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ABSTRACT

This paper exploits a natural experiment created by public housing closings in Chicago to examine the impact of residential relocation on educational outcomes. During the 1990s, the Chicago Housing Authority closed over 7,400 units of public housing as part of redevelopment and consolidation efforts. Households affected by the closures were offered Section 8 vouchers to move to private housing. Using the home addresses provided in school records, this study matched students to public housing developments and tracked educational outcomes over time for children affected and unaffected by closings. Results found that children in families who were offered the opportunity to relocate from high-rise public housing did no better than their peers. Though these children were considerably less likely to be living in high-rise public housing, they had achievement scores and dropout rates identical to comparison students. These findings suggest that high-rise public housing does not have an independent impact on student achievement and that eliminating high-rise public housing will not necessarily lead to benefits documented in earlier housing mobility experiments. Instead, targeted efforts to move families to substantially better neighborhoods may be necessary to obtain results documented in earlier programs. (Contains 37 references.) (SM)

The Impact of Public Housing Demolitions on Student Achievement in Chicago*

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Abstract

This paper exploits a natural experiment created by public housing closings in Chicago to examine the impact of residential relocation on educational outcomes. During the 1990s, the Chicago Housing Authority closed over 7,400 units of public housing in 12 developments as part of redevelopment and consolidation efforts. Households affected by the closures were offered Section 8 vouchers to move to private housing. Using the home addresses provided in school records, I match students to public housing developments and track educational outcomes over time for children affected and unaffected by the closings. Unlike previous housing mobility studies, I find that children in families who were offered the opportunity to relocate from high-rise public housing do no better than their peers. Despite the fact that these children were considerably less likely to be living in high-rise public housing, they had achievement scores and dropout rates identical to comparison students. These findings suggest that high-rise public housing does not have an independent impact on student achievement and that eliminating high-rise public housing will not necessarily lead to the benefits documented in earlier housing mobility experiments such as Gautreaux and Moving to Opportunity. Instead, targeted efforts to move families to substantially better neighborhoods may be necessary to obtain the results documented in earlier programs.

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1. Introduction

During the 1990s, the Chicago Housing Authority (CHA) closed over 7,400 units of public housing in 12 developments as part of redevelopment and vacancy consolidation programs. Many of these developments, including the Robert Taylor, Cabrini-Green and Henry Horner Homes, had become symbols of urban decay and bureaucratic mismanagement. Redevelopment efforts in Chicago mirror a national movement toward tenant-based housing assistance and mixed-income communities.

A fundamental assumption underlying the redevelopment initiatives in Chicago and other urban areas is that the concentration of poverty in high-rise public housing diminishes the educational and employment opportunities of public housing residents. Households affected by the redevelopment were offered Section 8 vouchers to move to private housing. Relocation might benefit children in public housing for two primary reasons. First, considerable evidence suggests that neighborhood characteristics such as poverty and racial segregation influence individual opportunities (Jencks and Mayer 1990). Two well-known housing mobility experiments—Gautreaux and Moving to Opportunity (MTO)—document that moving children from low-income housing projects to government subsidized private housing in middle class suburbs increases educational attainment (Kaufman and Rosenbaum 1992; Rosenbaum 1995; Ladd and Ludwig 1998; Katz, Kling et al. 2000). Second, some studies contend that high-rise public housing itself has detrimental effects above and beyond the effect of the surrounding neighborhood

In this paper, I examine the impact of recent public housing closings in Chicago on student achievement. The Chicago demolitions are a particularly interesting case to study because, unlike Gautreaux and MTO, they provide a more realistic view of large-scale relocation

programs. While the affected families were given several options for relocation, they did not volunteer to move, were not provided the same levels of support as families in the earlier experiments, and did not face a requirement to move to a low-poverty area.

To measure educational outcomes over time, I combine administrative data from the CHA and the Chicago Public Schools (CPS). I match students to housing developments through home addresses in school records and gather closure information from building level occupancy data provided by the CHA. While the CHA targeted projects for redevelopment on the basis of vacancy rates and physical deterioration, the *timing* of building closures *within* developments is uncorrelated with observable resident characteristics (and plausibly uncorrelated with unobservables). In order to identify the effect of the closings, I exploit the natural experiment generated by the closings and compare students living in CHA units slated for closure with peers living in units in the same development 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.

Unlike previous housing mobility studies, I find that children in households affected by the closures do no better than their peers. Children affected by the closures had achievement scores and dropout rates identical to comparison students, despite the fact that they were considerably less likely to be living in high-rise public housing. This finding suggests that high-rise public housing does not have an independent impact on student achievement.[1] More specifically, I find that (1) a large proportion of families does not take advantage of the relocation opportunity provided by public housing closings to move to substantially different neighborhoods, (2) even those students who did move to substantially better neighborhoods did

[1] It is possible that benefits associated with the opportunity to relocate to better neighborhoods may have been offset by negative effects of the move itself. I examine this possibility in greater detail below.

not end up in significantly better schools, and (3) even those students who ended up in considerably better schools as a result of the public housing closings did poorly in comparison to control students who moved to better schools, presumably as a result of a more voluntary and less disruptive move. These findings suggest that eliminating high-rise public housing will not necessarily lead to the benefits documented in Gautreaux and MTO. Instead, targeted efforts to move families to substantially better neighborhoods may be necessary to obtain the results documented in earlier programs.

The remainder of this paper is structured as follows. Section 2 provides background on federal housing assistance programs and reviews the prior literature on housing voucher programs. Section 3 describes the recent history of public housing in Chicago, highlighting the reasons for and process of building closure during the 1990s. Section 4 outlines my empirical strategy and Section 5 describes the data and sample. Sections 6 and 7 compare treatment and control students before and after the building closures. Section 8 examines several reasons why the public housing closures may not have had a positive impact on student outcomes. Section 9 discusses policy implications and concludes.

2. Background on Housing Assistance Programs

The federal government provides three types of rental housing assistance to low-income households: project-based assistance (federal subsidies tied to specific, often privately-owned, properties), public housing assistance (units owned and operated by local public housing authorities), and tenant-based assistance (housing vouchers or certificates). Over the past two decades, tenant-based programs have become increasingly popular, with about 1.4 million

households nationwide receiving tenant-based assistance in the mid-1990s (Newman 1997).[2] The most common form of tenant-based assistance consists of "Section 8" vouchers, which allows households to rent an apartment in the private housing market.

The relocation opportunity provided by housing vouchers might improve educational outcomes by (1) raising neighborhood quality, (2) raising school quality, or (3) increasing the probability of living in private as opposed to public housing. Neighborhood quality is perhaps the most obvious (certainly the most widely cited) pathway through which public housing closure might influence achievement. There is an extensive literature documenting the association between neighborhood or school characteristics and individual outcomes, including the recent housing mobility experiments that provide strong evidence of a causal impact of neighborhood poverty on a variety of outcomes (Jencks and Mayer 1990; Brooks-Gunn, Duncan et al. 1997; Ellen and Turner 1997; Jargowsky 1997). Even a modest change in neighborhood quality might be associated with substantial improvements in school performance if the child moves to a different school attendance area.[3]

Regardless of the distance of the move, Section 8 relocation allows households to leave public housing, which may influence educational outcomes. 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 Franck 1980; Roncek, Bell et al. 1981; Farley 1982; Dunworth and Saiger 1994; Holzman 1996). One study in Chicago found a relationship between public housing, increased criminal behavior and decreased employment rates in census tracts with high-rise developments,

[2] This is roughly one-third of all low-income renters served by the Department of Housing and Urban Development (HUD).

but found no such relationship in tracts with low-rise developments (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 small positive effect on educational outcomes (Newman and Harkness 1999; Newman, Harkness et al. 1999; Currie and Yelowitz 2000).

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 (Ingersoll, Scamman et al. 1989; Alexander, Entwisle et al. 1994; Kerbow 1996). Unfortunately, there are relatively few studies that examine the impact of housing relocation and those that do exist rarely measure the educational outcomes of children in public housing.

