, I reestimated all specifications using as control students only those in buildings not scheduled for demolition until 2004 or later and found comparable results.

[27] The bottom two panels show the relocation and educational outcomes for younger children who remained in the ChiPS (i.e., did not drop out, transfer to private school, or move out of the school district) for the entire five-year period

TABLE 4—EFFECTS OF PUBLIC HOUSING CLOSURE ON STUDENT OUTCOMES BY STUDENT CHARACTERISTIC

	Age at time of announcement			Gender		Test score at time of announcement		
Dependent variable	3–8 (1)	9–13 (2)	14–21 (3)	Boys (4)	Girls (5)	<15 percentile (6)	15–30 percentile (7)	>30 percentile (8)
Dropped out (age 14+)			0.044**	0.026†	0.058**	0.044	0.021	0.073*
	—	—	(0.021)	(0.031)	(0.029)	(0.035)	(0.042)	(0.043)
			[0.538]	[0.599]	[0.480]	[0.610]	[0.529]	[0.377]
Old for grade	−0.001	−0.010		−0.004	−0.002	−0.028	0.032	−0.019
	(0.016)	(0.019)	[b]	(0.018)	(0.016)	(0.029)	(0.034)	(0.020)
	[0.294]	[0.382]		[0.372]	[0.287]	[0.521]	[0.343]	[0.199]
Math score	−0.096	1.035		−0.536	0.891	0.623	0.305	1.121
(percentile rank)	(0.975)	(1.112)	[b]	(1.107)	(1.059)	(1.251)	(1.771)	(1.473)
	[32.4]	[29.6]		[29.6]	[33.7]	[17.0]	[31.1]	[48.0]
Absences (per course)		0.570		0.30	0.95	−0.67	−0.36	2.09
	[a]	(0.976)	[b]	(1.56)	(1.26)	(1.80)	(1.83)	(1.56)
		[14.8]		[15.6]	[14.1]	[15.8]	[14.6]	[13.7]
GPA		−0.017		−0.026	−0.023	0.08	0.01	−0.13
	[a]	(0.065)	[b]	(0.010)	(0.085)	(0.10)	(0.14)	(0.11)
		[1.56]		[1.35]	[1.74]	[1.27]	[1.51]	[1.94]

Notes: Each cell contains an estimate of the treatment effect of building closure on the dependent variable listed in the first column and the associated Eicker-White standard errors that account for correlation within students (in parentheses) and the comparison group mean for the dependent variable (in square brackets). Each column represents a different sample of students. All achievement estimates (other than drop-out rates) are based on children age 3–13 at the time of the closure announcement. Sample sizes differ across dependent variables as well as estimation sample.

[a] High school transcript outcomes (absences, credits, GPA) are not available for elementary school students.

[b] Achievement estimates are not shown for the oldest group because of differential sample attrition.

* Significant at the 10-percent level.

** Significant at the 5-percent level.

† Difference in the treatment effect across the subsamples is significant at the 5-percent level.

roughly 20 percentage points in year two, but actually declined somewhat by years four and five. This pattern is repeated for census-tract poverty rates and distance from initial residence.[28] The impact on school quality does not change over this five-year period.

For older children, the drop-out effect seen in the baseline results appears to increase over the first few years but then level off by year four or five, reinforcing the notion that the relationship between demolitions and dropouts operates largely through the disruption associated with the initial relocation. For younger children, however, there is no statistically significant difference between the two groups at any point on any of the achievement measures. The fact that there is no trend in the achievement effects suggests that the disruption of the closures may not have had a significant influence on the academic achievement of these children (perhaps not surprising given the other barriers already facing many children from low-income families in public housing).

B. *The Impact of Closure by Student Characteristics*

While public housing closures do not appear to have much of an effect on student outcomes in aggregate, it is possible that they may have significant effects for certain subgroups. Table 4 examines closure effects by gender, age, and ability at the time of the announcement.[29] With

(n = 3,889). The number of observations varies across the dependent variables because certain outcome variables are only applicable to elementary or secondary students.

[28] This may be due to treatment families moving back into public housing, a phenomenon that occurred in the MTO program (Susan Popkin et al., 2002).

[29] Achievement results are not shown for children in the 14–21 age group because of the selection bias stemming from differential drop-out rates.

regard to gender, the building closures appear to have a larger effect on the likelihood of dropping out for girls than for boys. The demolitions increased drop-out rates for girls by 5.7 percentage points or 12 percent compared with only 2.6 percentage points or 4 percent for boys (with the difference significant at the 5-percent level). On the other hand, there are no significant gender differences in the achievement effects for younger children. In fact, the point estimates for math scores suggest that girls may have fared slightly better than boys as a result of the demolitions. There is no clear pattern in the effects by age or prior achievement level—the point estimates for all groups are substantively small and the differences across subgroups are not close to significant at conventional levels.[30]

C. *The Impact of Closure By Development*

Another reason that we might find few positive effects involves unobserved heterogeneity relating to the nature of the closings. Insofar as the public housing closings and demolitions over this period differed widely in both their underlying causes and the processes by which they were carried out, it is reasonable to expect that some of the closings may have benefited residents whereas the closings in other situations hurt the residents. Such a situation could lead to a zero average effect. For example, residents in buildings that were closed as part of planned redevelopment activities might have received more notice and thus been able to negotiate the relocation more successfully. Alternatively, residents that experienced emergency building closures may have been less likely to find another CHA unit for transfer and thus more likely to have left public housing.

Because there were a number of changes within the CHA during the 1990's, it is possible that the building closures were carried out differently depending on the year, and that these differences influenced the experiences of families. For example, by all accounts, CHA was in extreme disarray in the late eighties and early 1990's, both financially and administratively. HUD took control of CHA in 1995 and instituted a number of changes in the management and financial systems. In addition, the Section 8 program in Chicago was reorganized and contracted out to a private organization in 1995–1996. By the end of the nineties, the City had assumed control of day-to-day operations in CHA, and the agency appeared to be running more smoothly than in the past. Given this history, it is likely that residents living in buildings closed in earlier years had fewer opportunities to relocate out of public housing with Section 8. In contrast, by the time the 1998–1999 closings occurred, the Section 8 program for relocatees was better organized and more widely known by tenants.

To explore these potential forces, Table 5 examines the treatment effects across closure year and development. Columns (1) and (2) show the effect of the closings separately for closures announced in 1995–1996 and 1998–1999, respectively. Outcomes are measured three years after the announcement year. We see that children affected by the later closings were more likely to leave public housing and, consequently, relocated in slightly less impoverished neighborhoods in comparison to children affected by the earlier closings. This is consistent with the fact that the CHA and Chicago Housing Authority Corporation (CHAC) were better organized in the late 1990's. It is important to keep in mind, however, that in both cases, the treatment students were still on average living in highly impoverished neighborhoods relatively close to their original residence and neither the earlier nor later group appear to be attending significantly better schools as a result of the relocations. Turning to the education outcomes, we see that the earlier demolitions increased drop-out rates by 6.7 percentage points while the later demolitions appear to have had no effect on the likelihood of leaving school. In contrast, there are no significant differences in the achievement effects for younger children in the earlier versus later closures.

Columns (3) and (4) show the results for demolitions that were planned and unplanned,

[30] Prior achievement is measured by the average math and reading score in the year prior to the closure announcement. Because many children in the 14–21 age group did not take the exams in this year, we use an average of math and reading tests in prior years to impute an ability level for these students as well as others who were missing achievement scores from the previous year.