The earliest evidence on housing vouchers comes from the Experimental Housing Allowance Program (EHAP), a massive, federally funded social experiment during 1970s. In a comprehensive review of EHAP findings, Struyk (1981) concluded that housing allowances neither increased mobility nor affected racial or economic segregation. More recent studies show that participants in tenant-based assistance live in less highly concentrated poverty areas and have higher employment rates. However, these studies also highlight important exceptions to the aggregate association between vouchers and neighborhood quality, emphasizing that inner-city and minority voucher recipients tend to relocate in highly segregated and relatively

[3] This is only one mechanism that could generate non-linear neighborhood effects.

high poverty areas that are close to their neighborhood of origin (Goering, Stebbins et al. 1995; Hartung and Henig 1997; Newman 1997; Turner 1998; Turner, Popkin et al. 1998; Cunningham, Sylvester et al. 1999). For example, Fischer (1999) found that almost 80 percent of relocated families in Chicago moved to census tracts that were over 90 percent black and that 90 percent moved to areas with median incomes under $15,000.[4]

The stability of Section 8 households has generated increasing interest in mobility assistance programs.[5] The most well-known housing mobility program—the Gautreaux Program in Chicago—resulted from a lawsuit brought against the CHA and HUD in the early 1970s 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 vouchers to move to private housing in Chicago or nearby suburbs. Since 1976, more than 5,000 families have participated in this program and more than half moved to middle-income white suburbs (Rosenbaum 1995). In a series of studies, Rosenbaum and his colleagues compared the educational and employment outcomes of households that moved within the city ("urban movers") to those who moved to the suburbs ("suburban movers"). Kaufman and Rosenbaum (1992) found that children of suburban movers consistently and substantially outperformed the children of urban movers.

While the Gautreaux experiment illustrates the potential benefits of residential relocation, because participants were not randomly assigned to urban or suburban locations, it is possible that neighborhood effects may be confounded with unobserved participant characteristics. Moving to Opportunity (MTO), a randomized housing-mobility experiment funded by HUD in

[4] Popkin and Cunningham (1999; 2000) listed 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.

five cities, attempts to address these concerns. Low-income families who lived in public housing were randomly assigned 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.

Preliminary results from the MTO program indicate substantial benefits of housing mobility, particularly relocation to low-poverty areas (Goering, Kraft et al. 1999; Ludwig, Duncan et al. 1999; Katz, Kling et al. 2000; Leventhal and Jeanne 2000; Rosenbaum and Harris 2000). Studies in all sites found that the experimental and comparison groups moved to lower poverty areas compared to controls, with the most dramatic differences for the MTO group. Several studies found improved physical and mental health of mothers and children in the MTO group, increased feelings of safety and satisfaction with neighborhood and school, and decreased problem behavior among children. Ludwig et. al. (1999) found that the MTO treatment substantially reduced violent criminal behavior among adolescent boys. At this point, the studies have not documented substantial impacts on maternal employment or student achievement (Katz, Kling et al. 2000; Rosenbaum and Harris 2000).

An important feature of most Section 8 programs including Gautreaux and MTO is that they involve *voluntary* relocation. Only a few studies examine forced relocation. In one such study, Varady and Walker (2000) examined households that received Section 8 vouchers to leave distressed developments in four cities (Baltimore, Newport News, VA; Kansas City, MO, and San Francisco). They found that the majority chose to remain in the same neighborhood and

[5] Today there are 54 mobility programs in 33 different metropolitan areas (Turner 1998; Turner, Popkin et al. 1998).

thus continued to live in racially segregated neighborhoods, though participants did move to census tracts with somewhat higher median household income.

3. Public Housing in Chicago During the 1990s

The Chicago Housing Authority (CHA) was organized in 1937 to provide temporary housing for people unable to obtain "decent, safe and 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 2000).

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. 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, comprehensive 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 (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 1980s, obtained 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 (Hawes 1992; 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 (Garza 1999; Garza 1999; Jackson and Garza 1999).

The pattern of residential relocation also differed considerably across buildings (Figure 1). In certain cases, buildings were closed within several months of the initial notification. For example, residents of building #9 in the Henry Horner Homes were notified in June 1996; the occupancy rate in the building dropped from forty percent to one percent the next month. 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.[6] This was the case in Robert Taylor #20, where occupancy steadily declined from June 1996 until October 1999 when the

[6] One finds similar declines in occupancy rates for buildings that were not closed, the difference being there was no sharp drop to near zero occupancy at any point.

building was vacated and closed over a two-month period in anticipation of winter heating problems.

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 1990s 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.[7] Second, Chicago families were not required to relocate to low-poverty neighborhoods, as was the case for the experimental groups in Gautreaux and MTO.[8] Finally, the Chicago families received considerably fewer support services than the experimental families in Gautreaux and MTO.[9]

4. The Empirical Strategy

The primary goal of this analysis is to estimate the impact of building closure on student achievement, the relevant counterfactual being how a student would have done in the absence of any change in her public housing unit. We cannot reasonably compare students whose buildings

[7] 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.
[8] Certain groups in the mobility experiments—the urban movers in Guatreaux 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 non-movers though not as much as the families who moved to low-poverty neighborhoods.

were closed or demolished to the average CPS student because, as Table 1 shows, students in public housing are systematically different than the average CPS student. For example, over 80 percent of students in public housing receive free lunch compared with 68 percent of the entire student population. Public housing students live in census tracts with poverty rates three times higher than other students, attend schools with substantially fewer high performing peers, score an entire year lower on standardized math and reading exams, and miss 60 percent more days in high school than other students. Moreover, as Figure 2 shows, students in the public housing developments where some buildings were eventually closed (column 3) lived in more disadvantaged neighborhoods than students in other public housing developments, which is consistent with the fact that CHA's redevelopment efforts focused on the most distressed developments.

In order to identify the effect of the closings, 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 development that were not slated for demolition. This comparison assumes that students in "treatment" buildings (i.e., those buildings that were slated for closure or demolition) were no different than students in comparison buildings prior to the closure announcement. To justify this assumption, Section 6 presents evidence suggesting that there are no observable differences between the treatment and control groups. 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

[9] The average cost to counsel each experimental family in Gautreaux and MTO was $555 and $1,455 respectively (Goering, Kraft 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.

worse outcomes in the absence of the public housing closings, thus biasing the estimated policy effect downward.

It is impossible to completely rule out the possibility of unobservable differences. However, the process of tenant assignment, building closure and residential relocation suggests that such differences are unlikely. In order 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. 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 theoretically reject an offer and place their name on a waiting list for a particular development. This rarely occurs in practice because the site specific waiting lists are often longer than the general CHA list which means that gaining a preferred unit could entail an additional wait of up to eight years. 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 (Russ 2000).

Closure decisions were clearly linked to the physical condition of the building, although the relationship was not always straightforward. For example, the Robert Taylor high rises were built at the same time in the same style, are in similarly poor condition and all of the high rises 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. Similarly,

the highly publicized shooting of Dantrell Davis spurred City officials to close several of the high rises in Cabrini-Green despite the fact that many of the buildings in that part of the complex had suffered from gang problems for years. When a comprehensive redevelopment plan was later devised for Cabrini, demolition of high rises in the North extension started 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 10 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.

4.1. Statistical Model

In order to estimate the average treatment effect across developments that experienced building closures at different times, I estimate the following OLS model:

$$(1) \qquad y_{ijt} = (\text{Treatment})_{ijt}\beta + \gamma_j + \delta_t + \varepsilon_{ij}$$

where y_{ijt} is an outcome for student i in development j at time t, and γ_j and δ_t are fixed effects for developments and outcome years respectively. If y is a level outcome, then β simply estimates the difference in means between treatment and control students in year t. If the outcome is a difference or change score, then the treatment coefficient corresponds to the difference-in-difference estimate.