TABLE 5—THE EFFECT OF BUILDING CLOSING ON STUDENT OUTCOMES BY CLOSURE CHARACTERISTIC

	Earlier closures (pre-1996) (1)	Later closures (post-1996) (2)	Unplanned closures (3)	Planned closures (4)
Sample attrition				
Left the ChiPS (i.e., transferred to private, or moved out of the district)	0.001	0.001	0.002	−0.002
	(0.007)	(0.010)	(0.008)	(0.008)
Enrolled in school (ages 3–13)	−0.007	−0.008	0.000	−0.017*
	(0.009)	(0.010)	(0.009)	(0.010)
Missing test score (ages 3–13)	0.000	0.006	0.010	−0.009
	(0.011)	(0.013)	(0.011)	(0.013)
Educational outcomes				
Dropped out (ages 14+)	0.067**	0.005†	0.047*	0.051
	(0.027)	(0.034)	(0.026)	(0.032)
Math score (percentile rank) (ages 3–13)	−0.442	1.331	1.601	−1.530
	(1.036)	(1.126)	(0.977)	(1.110)
Old for grade (ages 3–13)	0.008	−0.014	−0.010	0.008
	(0.016)	(0.018)	(0.016)	(0.017)
Absences (per course) (ages 3–13)	1.056	−0.407	1.337	−0.164
	(1.426)	(1.262)	(1.308)	(1.402)
Relocation outcomes				
Living in public housing	−0.189**	−0.293**	−0.215**	−0.177**
	(0.018)	(0.015)	(0.015)	(0.179)
Miles from original residence	1.38**	1.03**	1.08**	1.49**
	(0.11)	(0.08)	(0.10)	(0.13)
Census-tract poverty rate	−0.108**	−0.152**	−0.163**	−0.119**
	(0.010)	(0.010)	(0.010)	(0.010)
Percent school peers met norms in math	0.003	0.000	−0.003	−0.001
	(0.003)	(0.004)	(0.004)	(0.004)
Number of treatment students	1,774	1,752	2,014	1,480
Number of control students	5,709	1,321	7,030	7,030

Notes: Each cell contains an estimate of the treatment effect of building closure on the dependent variable listed in the first column and the associated Eicker-White standard errors that account for correlation of errors within students in parentheses. Each column provides estimates for a different sample of students. All achievement estimates (other than drop-out rates) are based on children age 3–13 at the time of the closure announcement. The number of observations reflected in each cell varies across the dependent variables because certain outcomes are only available for elementary or secondary students. All models include development and year fixed effects as well as the full set of controls used to generate the estimates shown in column (4) of Table 2. Planned closures are defined as those in which there was at least a year between notification and vacancy; in unplanned closures vacancy occurred less than a year after the notification.

* Significant at the 10-percent level.

** Significant at the 5-percent level.

† Difference in the treatment effect across the subsamples is significant at the 5-percent level.

respectively.[31] There is little difference in the relocation outcomes across these groups, suggesting that the speed of the closure did not play a large role in whether families were living in private housing in the long run. Similarly, there are no significant differences in the achievement effects across these groups.

[31] For the purpose of this analysis, planned closures are defined as those in which there was at least one year between notification and vacancy. This definition is broadly consistent with the reports of CHA residents and officials.

V. The Impact of Public Housing on Student Outcomes

We have seen that the demolitions led to a small increase in the likelihood of dropping out among older students, but had no significant impact on the academic achievement of children under the age of 14. While these reduced-form estimates of public housing closures are useful from a policy perspective, it may also be possible to use the demolitions as an instrumental variable to estimate the effect of living in

public housing (relative to subsidized private housing) on student outcomes. As discussed earlier, in order for the demolitions to be a valid instrument, they must be correlated with public housing participation, but not directly related to student achievement.

The demolitions clearly satisfy the first criterion—students in demolished buildings were 20 percentage points less likely than comparable peers to be living in public housing three years following the closure announcement (see Table 2). As for the second criterion, the quasi-random nature of the demolitions suggests that building closures were plausibly uncorrelated with unobserved child or family characteristics that may independently influence educational outcomes. Still the demolitions might influence student outcomes through several different pathways. Strictly speaking, this means that in order for the demolitions to be a valid instrument for public housing participation, the mobility, neighborhood, school, and relocation pathways described earlier must have a zero effect. In the previous section, we saw that children from demolished buildings were attending virtually identical schools as their peers in control buildings, suggesting that school factors could not have contributed to the demolition impact. The comparison of short- and long-run demolition effects in Table 3 indicates that the impacts did not change much over time, suggesting that the initial disruption had little effect on student achievement.