5. The Data

This study combines student and school level data from the CPS with building level data CHA. Administrative data from the CPS includes student records for each semester (fall and

spring) that a student was enrolled in a CPS school from 1991-92 to 1999-2000. These records indicate a student's school, grade, and home address along with other information such as race, gender, legal guardian, and special education status. Administrative data from the CHA lists all public housing developments in the city, including building addresses and the number of units per building. By merging the CPS and CHA data by address, I am able to determine whether a student was living in public housing at a particular time (Appendix A provides a more detailed discussion of the construction of the variables used in this analysis). The full data set is thus a nearly complete census of CPS students who lived in public housing during at least one semester between 1991-92 and 1999-2000. This includes 67,912 students who lived in 1,290 buildings[10] in 33 developments during this period. While these data have detailed information about educational outcomes, they do not include information on other outcomes such as employment, earnings or juvenile delinquency.

The sample used in this analysis is a sub-sample 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[11] 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 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

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

[11] Following the standard practice of the CHA, I define a high rise as any building with at least 100 units (roughly 10 stories high).

estimate the treatment effect.[12] 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. 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 one percent of the sample). The final sample consists of 18,369 students in 114 buildings in 12 developments.

Table 2 describes the public housing experiences of the full data set as well as the analysis sample. Note that this information only refers to periods that the students were also enrolled in the CPS. For example, among the group of students who lived in public housing at least one semester between 1992 and 2000, the average child was enrolled in school for five years (between 1992 and 2000) and lived for just over three years in public housing. Only 52 percent of these students lived in a high-rise building for any period of time, though an additional 16 percent lived in a mid-rise. On average, students lived in three different residences over this period, although they lived in only 1.2 different public housing buildings. Thus, students in this population were mobile, but did not frequently transfer between public housing buildings. (It is likely that many of these students transferred to publicly subsidized housing in the private market through programs such as Section 8 or scattered site housing.) By definition, all students in the analysis sample (columns 3 and 4) lived in a high-rise at some point. In addition, these students spent more total time in public housing and more time in high-rise. Treatment students spent

[12] 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.

less time in public housing and moved more often than control students (4.88 versus 5.39 years in public housing and 2.7 versus 2.38 moves).

I create a panel in which each observation corresponds to a student-year, yielding a maximum of nine observations per student. Note that the observations are based on school enrollment, so that a student in the CPS from 1992 to 2000 would have nine observations, although she may have lived in public housing for as little as one semester or as long as nine years. Students who entered school after 1992 or left school prior to 2000 will have fewer than nine observations. 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. 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.[13] 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 2000) serve as the comparison or control group. 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.

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

Because the analysis strategy relies on the equivalence of treatment and control students, I first compare these groups on a number of observable characteristics. Table 3 compares treatment and control students within housing development in the year prior to the closure announcement by regressing each of the dependent variables on a dummy variable for treatment

group along with fixed effects for development and year (see equation 1). To facilitate interpretation of the effect sizes, column 1 presents the sample means and standard deviations of the dependent variables. Column 2 shows the estimated difference between treatments and controls, $\overline{Y}_t - \overline{Y}_c$, with Eicker-White robust standard errors in parentheses.

The top row shows that students in treatment buildings were slightly younger on average than students in control buildings, although this difference represents only 0.05 standard deviations. Roughly 49 percent of treatment students are male compared with 51 percent of control students, a statistically significant though substantively small difference. However, the remainder of the table indicates that there are no significant differences between treatment and control students in terms of family composition, socio-economic status, neighborhood poverty, peer quality, reading ability, high school GPA, attendance or credit accumulation.[14] Treatment students have slightly lower math scores, although the magnitude of this difference is only 0.03 standard deviations.[15]

6. What was the Effect of Public Housing Closure on Residential Relocation and Educational Achievement?

Table 4 shows the effect of the public housing closures on a variety of relocation and educational outcomes three years after the initial notification. Column 1 shows the control mean three years after the announcement to facilitate interpretation of the magnitudes. The remaining columns present different estimates of the treatment effect. Column 2 shows the differences in

[13] This structure is chosen because the school records contain addresses as of October for each academic year.
[14] These estimates include controls for age.
[15] This difference is due entirely to the inclusion of students from three buildings in the Robert Taylor Homes that were demolished in 1997. To test the sensitivity of the findings presented in this paper, I have run all analyses excluding these students (and their controls) and obtained virtually identical results. I explore the demolitions of these buildings in more detail in Table 9.

levels between treatments and controls ($\overline{Y_t} - \overline{Y_c}$), column 3 presents the same estimates conditional on observable characteristics prior to the announcement and column 4 shows the differences in gains between treatments and controls ($\Delta\overline{Y_t} - \Delta\overline{Y_c}$) conditional on prior characteristics – i.e., a difference-in-difference estimate. 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 to 4, which is generally the case in Table 4.

We see that roughly 47 percent of control students moved during this period, reflecting the high mobility rates among the general public housing population. Students in treatment buildings were 31 percentage points more likely to move, which indicates that the public housing closings substantially increased the likelihood of relocation. Note that by three years after the initial notification, roughly 20 percent of the treatment group still had not moved. In some cases, notification preceded actual demolition by several years. Conditional on having moved, the treatment group changed residences roughly the same number of times as the comparison group.

While treatment students were considerably more likely to move following the closure notification, they relocated to neighborhoods relatively close to their original residence. The average treatment student who moved relocated in a census tract only three miles[16] from his or her original residence, where the poverty rate was 60 percent on average (i.e., control group mean of .656 minus treatment effect of .058). This finding is consistent with the Section 8 literature that suggests that inner-city public housing residents are unlikely to leave their neighborhoods without intensive support and/or a requirement to relocate to a low-poverty area. In contrast, the experimental groups in Gautreaux and MTO moved to neighborhoods with average poverty rates of less than 10 percent, the urban mover comparison group in Gautreaux

[16] Considering that the city of Chicago is roughly 20 miles long and 10 miles wide, this is a relatively short move.

settled in tracts with an average poverty rate of 28 percent, and the majority of Section 8 controls in MTO relocated to tracts with poverty rates between 10 and 40 percent (Goering, Kraft et al. 1999; Deluca 2000).

While students forced out by redevelopment did not move far away or to more advantaged neighborhoods, many did leave public housing. Only 65 percent of control students were still living in public housing three years after the announcement, perhaps reflecting the healthy economy during the 1990s or the anticipation of future demolitions. Living in a building slated for closure increased the probability of leaving public housing by roughly 15 percentage points. As a result of the public housing closings, therefore, 50 percent of students had left public housing, 30 percent had transferred to another unit within CHA, and 20 percent had not yet moved.

Treatment students were only 13 percent (8 percentage points) more likely to have changed schools than the control students, consistent with the fact that many remained in public housing or moved to nearby neighborhoods. Among those students who had switched schools at least once, the treatment group did not move more often than the comparison group. Three years after the closure notification, the comparison students were attending schools in which only 28 percent of students had met national norms in mathematics and treatment students were in similar schools.

The public housing closures shifted students out of public housing, but they did not appear to have substantial influence on the neighborhood or school environment experienced by children (at least in the measures explored here). If social context influences educational outcomes largely through the school and neighborhood environment, then we would not expect to find significant improvements among the treatment students. In fact, to the extent the

relocations were disruptive, one might expect to find negative effects in the short-run. On the other hand, if living in high-rise public housing has an impact on student achievement independent of school and neighborhood environment, then treatment students might exhibit higher performance following the building closures.

A comparison of educational outcomes among treatment and comparison students in the bottom panel of Table 4 indicates that while the public housing closings had no observable impact on achievement. The point estimates are extremely small and are never statistically different than zero. The bottom two rows in Table 4 show that the closings had no effect on the probability of dropping out or being enrolled in school three years after the announcement, suggesting that the achievement effects were not due to differential selection.

6.1 Short versus Long-Run Treatment Effects

To examine the effects over time of public housing closings, Table 5 presents estimates of the treatment effects one, two and four years after initial notification for a sample of students in buildings with closure notification prior to 1996 (n=8,803). 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 4 that also included students in developments with closures at later dates. The top panel of Table 5 shows that building closures did not have a significant impact on either school enrollment or dropout rates within this sample for any period after the closure announcement.

The bottom two panels show the relocation and educational outcomes for students who remained in the CPS (i..e, did not drop out, transfer to private school, or move out of the school

district) for the entire four-year period (n=5,537).[17] 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 four. For example, one year following the notification treatment students were 18 percentage points more likely to have moved than control students. Two years following the closure announcement, however, treatment students were 27 percentage points more likely to have moved.