On the other hand, the demolitions did result in a modest decline in neighborhood poverty, although this change was largely associated with a shift from extremely high-poverty tracts (e.g., 70 percent) to only high-poverty tracts (e.g., 55 percent). If such changes in neighborhood poverty improve educational outcomes, then one might worry that the IV estimates of β_P will be biased downward, and therefore overstate any negative effects of public housing.[32] Insofar as I find small and insignificant negative effects, this bias simply reinforces the conclusion that public housing itself may not have a negative effect on academic achievement.[33]

Table 6 presents estimates of the impact of public housing (relative to subsidized private housing) on educational outcomes. Note that the estimates in the rows 1–3 are based on the specifications in (3′) and (4′) where the public housing variable is defined as the number of years living in public housing since closure announcement while the estimates for the high

TABLE 6—THE RELATIONSHIP BETWEEN PUBLIC HOUSING PARTICIPATION AND STUDENT ACHIEVEMENT

Outcome variables	First stage	OLS	2SLS
Math score	−0.600	0.055	−0.331
(percentile rank)	(0.037)	(0.281)	(1.261)
	$F = 263.1$		
Reading score	−0.600	−0.236	−0.163
(percentile rank)	(0.037)	(0.248)	(1.127)
	$F = 263.1$		
Old for grade	−0.597	0.004	0.008
	(0.032)	(0.005)	(0.020)
	$F = 351.2$		
Absences (per	−0.197	1.186	−2.890
course)	(0.032)	(0.846)	(5.018)
	$F = 36.6$		
Credits	−0.197	−0.065	−1.791
	(0.032)	(0.291)	(1.452)
	$F = 36.6$		
GPA	−0.197	−0.048	0.088
	(0.032)	(0.061)	(0.328)
	$F = 36.6$		

Notes: The estimates in rows 1–3 are based on the specifications in (3′) and (4′) where the public housing variable is defined as the number of years living in public housing since closure announcement. The estimates for the high school transcript outcomes in rows 4–6 come from equations (3) and (4) where public housing is a binary variable indicating whether the student was living in public housing in year *t*. In both cases, outcomes are measured three years after the closure announcement, corresponding to the estimates in Table 2. See the text for more detailed discussion. Eicker-White standard errors that account for correlation of errors within students are shown in parentheses.

[32] Note that I could have chosen to instrument for neighborhood poverty as well, but chose to scale by public housing choice because that was the relevant choice variable for families.

[33] Alternatively, one might simply choose to interpret the IV estimates as the impact of public housing participation (i.e., one's physical housing structure) and the associated poverty in the immediate neighborhood. In Chicago, many of the high-rise public housing developments comprise entire tracts, making it impossible to distinguish between exiting public housing and changing tracts and therefore neighborhood poverty. Moreover, as noted earlier, the decrease in neighborhood poverty was driven almost entirely by the transfer out of public housing.

school transcript outcomes in rows 4–6 come from equations (3) and (4) where public housing is a binary variable indicating whether the student was living in public housing in year *t*.[34] The OLS estimates suggest that living in public housing has no effect on student performance, but may be biased for several reasons. On one hand, families whose income increased over this period may have been able to leave public housing, in which case one might think that the estimates are biased upward. On the other hand, families who were evicted from public housing (e.g., for drug violations), or who were more mobile, likely spent less time in public housing as well, which might be expected to bias the estimates downward.

However, the 2SLS estimates that use the exogenous variation in public housing participation generated by the demolitions also suggest that public housing participation has no significant effect on student achievement. For the outcomes in rows 1–3, the coefficient on the demolition indicator in the first-stage regression is −0.600 with a standard error of 0.037 and corresponding *F*-statistic of 263.1 ($p < 0.000$). This suggests that students in demolished buildings lived in public housing on average 0.6 fewer years than students in control buildings. The point estimates in the second stage suggest that each additional year spent in public housing is associated with a decline of 0.33 and 0.16 percentile points on the math and reading exams respectively. To gain a sense of the magnitude of the effects, note that the average math score was 31.8 with a standard deviation of 24.9 in the third year after the closure announcement (and the standard deviation of the annual gain for this year was roughly 19.1 points).[35]

Insofar as these estimates may overstate any negative effects of public housing, the results suggest that public housing certainly does not have a substantial negative impact on educational outcomes. Given the precision of the 2SLS estimates, we can rule out achievement effects of roughly +/− 2.5 percentile points (or 0.1 standard deviations) per year. While this is a fairly large effect, it may be informative given that the high-rise public housing developments included in the sample, such as the Robert Taylor Homes, are regarded as among the worst in the country. Nonetheless, it is important to recognize that these estimates cannot rule out smaller (positive or negative) effects of public housing. Because of the considerably smaller sample sizes, the estimates for the high school transcript outcomes are even less precise. Note, however, that the direction of the point estimates does not suggest a consistent positive or negative effect of public housing.

VI. Conclusion

This paper examines the impact of public housing demolitions on educational outcomes. I find that the demolitions in Chicago during the 1990's led to a small increase in the likelihood of dropping out among older students, but had no significant impact on the academic achievement of children under the age of 14. These results suggest that demolishing high-rise public housing and providing households the option of using housing vouchers to relocate to different neighborhoods will not necessarily produce better (or worse) educational outcomes for poor children.

Consistent with recent housing mobility experiments such as MTO, I find that a large proportion of families did not take advantage of the relocation opportunity provided by public housing closings to move to a substantially different neighborhood, and even those children who did move to substantially better neighborhoods did not end up in significantly better schools.[36] On the other hand, these

[34] The models with transcript outcomes are specified differently because (a) there are no predemolition measures for these outcomes, and (b) these outcomes—absences, credits, and GPA—are not cumulative measures but rather reflect student performance in year *t*.

[35] In terms of standard deviation units, if we standardize within grade level, the point estimates for math and reading are −0.022 and −0.014.

[36] The reluctance of Section 8 families to leave familiar neighborhoods and the difficulty of relocating to low-poverty areas is well documented in the Section 8 literature. Popkin and Mary Cunningham (1999, 2000) list a number of barriers to successfully leasing an apartment in the private market, including costs (of transportation, credit checks, security deposits), limited time to search, large family sizes (which limit apartment options), personal problems (lack of communication skills, substance abuse, criminal backgrounds, illness, disability), and landlord discrimination. In personal interviews with the author,

findings suggest that it may not be appropriate to generalize some of the benefits documented in housing mobility experiments such as Gautreaux or earlier MTO studies to the general population of low-income households. One reason for this difference may be that families affected by the demolitions in Chicago were not volunteers who sought out the housing voucher opportunity.[37]

The findings from this study also shed light on the impact of high-rise public housing on educational outcomes. Specifically, these results provide support for the Currie and Yelowitz (2000) finding that public housing does not have an independent negative impact on student performance. While students impacted by the closures did not move far from their original neighborhoods, they were considerably less likely to live in public housing following the closures. Yet these students had no better educational achievement and attainment than comparable peers who were living in buildings not directly impacted by the closures and were thus more likely to continue living in public housing. This is particularly noteworthy since unlike the Currie and Yelowitz (2000) study, which included a cross section of public housing, the sample in this study includes what is considered the worst public housing in the country.[38]

In conclusion, it is worthwhile noting that while it appears that the public housing closures in Chicago did not academically benefit children in public housing, they may have influenced other youth or adult outcomes such as employment or criminal activity. Moreover, the redevelopment might be desirable for a number of other reasons, including the removal of unsafe dwellings, the construction of new, mixed-income developments, and the economic growth associated with the redevelopment. More research is needed to determine the consequences of dismantling the system of high-rise public housing that has been such a large part of social welfare policy in this country for the past half century.

public housing residents cited a variety of reasons for not choosing the Section 8 option, including the low quality of affordable housing in the private market, the additional expense of Section 8 (the program requires the resident to contribute up to 30–40 percent of their monthly income toward rent), the location and convenience of public housing, the existing network of friends in public housing, and the uncertainty of the Section 8 program.

[37] One alternative explanation involves the difference in cities. The earlier MTO studies documenting benefits for Section 8 comparisons focus on Baltimore and Boston, cities with different housing markets, patterns of residential segregation, and public housing quality. I thank Jeff Kling for suggesting this possibility.

[38] Insofar as this study examined the impact of relatively short-term changes in public housing participation (i.e., from one to five years), it is still plausible that long-term exposure to public housing (e.g., growing up for 10 or 15 years in a public housing development) has a substantial impact on educational outcomes. However, Oreopoulos (2003) finds that there are no long-run labor market consequences of growing up in a poor neighborhood.