Conditional on moving, treatment students relocated closer to their original neighborhood than control students. This gap increased over time, suggesting that the treatment families who moved immediately following notification were more likely to have moved voluntarily with the intention of leaving their old neighborhood than treatment families who waited several years to move. The effect on neighborhood poverty, school mobility and percent of school peers meeting national norms does not appear to vary much over time.

There is no statistically significant difference in the standardized test scores or absences of the two groups over time. However, by four years following the initial notification, students in the treatment group were 10 percent (3.8 percentage points) more likely to be old for their grade, suggesting that treatment students were more likely to have been retained since the public housing closings.

6.2 Variation in Treatment Effects Across Students

While public housing closures do not appear to have an aggregate effect on student outcomes, it is possible that they may have significant effects for certain groups. Table 6 examines closure effects by gender, age and ability at the time of the announcement. Two

[17] The number of observations varies across the dependent variables because certain outcome variables are only applicable to elementary or secondary students.

interesting findings stand out. First, the effect on dropout rates is significantly higher among older students for whom dropping out is a viable alternative. Among students who were 14 years or older at the time of the closure announcement, those children living in buildings slated for demolition were roughly seven percent (three percentage points) more likely to have dropped out of school within three years. Second, the closures seem to have had a more negative effect on low achieving students. Students who scored two or more grade levels below national norms at the time of the announcement were roughly 33 percent (8.6 percentage points) more likely to have dropped out of school within three years as compared with comparable peers who were living in buildings that were not closed.

7. Why Did the Public Housing Closings Have So Little Apparent Effect on Educational Outcomes?

There may be several reasons why the public housing closings had no positive (and some negative) effect on educational outcomes. One reason is certainly that the relocated families did not move to better neighborhoods and, even if the children did change schools as a result of the move, their new schools were not substantially different than their old schools in terms of mean student achievement levels. However, this analysis does not tell us whether students would have benefited academically if they *had* moved to higher "quality" neighborhoods or schools. By comparing outcomes of treatment and control students who did move to better environments with students who did not, Table 7 sheds some light on this question. Because families who moved to better environments were likely different than those who did not, we cannot attribute differences in outcomes entirely to the neighborhoods or schools. However, these estimates may be an upper bound on the true causal effect.

Each of the panels in Table 7 represents a different specification. In each case, the different categories of students described in the first column are compared to the same group—treatment and control students who did not move. Panels A and B emphasize the importance of voluntary moves. Panel A shows the differential outcomes of treatment and control students who moved at least once during the three years following the initial notification; the comparison group includes students (treatments as well as controls) who did not move during this period. The first column indicates that 81 percent of treatment students had moved in contrast to only 47 percent of control students. Columns 2 through 8 show the gains for a variety of outcomes. Control students who moved were 11 percentage points more likely to have left public housing than treatment students who moved (.69 - .58), suggesting that their moves were more often motivated by a desire to leave public housing. Similarly, controls who moved were nine percentage points more likely to change schools and they settled in neighborhoods with slightly lower poverty rates in comparison to treatment students who moved. Despite this apparent desire to move, control students did not attend higher-achieving schools or have better educational outcomes than treatment group movers or treatment and control students who did not move.

Panel B offers another perspective on voluntary versus involuntary moves by distinguishing between treatment students who moved within the first year of the announcement date from those who moved after this time (presumably those who did not want to move and waited until the last possible moment to do so). Treatment students who moved within the first year looked closer to control students who moved during the entire period. Both groups are more likely to leave public housing and to change schools than either treatment students who

waited to move, or treatment and controls that did not move. There is no significant difference between the achievement gains of these groups however.

Panel C and D differentiate students on the basis of their destination neighborhood or school. I define good neighborhoods as those with poverty rates lower than 25 percent and good schools as those in which at least 40 percent of students met national norms in mathematics. Columns 2 and 3 show that students who move to good neighborhoods or schools are substantially more likely to have left public housing and changed schools than students who moved to less desirable schools and neighborhoods. Students who move to good neighborhoods, however, do not have better educational outcomes than those who do not move or move to other neighborhoods. In fact, treatment students who move to lower poverty neighborhoods have somewhat smaller math gains than students who did not move (though it is not statistically significant), which may reflect the fact that these students experienced a larger, more disruptive transition.

In contrast, treatment and control students who moved to relatively higher achieving schools gained 0.30 grade equivalents more than peers who did not move. While these estimates are conditional on a variety of observable demographic and prior achievement characteristics, it is quite likely that the families and children that made such dramatic moves are different than others along unobservable dimensions. Only 12 and 6 percent of treatments and controls respectively moved to these "good" schools, suggesting that the students were a select group.

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 received more notice and thus may have 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 between 1992 and 2000, 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 1990s, 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 re-organized and contracted out to a private organization in 1995-96. By the end of the nineties, the City had assumed control of day-to-day operations in CHA, which appeared to be running more smoothly than in the past. Given this history, it is likely that residents living in buildings closed in 1992-93 had fewer opportunities to relocate out of public housing with Section 8. In contrast, by the time the 1998-99 closings occurred, the Section 8 program for relocatees was well established, organized and known by all tenants.

Table 8 examines the treatment effects in a variety of different situations. Columns 1 to 3 show the effect of the closings for three time periods. Outcomes are measured one year after the announcement year. To begin, note that none of the closures in 1992-93 and only 15 percent in 1997-98 were planned, defined as closures in which there was at least a year between initial notification and final vacancy of the building. The top panel shows that treatment students in the

1992-93 and 1997-98 closings were roughly 55 percentage points more likely to have moved than control students in comparison to treatment students in 1995-96 who were only 16 percentage points more likely to have moved than controls. Treatment students in the early and late periods did not move as far from their original residence as did control students who moved whereas treatments and controls who moved in 1995-96 moved roughly the same distance from their residence. This is consistent with unplanned closures forcing residents to leave more quickly and thus locate in nearby areas.

The 1997-98 closings had a considerably larger effect on neighborhood poverty rates, school mobility and the probability of living in public housing than either of the earlier waves of closures. Treatment students in the 1997-98 closures were 28 percentage points less likely to be living in public housing than controls and 17 percentage points more likely to have changed schools. There were no significant differences in educational outcomes between treatment students in the two types of closures.

Column 1 in the bottom panel presents some evidence that the 1992-93 closures had a positive effect on student achievement. Elementary children in the treatment group gained roughly 0.25 grade equivalents more than control students in math and reading, a substantial benefit that is not statistically significant because of the large standard errors. At the same time, high school students in the treatment group appear to have suffered as a result of the closures. The change in their average absences was nearly seven days per course greater than that of control students and their GPA decreased 0.23 relative to controls. Treatment students in the 1995-96 and 1997-98 closures have outcomes identical to controls.

Columns 4 to 9 examine the effect of planned versus unplanned closures more carefully by comparing the treatment effects for both closure types within the 1995-96 period. While there

is some evidence that treatment students in the unplanned closures were more likely to have moved, left public housing and changed schools (particularly in year one), the closures did not have a significant impact on the educational outcomes in either scenario.

8.1. The Effect of Public Housing Closures in the Robert Taylor Homes

The most extensive and most visible closings and demolitions during this period took place in the Robert Taylor Homes. Robert Taylor is a good case to study not only because of the number of families affected, but also because of the differences in the nature of the closings that occurred in the development. One of the high rises was closed in Fall 1997 as part of the planned vacancy consolidation program and residents were notified of the closing in 1995. In contrast, in the Fall of 1998 the CHA closed three Taylor buildings commonly referred to as "The Hole" because of prevalent gang activity. Following on the heels of a highly publicized shooting, these buildings were vacated with little advance warning. Finally, in January 1999, residents in a number of high rises were evacuated on an emergency basis when burst pipes during the middle of a snowstorm, causing heat to fail in many of the apartments.

Table 9 shows estimates of the treatment effect for each of the three sets of closings that took place in Robert Taylor. All of the outcomes are measured one year after the initial notification. Column 2 shows that the effect of the closures the probability of living in public housing was greatest in "The Hole," where treatment students were 59 percentage points less likely to live in public housing than comparison students after only one year. Note that students who left the Taylor buildings because of maintenance emergencies were only 18 percentage points less likely to be living in public housing than comparable peers, reflecting the fact that CHA relocated many of these families to temporary apartments within the Taylor complex.

While the vacancy consolidation does not appear to have impacted educational outcomes, the other closures in Robert Taylor may have had some impact on student achievement. Treatment students in column 2 were 8.7 percentage points more likely to have become old for grade following the closure announcement, suggesting that many of these students were retained. Because students in the Hole were observational lower-achieving than comparable peers in other Robert Taylor buildings, one may not be able to attribute this effect to the closings. At the same time, treatment students in column 3 were less likely to have been retained than control students during the year after the closure announcement. Because these impacts may simply reflect short-term adjustment effects, it is difficult to interpret them yet.

9. Policy Implications and Conclusions

The findings from this analysis have several policy implications. Perhaps most importantly, this study suggests that demolishing public housing and providing households the option of using housing vouchers to relocate to different neighborhoods will not necessarily produce better educational outcomes for poor children. I find that (1) 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, (2) even those students who did move to substantially better neighborhoods did not end up in significantly better schools, and (3) even those students who ended up in considerably better schools as a result of the public housing closings did poorly in comparison to control students who, presumably as a result of a more voluntary and less disruptive move, ended up in better schools. Together these results suggest that if one hopes to improve the educational outcomes of children in public housing, an explicit effort must be made to move families to substantially better neighborhoods and schools.

Second, in contrast to the previous literature on "defensible space," the findings from this study provide some evidence that high-rise public housing does not have an independent impact on student achievement. While students impacted by the closures did not move far from their original neighborhood, they were considerably less likely to live in public housing following the closures. Yet these students had no better, and occasionally somewhat worse, 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. One alternative interpretation is that the benefit of leaving public housing and living in a slightly better neighborhood was offset by the negative effect of moving. And a third possibility is that *long-term* exposure (or lack of exposure) to public housing is what influences achievement levels. That is, in order to obtain the benefit of private housing, a child must grow up for an extended period of time in that environment. Thus we would not expect a child who lived in public housing until the age of 12 to show better outcomes three years after leaving public housing, at age 15.

Third, this study reinforces the importance of mobility assistance and low poverty relocation requirements, although it does not shed light on which of these two mechanisms is most important. 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. In personal interviews I conducted, 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.[18]

Fourth, the findings from this analysis highlight the importance of voluntary versus involuntary relocation. I find some evidence that families required to relocate from public housing fare poorly in comparison to those who leave voluntarily. Movers in control buildings locate in better neighborhoods and attend better schools than students in treatment buildings, some of whom would presumably not have moved except for the demolitions. In addition, control students who move to a good school perform higher than treatment students who move to a good school.

Finally, the fact that students who moved to neighborhoods with poverty rates more than 40 percentage points lower than their original neighborhoods did not move to schools with higher quality peers reflects the extensive segregation in Chicago and the low achievement levels throughout the public school system and speaks to the importance of system-wide school reforms as well as treatments focused on a particular segment of the student population such as public housing children. This is an important finding considering that relocation to a lower poverty neighborhood was not associated with enhanced performance whereas a move to a school with higher achieving peers did coincide with improvements in academic achievement. While it is not possible to conclude that this relationship is causal given the endogeneity associated with the location decision, it does suggest that the school environment may be a more important influence on educational outcomes than the neighborhood environment.

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, this does not imply

[18] Many residents feared that if they took Section 8, the government could cut their funding in a year or two and then they would be without any housing option.

that the demolitions themselves are a bad policy. First, as mentioned above, there may be longer term impacts associated with living public housing or moving to marginally better neighborhoods that are not evident in this study. 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 new construction. While the results presented in this paper inform the education and housing literatures, many questions remain concerning the consequences of dismantling what has been such a large a part of social welfare policy in this country for the past half century.

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Appendix A: Definition of Variables

Variables	Data Source	Definition
Demographics		
Student Demographics (Race, Gender, Birthdate, Household Composition, Free or Reduced Lunch, Special Education)	CPS	Taken directly from student records. Household composition is drawn from information on the student's guardian, which varies by semester. I take 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 100 units (roughly 10 stories). 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, .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.
% school peers meeting national norms in math	CPS	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 & School Mobility	CPS	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 is 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	CPS	A student is considered old for grade if Age (in September) > Grade + 6.5. Students in non-graded classrooms received a missing for this variable.
Math & Reading Scores	CPS	From student test files. These variables are measured in grade equivalents that represent the years and months of learning as of the testing date. For example, a 6th grade student who scores at the 50th percentile nationally will receive a 6.8, indicating 6 years and 8 months of learning (the national norms assume that the exams are taken in the 8th month of the school year).
GPA, Absences and Credits	CPS	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.
Dropout and Enrollment Status	CPS	Student records.

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 shorter 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. I conduct a similar analysis using the annual public school enrollment by building.

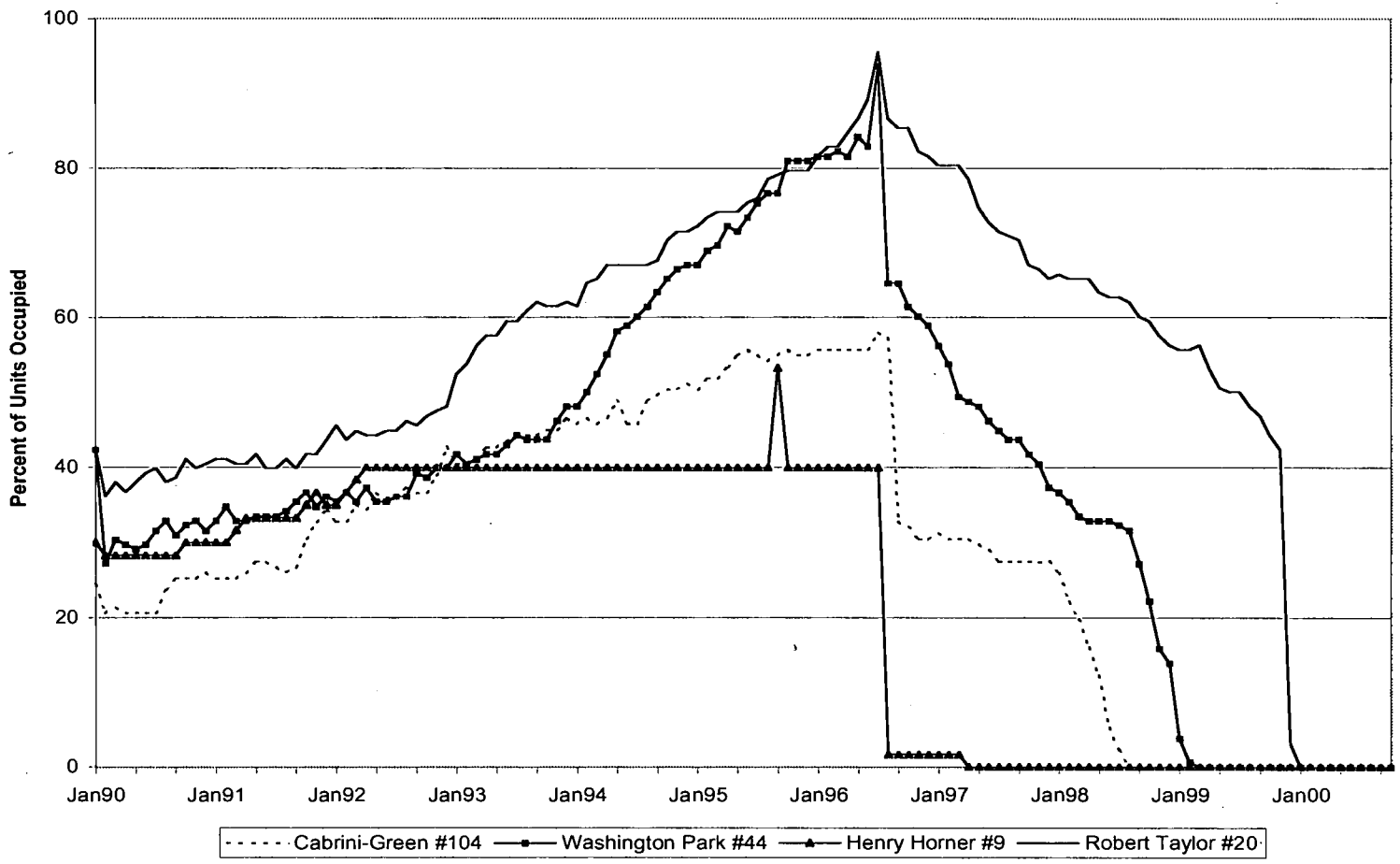
Finally, I supplement these analyses with information from interviews with CHA officials, housing advocates and the presidents of the Local Advisory Councils (LACs) in all 13 of the developments that experienced some building closures during this period. The LAC presidents were particularly helpful in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