TABLE A1—DEFINITION OF VARIABLES

Variables	Data source	Definition
Demographics		
Student demographics (race, gender, birthdate, household composition, free or reduced lunch, special education)	ChiPS	Taken directly from student records. Household composition is drawn from information on the student's guardian, which varies by semester. I use the semester prior to the notification date.
Public housing status		
Residence in public housing and/or high-rise public housing	CHA	High-rises are defined as buildings with at least 75 units (most often over 100 units). Annual (high-rise) public housing and high-rise residence are defined as the fraction of the year the student lived in (high-rise) public housing (0, 0.5 or 1, corresponding to 0, 1, or 2 semesters in that academic year).
Neighborhood and school characteristics		
Neighborhood poverty rate	Census	From the 1990 Census data. Based on the census tract in which the student was living. The annual poverty rate is the mean of fall and spring rates.
Percent school peers meeting national norms in math	ChiPS	From school-level records.
Miles from original residence	Census	Indicates the distance between the residential census tract at the time of the closure announcement and the current census tract in any year. Distances are measured between the centroids of the tracts.
Mobility		
Residential and school mobility	ChiPS	Residential mobility is based on changes in home address and school mobility is based on changes in current school, both of which are contained in the student records. Because data are only available once per semester, the estimates of residential and school mobility may be understated. For example, if a student changed residences or school after September but had returned to her original home address or school by the following May, then moves will not be recorded. Since I only have data on three time points during the calendar year (September, May, and the following September), the maximum number of moves is two.
Educational outcomes		
Old for grade	ChiPS	A student is considered old for grade if Age (in September) $>$ Grade + 6.5. Students in nongraded classrooms received a missing for this variable.
Math and reading scores	ChiPS	From student test files. These variables are measured in terms of national percentile ranks.
GPA, absences, and credits	ChiPS	From high school transcript files. GPA is a measure of cumulative high school GPA measured in May of the academic year (i.e., GPA in 1994 is the GPA from May 1994, referring to the 1993–1994 academic year). Absences refer to the average number of days missed per course in that academic year. Credits refer to the total number of credits earned in that academic year.
Left the district, dropped out, and enrollment status	ChiPS	Student records provide reasons why the student has left the ChiPS, including transfer to a private school (code = 32), moved to another district (code = 33), graduated (code = 45). Student records also provide separate information regarding whether a student is actively enrolled in any one semester. In some cases, a student who has not left the ChiPS may not be enrolled due to illness, excessive absence, etc.

Appendix B: Determination of Notification and Closure Dates

Data on building closures and demolitions was gathered from a variety of sources. The CHA provided information on which buildings had been demolished and the date of demolition. However, during this period, a number of other buildings were vacated in preparation for future demolition. For this analysis, it is crucial to not only identify these buildings, but also determine the approximate date tenants were notified of the closure. Consider, for example, the impact of building closures on school mobility. Suppose tenants in a particular development were notified in October 1995 that their building was to be closed in January 1996. Because families moved out between October and January, it is likely that the affected children changed schools at this time as well. If we measure school mobility after January 1996, we will likely understate the impact of the closure. Similar problems arise if we pick an arbitrary date prior to the closure. Suppose we choose to start tracking student mobility one year prior to the official closure date. Because public housing residents are quite mobile, it is likely that at least some of the students in our sample would have changed schools during that year even before the closure was announced. Moreover, children in buildings that were not slated for closure might be just as likely to move as children in the soon-to-be-closed buildings. Therefore, if we begin tracking students significantly in advance of the closure announcement, we will not be able to attribute the mobility to the closure and, more importantly, we may see little difference in student performance by building.

CHA policy requires tenants be notified at least 120 days prior to a building closure. However, this is often a poor approximation for the time at which tenants were aware of building closures. On one hand, a number of buildings were vacated and closed in fewer than 120 days due to emergency maintenance problems, particularly in the winter months. In these cases, tenants were sometimes given as little as a week notice prior to closure. On the other hand, there were instances in which redevelopment had been planned for several years and tenants knew of the impending closures well in advance of the official notification.