Appendix C: Sample Construction

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 Washington Park development prior to May 1997 when a set of building closures were announced in that development, the family will be included in both the Taylor '95 and Washington Park '97 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, 1997 and 1998, each year for a different set of buildings. Therefore, there are four 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 (i..e, a student who belongs to four base groups will appear four 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 possible for students to be in multiple demolition groups. For example, a student might be living in a building within Robert Taylor in 1995 when its closure is announced and then move to a building in ABLA that is closed in 1998. Fortunately, there are extremely few students with multiple demolition groups. For these students, I simply consider the first demolition group, thus assuming that the initial disruption is the treatment of interest.

Figure 1: Residential Relocation Patterns in Selected Public Housing Buildings that Were Closed



Notes to Figure 1: Monthly occupancy rates provided by the Chicago Housing Authority.

Table 1: Summary Statistics on Chicago Public School (CPS) Students in 1994-95

Dependent Variable	All CPS Students (1)	Students who Lived in Public Housing	
		All Developments (2)	Developments with Closures (3)
Male	0.51 (0.50)	0.50 (.50)	0.50 (.50)
Black	.55 (.50)	0.98 (.15)	0.99 (.08)
Hispanic	.30 (.46)	0.01 (.11)	0.00 (.05)
Age	10.9 (3.9)	10.4 (3.9)	10.4 (4.0)
Living with at least one parent	.87 (.34)	.92 (.27)	.92 (.28)
Living in foster care	.03 (.18)	0.04 (.19)	0.04 (.20)
Old for grade	.07 (.25)	0.10 (.30)	0.10 (.31)
Free lunch	.68 (.47)	.81 (.39)	.82 (.38)
Reduced price lunch	.06 (.24)	.01 (.07)	.00 (.07)
Special Education	.11 (.31)	.13 (.33)	.13 (.33)
Math Score	5.54 (2.89)	4.58 (2.47)	4.51 (2.4)
Reading Score	5.28 (3.08)	4.19 (2.60)	4.10 (2.53)
GPA	1.84 (1.05)	1.36 (.95)	1.33 (.96)
# Course Absences per Semester	17.5 (15.6)	27.2 (18.0)	28.4 (18.3)
Credits	43.6 (8.4)	43.0 (9.5)	42.9 (9.4)
Census Tract Poverty Rate	.27 (.21)	0.76 (.17)	0.82 (.12)
% school peers meeting national norms in math	.27 (.18)	0.16 (.12)	0.15 (.12)
Number of Observations	423,370	27,902	20,448

Notes for Table 1: Standard deviations are presented in parentheses below the group means.

Table 2: Public Housing Experiences of Chicago Public School Students from 1991-92 to 1999-2000

	Mean (standard deviation)			
	All Developments	Developments with no Closures [a]	Analysis Sample	
			Control Buildings [b]	Treatment Buildings [c]
Outcome Variables	(1)	(2)	(3)	(4)
# Years actively enrolled in school	5.05 (2.72)	4.41 (2.77)	6.38 (2.15)	6.32 (2.19)
# Years in public housing	3.22 (2.46)	2.34 (2.06)	5.39 (2.32)	4.88 (2.29)
% Lived in High Rise Building	0.52 (0.49)	0.39 (0.49)	1.00 (0.00)	1.00 (0.00)
% Lived in Mid-Rise Building	0.66 (0.47)	0.54 (0.50)	1.00 (0.00)	1.00 (0.00)
# Years in High Rise Building [d]	3.09 (2.38)	1.57 (1.20)	5.29 (2.34)	4.62 (2.28)
# Years in Mid-Rise Building [d]	3.02 (2.36)	1.76 (1.47)	5.31 (2.33)	4.72 (2.27)
# Different Residences	2.52 (1.86)	2.51 (1.92)	2.38 (1.64)	2.70 (1.62)
# Different Public Housing Developments Lived In	1.07 (0.28)	1.05 (0.25)	1.09 (0.31)	1.12 (0.35)
# Different Public Housing Buildings Lived In	1.20 (0.48)	1.14 (0.40)	1.27 (0.53)	1.46 (0.65)
Number of Observations	67,912	41,989	13,031	5,338

Notes for Table 2: [a] These developments had no buildings that were closed during the period 1992-2000. [b] These buildings were located in developments that experienced some closures, but were not themselves closed. [c] These buildings were closed and/or demolished during the period 1992-2000. [d] Estimated means are conditional on having lived in a mid- or high-rise respectively. Standard deviations are in parentheses below the group means.

Table 3: Differences Between Treatment and Control Students Prior to the Closure Announcement

Dependent Variable	Control Means (s.d.) (1)	Difference Between Treatments and Controls ($\overline{Y}_t - \overline{Y}_c$) (s.e.) (2)
Age	10.23 (3.98)	-0.225 (0.080)
Male	0.514 (0.500)	-0.023 (0.010)
Living with at least one parent	0.918 (0.275)	0.002 (0.006)
Living in foster care	0.048 (0.213)	0.003 (0.004)
In Special Education	0.113 (0.317)	0.001 (0.006)
Free Lunch	0.978 (0.147)	-0.003 (0.003)
Old for Grade	0.214 (0.410)	-0.002 (0.007)
Math Score	4.918 (2.316)	-0.109 (0.038)
Reading Score	4.481 (2.430)	-0.064 (0.039)
GPA	1.467 (0.903)	0.005 (0.053)
# Absences	21.2 (16.2)	-0.105 (0.754)
# Credits	45.5 (8.5)	-0.107 (0.537)
Moved in Past Year	0.117 (0.321)	-0.014 (0.006)
Changed Schools in Past Year	0.169 (0.367)	-0.009 (0.007)
Census Tract Poverty Rate[a]	0.837 (0.100)	0.011 (0.018)
% peers meeting national norms in math[a]	0.198 (0.128)	-0.003 (0.008)

Notes for Table 3: The first column shows the control group means with the standard deviations in parentheses in the year prior to the closure announcement. Column 2 shows the difference between treatments and controls in the year prior to the closure announcement. The differences shown in column 2 are estimated from a regression model that includes fixed effects for housing development and year as well as linear and quadratic age terms (see equation 1 in the text). The number of observations varies across the dependent variables because certain outcomes are only available for elementary or secondary students. In column 2, Eicker-White robust standard errors clustered by student are shown in parentheses. [a] Standard errors of these estimates account for correlation within census tract.

Table 4: Effects of Public Housing Closure Three Years After the Announcement Year

Dependent Variable	Control Mean 3 Years Post Notification (s.d.)	Outcome		
		Difference in Levels Between Treatments and Controls $(\overline{Y_t} - \overline{Y_c})$ (s.e.)	Difference in Levels Between Treatments and Controls $(\overline{Y_t} - \overline{Y_c})$ (s.e.)	Difference in Gains Between Treatments and Controls $(\Delta\overline{Y_t} - \Delta\overline{Y_c})$ (s.e.)
	(1)	(2)	(3)	(4)
Relocation Outcomes				
Moved Since Notification	0.468 (0.499)	0.291 (0.016)	0.291 (0.016)	0.291 (0.016)
# of Residential Moves[a]	1.50 (0.73)	-0.08 (0.03)	-0.08 (0.03)	-0.08 (0.03)
Miles from original residence[a]	3.15 (3.10)	-0.20 (0.14)	-0.21 (0.14)	-0.21 (0.14)
Census Tract Poverty Rate	0.656 (0.258)	-0.058 (0.009)	-0.057 (0.009)	-0.053 (0.009)
Living in Public Housing	0.646 (0.463)	-0.141 (0.017)	-0.141 (0.018)	-0.141 (0.018)
Changed Schools Since Notification	0.600 (0.490)	0.083 (0.016)	0.085 (0.016)	0.085 (0.016)
# of School Moves[b]	1.47 (0.70)	-0.00 (0.03)	-0.00 (0.03)	-0.00 (0.03)
% school peers meeting national norms in math	0.285 (0.120)	0.005 (0.004)	0.004 (0.004)	0.024 (0.004)
Educational Outcomes				
Math Score	5.437 (2.520)	-0.033 (0.063)	-0.001 (0.059)	0.080 (0.079)
Reading Score	5.018 (2.559)	-0.056 (0.066)	-0.024 (0.062)	-0.008 (0.083)
Old for Grade	0.323 (0.468)	0.009 (0.017)	0.011 (0.015)	0.011 (0.018)
# of Absences per Course	15.66 (11.68)	0.85 (1.00)	0.87 (1.00)	-2.16[d] (1.69)
GPA	1.56 (0.91)	-0.01 (0.07)	-0.03 (0.07)	0.08[d] (0.10)
Credits	49.30 (8.10)	0.55 (0.67)	0.58 (0.66)	-2.09[d] (1.62)
Enrolled in School	0.655 (0.475)	0.003 (0.011)	0.003 (0.011)	0.003 (0.011)
Dropped Out	0.150 (0.357)	0.011 (0.009)	0.009 (0.009)	0.009 (0.009)
Controls for Student Characteristics at Time of Closure[c]	No	No	Yes	Yes

Notes to Table 4: The number of observations varies across the dependent variables because certain outcomes are only available for elementary or secondary students. [a] Conditional on having moved during this period. [b]Conditional on having changed schools. [c]Controls include gender, age, age squared, living with at least one parent, living in foster care, special education, free lunch and old for grade. [d]These estimates are based on students who were in high school in the year prior to the closure announcement as well as three years prior to the closure announcement.

Table 5: Effects Over Time of Public Housing Closure for Students Enrolled in School Four Years After Closure Announcement

Dependent Variable	Difference in Levels Between Treatments and Controls ($\overline{Y_t} - \overline{Y_c}$) (s.e.) [Control Mean] One Year After Notification (1)	Two Years After Notification (2)	Four Years After Notification (3)
Sample Attrition (n=8,803)			
Enrolled in School	-0.009 (0.010) [0.820]	-0.001 (0.011) [0.738]	0.009 (0.012) [0.582]
Dropped Out	0.005 (0.007) [0.056]	0.008 (0.009) [0.105]	0.004 (0.011) [0.198]
Relocation Outcomes (n=5,537)			
Moved Since Notification	0.182 (0.019) [0.252]	0.271 (0.019) [0.362]	0.287 (0.017) [0.560]
Miles from original residence[a]	-0.010 (0.20) [2.07]	-0.532 (0.170) [2.781]	-0.708 (0.149) [3.288]
Census Tract Poverty Rate	-0.030 (0.008) [0.754]	-0.026 (0.009) [0.705]	-0.038 (0.010) [0.617]
Living in Public Housing	-0.064 (0.016) [0.825]	-0.109 (0.019) [0.724]	-0.090 (0.019) [0.540]
Changed Schools Since Notification	0.078 (0.019) [0.311]	0.078 (0.019) [0.454]	0.060 (0.017) [0.680]
% school peers met national norms in math	-0.011 (0.003) [0.206]	-0.012 (0.004) [0.257]	0.003 (0.004) [0.311]
Educational Outcomes (n=5,537)			
Math Score	-0.004 (0.054) [4.609]	-0.054 (0.055) [4.870]	-0.005 (0.071) [5.764]
Reading Score	-0.057 (0.060) [4.192]	-0.091 (0.062) [4.450]	-0.017 (0.078) [5.299]
Old for Grade	-0.010 (0.008) [0.149]	0.002 (0.013) [0.221]	0.038 (0.018) [0.395]
# Absences per course	0.02 (1.95) [15.02]	0.02 (1.28) [15.03]	0.48 (0.96) [17.37]
Controls for Student Characteristics at Time of Closure[b]	Yes	Yes	Yes

Notes to Table 5: The sample for this analysis includes students in public housing developments that experienced closures before 1996. The top panel shows the effect of treatment on school enrollment for the full sample (n=8,803). The bottom two panels show the effect of the treatment on relocation and educational outcomes for the sub-sample of students who were still enrolled four years after notification (n=5,537). 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 are shown in parentheses. [a]Conditional on having moved during this period. [b]Controls include gender, age, age squared, living with at least one parent, living in foster care, special education, free lunch and old for grade.

Table 6: Effects of Public Housing Closure on Student Outcomes Three Years After Closure Announcement, by Student Characteristic

Dependent Variable	Difference in Levels Between Treatments and Controls ($\overline{Y_t} - \overline{Y_c}$) (s.e.) [Control Mean]							
	Age at Time of Announcement			Gender		Test Score at Time of Announcement		
	3-9	9-14	14-21	Boys	Girls	2+ Years Below Grade Level	1-2 Years Below Grade Level	1 Year or Less Below Grade Level
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Math Score	0.009 (0.059) [3.976]	-0.041 (0.112) [7.406]	[b]	-0.041 (0.081) [5.219]	0.037 (0.083) [5.666]	0.411 (0.460) [6.390]	-0.052 (0.158) [6.491]	-0.094 (0.107) [7.345]
# Absences per course	[a]	-0.799 (1.169) [17.563]	0.360 (1.779) [17.575]	-1.013 (1.492) [18.462]	0.316 (1.315) [16.767]	-2.226 (2.523) [18.998]	-2.351 (1.873) [18.429]	1.170 (1.420) [16.577]
Dropped Out	0.001 (0.007) [0.033]	0.002 (0.013) [0.082]	0.031 (0.028) [0.476]	-0.005 (0.012) [0.163]	0.023 (0.012) [0.136]	0.086* (0.044) [0.271]	-0.014 (0.027) [0.185]	-0.001 (0.014) [0.086]

Notes for Table 6: 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 error in parentheses as well as the comparison group mean for the dependent variable in square brackets. Each column represents a different sample of students. [a] Course absences are not available for elementary school students. [b] Few high school students take standardized exams. * indicates that the difference in the treatment effect across the comparison groups is significant at the 0.05 level.

Table 7: The Relationship between Residential Mobility, Destination Neighborhood, Destination School and Student Outcomes Three Years Following the Announcement Year