In order to identify which buildings had been vacated and to determine the approximate date that residents were notified, I examined the trends in the monthly occupancy rates by building since 1990 provided by the CHA. Because many public housing buildings in Chicago experienced slow declines in occupancy over this decade, I rely on sharp declines in building occupancy followed by vacancy to identify the initial notification date. Figure B1 illustrates occupancy rates and closure announcements for several buildings. Residents of building #9 in the Henry Horner Homes were notified in June 1996; the occupancy rate in the building dropped from 40 percent to 1 percent the next month.

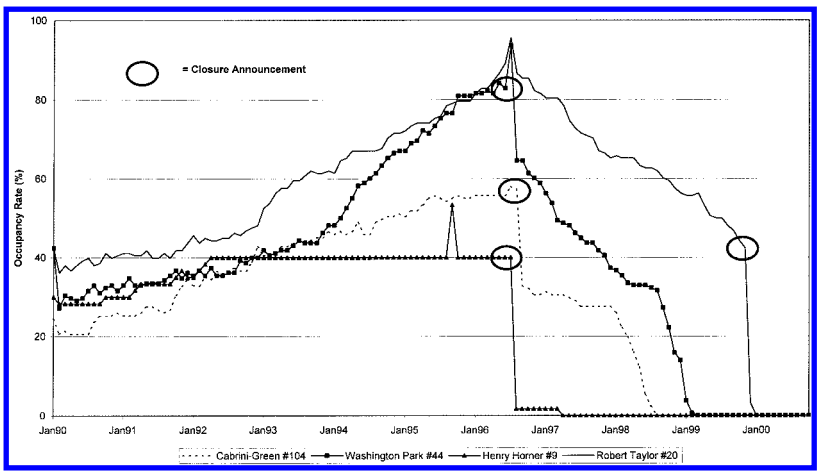

Figure B1. Occupancy Trends and Building Closure

In other cases, such as Cabrini-Green #104 and Washington Park #44, a large fraction of residents left immediately after the announcement (i.e., June 1996 in these cases), but the remaining tenants left over a period of several years. Finally, in many buildings the occupancy rate had been declining for several years prior to the closure, largely because the CHA had stopped assigning new tenants to the building due to maintenance problems. This was the case in Robert Taylor #20, where occupancy steadily declined from June 1996 until October 1999 when the building was vacated and closed over a two-month period in anticipation of winter heating problems. I conduct a similar analysis using the annual public school enrollment by building.

Finally, I supplement these analyses with information from interviews with CHA officials, housing advocates and the presidents of the Local Advisory Councils (LACs) in all 13 of the developments that experienced some building closures during this period. The LAC presidents were particularly helpful in determining the sequences of events in the developments and determining when residents became aware of the closures.

Appendix C: Sample Construction

I create a panel in which each observation corresponds to a student-year, yielding a maximum of 11 observations per student. Note that the observations are based on school enrollment, so that a student in the ChiPS from 1992 to 2002 would have 11 observations, although she may have lived in public housing for as little as one semester or as long as 11 years. Students who entered school after 1992 or left school prior to 2002 will have fewer than 11 observations.

Recall that students are categorized according to the building in which they were living in the semester prior to the initial closure notification. These categorizations are referred to as base groups. Note that students may be in more than one base group if they lived in several different developments that experienced closures during this period. For example, if a family lived in the Robert Taylor Homes in May 1995 and later moved to the Madden Park Homes prior to 1998 when a set of building closures were announced in that development, the family will be included in both the Taylor 1995 and Madden 1998 base groups. Similarly, there may be more than one base group per development. For example, closure announcements in the Taylor Homes took place in 1995 and 1998, each year for a different set of buildings. Therefore, there are two separate Taylor base groups.

Because certain developments experienced a series of building closures at different times, γ_j is actually a vector of development $*$ year effects. In practice, I handle this by expanding the data so that a student's data appears once for each base group (e.g., a student who belongs to two base groups will appear two times in the data set) and then correcting the standard errors to account for this. Just as it is possible for students to be in multiple base groups, it is also theoretically possible for students to be in multiple demolition groups. For example, a student might be living in a building within Washington Park in 1995 when its closure is announced and then move to a building in Robert Taylor that is closed in 1998. Fortunately, there are only six such cases in the data, and the results are obviously robust to excluding these observations.

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