Independent Variables	Dependent Variables: Difference in Gains Between Treatments and Controls ($\Delta\overline{Y_t} - \Delta\overline{Y_c}$) (s.e.)							
	%	Living in Public Housing	Changed Schools	Poverty Rate	% Peers at National Norms	Math Score	Old for Grade	Absences
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A								
Treatment students who moved	.81	-0.578* (0.015)	0.435* (0.016)	-0.251* (0.009)	0.036 (0.004)	0.070 (0.084)	0.006 (0.016)	-0.717 (1.004)
Control students who moved	.47	-0.691 (0.009)	0.525 (0.011)	-0.316 (0.006)	0.034 (0.003)	0.082 (0.059)	-0.013 (0.012)	-1.082 (0.727)
Panel B								
Treatment students who moved within 1 year	.44	-0.627* (0.018)	0.496* (0.017)	-0.264* (0.011)	0.040 (0.005)	0.079 (0.093)	0.006 (0.019)	-2.291 (1.194)
Treatment students who moved later	.37	-0.519 (0.020)	0.362 (0.019)	-0.235 (0.011)	0.032 (0.005)	0.057 (0.114)	0.007 (0.019)	0.882 (1.290)
Control students who moved	.47	-0.691 (0.009)	0.525 (0.011)	-0.316 (0.006)	0.034 (0.003)	0.082 (0.059)	-0.013 (0.012)	-1.082 (0.727)
Panel C								
Treatment students who moved to good neighborhood	.14	-0.858* (0.015)	0.564* (0.022)	-0.575* (0.009)	0.063* (0.009)	-0.157 (0.154)	-0.016 (0.027)	-1.875 (1.907)
Treatment students who moved to other neighborhood	.67	-0.512 (0.016)	0.406 (0.016)	-0.174 (0.008)	0.009 (0.004)	0.118 (0.088)	0.011 (0.016)	-0.446 (1.059)
Control students who moved to good neighborhood	.10	-0.956 (0.007)	0.579 (0.017)	-0.619 (0.008)	0.072 (0.006)	0.075 (0.096)	0.004 (0.028)	-0.655 (1.678)
Control students who moved to other neighborhood	.37	-0.624 (0.011)	0.512 (0.012)	-0.239 (0.005)	0.025 (0.003)	0.083 (0.065)	0.015 (0.013)	-1.186 (0.744)
Panel D								
Treatment students who moved to good school	.12	-0.568* (0.028)	0.739* (0.013)	-0.294* (0.020)	0.208* (0.009)	0.292* (0.167)	-0.016 (0.029)	0.778 (2.066)
Treatment students who moved to other school	.52	-0.516 (0.016)	0.708 (0.009)	-0.222 (0.009)	-0.005 (0.004)	-0.059 (0.091)	0.018 (0.017)	-1.044 (1.030)
Control students who moved to good school.	.06	-0.790 (0.016)	0.720 (0.010)	-0.391 (0.013)	0.185 (0.009)	0.297 (0.147)	-0.001 (0.026)	-4.511 (1.610)
Control students who moved to other school.	.35	-0.625 (0.011)	0.688 (0.008)	-0.286 (0.007)	0.005 (0.003)	0.000 (0.064)	0.023 (0.013)	-0.582 (0.761)

Notes for Table 7: The omitted category always consists of treatment and control students who did not move during the three years following the closure notification. Eicker-White robust standard errors are in parentheses below the estimates. A good school is defined as one where at least 40 percent of students met national norms in mathematics. A good neighborhood is defined as a census tract with a poverty rate of less than 25 percent. * indicates that the difference in the treatment effect across the comparison groups is significant at the 0.05 level.

Table 8: The Effect of Building Closing on Student Outcomes Across Development

	Difference in Gains Between Treatments and Controls ($\Delta\overline{Y_t} - \Delta\overline{Y_c}$) (s.e.)						
Type of Closure	Planned and Unplanned			Unplanned		Planned	
Notification Year	1992-93	1995-96	1997-98	1995-96		1995-96	
Year After Notification that Outcome is Measured	1 Year	1 Year	1 Year	1 Year	3 Years	1 Year	3 Years
	(1)	(2)	(3)	(6)	(7)	(8)	(9)
Relocation Outcomes							
Moved Since Notification	0.551	0.165	0.488	0.246	0.346	0.070	0.176
	(0.045)	(0.016)	(0.012)	(0.022)	(0.020)	(0.022)	(0.025)
Miles from original residence[a]	-1.174	0.242	-0.525	-0.046	-0.201	0.613	0.111
	(0.300)	(0.168)	(0.100)	(0.192)	(0.176)	(0.275)	(0.211)
Census Tract Poverty Rate	-0.028	-0.039	-0.124	-0.049	-0.052	-0.032	-0.070
	(0.016)	(0.006)	(0.006)	(0.009)	(0.013)	(0.008)	(0.013)
Living in Public Housing	-0.062	-0.083	-0.283	-0.104	-0.147	-0.063	-0.153
	(0.043)	(0.013)	(0.012)	(0.018)	(0.024)	(0.018)	(0.025)
Changed School Since Notification	-0.053	0.088	0.170	0.106	0.089	0.059	0.099
	(0.057)	(0.016)	(0.014)	(0.022)	(0.022)	(0.023)	(0.024)
% school peers meeting national norms in math	0.032	0.011	0.025	0.011	0.036	0.005	0.004
	(0.009)	(0.002)	(0.003)	(0.003)	(0.006)	(0.003)	(0.005)
Educational Outcomes							
Math Score	0.260	0.004	0.025	0.006	0.076	0.042	0.072
	(0.168)	(0.053)	(0.042)	(0.071)	(0.108)	(0.075)	(0.116)
Reading Score	0.257	-0.059	0.021	-0.094	-0.081	0.005	-0.078
	(0.201)	(0.063)	(0.055)	(0.084)	(0.111)	(0.088)	(0.122)
Old for Grade	-0.036	-0.009	0.001	-0.017	0.024	0.006	0.013
	(0.030)	(0.008)	(0.010)	(0.010)	(0.021)	(0.013)	(0.023)
# absences per course	6.886	0.143	0.092	0.354	1.178	-0.540	-6.554
	(3.848)	(1.022)	(0.725)	(1.261)	(2.037)	(1.547)	(2.940)
GPA	-0.236	0.030	0.058	0.026	0.015	0.072	0.236
	(0.129)	(0.041)	(0.038)	(0.055)	(0.140)	(0.053)	(0.151)
Dropped Out	0.000	0.006	0.005	0.016	0.010	-0.005	0.017
	(0.021)	(0.006)	(0.005)	(0.009)	(0.012)	(0.009)	(0.013)
% of Closures that Were Planned	0	63	15	--		--	
Number of Treatments	133	2,335	1,881	857		1,478	
Number of Controls	619	5,173	4,866	5,173		5,173	

Notes to Table 8: 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 error in parentheses. Each column provides estimates for a different sample of students. All models include development and year fixed effects as well as the following covariates: gender, age, age squared, free lunch, bilingual and special education status and household composition. Planned closures are defined as those in which there was at least a year between notification and vacancy; in unplanned closures vacancy occurred more than a year after the notification. [a]Conditional on having moved during this period.

Table 9: The Effects of Building Closures in the Robert Taylor Homes One Year After Announcement

Dependent Variables	Difference in Gains Between Treatments and Controls ($\Delta\overline{Y_t} - \Delta\overline{Y_c}$) (s.e.)		
	Vacancy Consolidation (1)	The "Hole" Gang Activity (2)	Maintenance Emergencies (3)
Relocation Outcomes			
Moved Since Notification	0.431 (0.042)	0.591 (0.021)	0.445 (0.016)
Miles from original residence[a]	1.222 (0.378)	0.667 (0.194)	-0.859 (0.132)
Census Tract Poverty Rate	-0.166 (0.025)	-0.257 (0.013)	-0.091 (0.008)
Living in Public Housing	-0.288 (0.043)	-0.664 (0.021)	-0.185 (0.015)
Changed Schools Since Notification	0.250 (.047)	0.484 (0.024)	0.093 (0.018)
% school peers meeting national norms in math	0.017 (0.006)	0.095 (0.006)	0.013 (0.003)
Educational Outcomes			
Math Score	-0.028 (0.136)	-0.073 (0.092)	0.077 (0.052)
Reading Score	0.015 (0.189)	-0.142 (0.111)	0.066 (0.067)
Average # of Absences	-0.375 (2.414)	0.787 (1.261)	0.108 (0.892)
GPA	0.172 (0.085)	-0.010 (0.092)	0.114 (0.046)
Old for Grade	-0.011 (0.023)	0.087 (0.025)	-0.042 (0.014)
Dropped Out	-0.006 (0.017)	0.011 (0.010)	0.001 (0.007)
Enrolled in School	0.059 (0.021)	-0.044 (0.017)	-0.013 (0.011)
% of Closures that were Planned	0	0	0
Number of Treatment Group Observations	128	340	1,154
Number of Control Group Observations	2,269	2,230	2,064

Notes to Table 9: One high rise was closed as part of a scheduled vacancy consolidation program in Fall 1997. Three notorious high-rises known as the "Hole" were closed in Fall 1998 in response to increased gang activity in the buildings. Several high-rises were evacuated and closed in January 1999 when burst pipes led to flooding and heating failures. [a]Conditional on having moved during this period.

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