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Blowing It Up and Knocking It Down: The Local and Citywide Effects of Demolishing High-Concentration Public Housing on Crime

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This paper estimates the effect that the closure and demolition of roughly 20,000 units of geographically concentrated high-rise public housing had on crime in Chicago. We estimate local effects of closures on crime in the neighborhoods where high-rises stood and in proximate neighborhoods. We also estimate the impact that households displaced from high-rises had on crime in the neighborhoods to which they moved and neighborhoods close to those. Overall, reductions in violent crime in and near the areas where high-rises were demolished greatly outweighed increases in violent crime associated with the arrival of displaced residents in new neighborhoods.

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1 Introduction

Large public housing developments, particularly those with high-rise buildings, have had a reputation as epicenters of crime and gang activity. This reputation developed from the mid-1970s through the mid-1990s as many large public housing developments originally built in the 1950s and 1960s became infamous for extremely high levels of poverty, poorly maintained units, and dangerous living conditions. In the 1990s, policymakers began to take measures aimed at changing public housing in ways that would reduce the concentration of poverty and thus reduce the negative externalities associated with concentrated poverty, including high crime.

However, the size and even the direction of the overall effect of de-concentrating public housing on city-wide crime have, to this point, remained unknown. On one side, researchers have postulated that in the presence of non-linearities in the relationship between neighborhood poverty rates and crime, spreading poverty more evenly throughout a city could lead to overall reductions in crime (Turner et al. (2007)).[1] Critics fear that crime will simply be displaced to the neighborhoods where former public housing residents move (Rosin (2008)).[2]

We use a change in federal policy to measure the effect of public housing de-concentration on crime. The policy (known as HOPE VI) made grants available for (and in some cases required) local public housing authorities to demolish and revitalize large public housing developments. We study the effects of this program in Chicago, where it led to the demolition of roughly 20,000 units of high-rise public housing and the relocation of the former residents to either private market housing (through the use of vouchers) or low-rise public housing (not slated for demolition), either permanently or temporarily until new mixed-income developments went up where high-rise developments were demolished.[3]

Estimating the effect that this massive demolition and relocation program had on crime throughout the city is challenging because it requires measurement of the

[1]Throughout this paper we use the terms neighborhood and geographical area to refer in general to Chicago community areas, Census tracts, Census block groups, and Census blocks. When necessary, we specify to which of these we are referring.

[2]There is a large literature concerned with negative externalities that arise from concentrated poverty (See Wilson (1987), Massey and Denton (1993), Sampson (2012)). Empirically, the causal link between concentrated poverty and crime has been harder to establish (See Durlauf (2004) and Heller et al. (2011)) for overviews.

[3]We define high-rises as buildings with 45 or more units sharing a single address. These are typically 8 story or taller buildings with elevators.

effect that closing and demolishing high-rise public housing developments had on their immediate and nearby neighborhoods, as well as the effect that displaced former high-rise residents had on the neighborhoods to which they moved. We overcome these challenges by using detailed data on the address and timing of the closure of almost every non-senior-citizen high-rise public housing building in Chicago prior to demolition. We track households displaced by the demolitions using administrative data from credit report histories showing how their Census block of residence changes over time. Finally, we use geographically detailed, Census block-level data on types of crime as outcome measures.

The key assumption for identification is that the timing of closures is not correlated with crime trends. We provide evidence showing that high-rise buildings with lower occupancy rates in 1990 were likely to be closed earlier than those with higher occupancy rates. However, there appears to be no relationship between levels or trends of crime prior to closure and the order in which buildings were closed. We also show evidence of similar pre-existing trends in crime in blocks that were selected by displaced public housing households and in blocks with similar characteristics. This alleviates the concern that households may be selecting neighborhoods based on pre-existing trends which would lead us to conflate these trends in crime with changes to crime that are due to the arrival of the displaced households.

Overall, reductions in violent crime in and near the areas where high-rises were demolished greatly outweighed increases in violent crime associated with the arrival of displaced residents in and near their new neighborhoods. Our estimates indicate that high-rise closures are associated with large reductions in crime in Census blocks in which the high-rises were located; ranging from a 33% reduction in theft to an 86% reduction in shots fired, relative to crime levels in those blocks in 1999. For most types of crime, these reductions spill over into the Census blocks within a half-mile of the high-rise block, but the reductions are smaller in magnitude, ranging from 7%, for theft, to 40%, for homicide, relative to the 1999 crime levels in these blocks.

We find an increase in crime associated with the arrival of displaced high-rise households in the Census blocks to which they relocate, for about half of the crime types. These effects range from about 2%, for disturbance, to 30%, for gang activity, of 1999 crime levels in these blocks. For the other half of the crime types, we find no effect of displaced households on crime. For assault and battery and burglary, there is also a measurable increase in crime in blocks within a half mile of the blocks where high-rise households relocate. However, these increases represent only 2.5% to 3.5% of 1999 crime levels in this set of blocks.

Overall, public housing demolitions appear to have had a big impact on homicide, shots fired, and vice and prostitution. Our analysis attributes a drop of between 5% and 10% of the 1999 city-wide levels of these crimes to the high-rise closures. There were smaller, but measurable, drops in city-wide assault and battery, theft, vandalism, and disturbance crimes attributed to public housing closure.

The next section discusses public housing demolition in the U.S. generally and in Chicago specifically. In Section 3, we document the sources of the data used in our analysis. Section 4 presents our identification strategy. We develop our empirical specification in Section 5. Our results are presented in Section 6. Section 7 provides a discussion of the results. Section 8 concludes.

2 Public Housing Demolition

From the mid-1970's through 1992, laws requiring one-for-one replacement of demolished units in order to qualify for HUD funding made demolition of public housing a prohibitively expensive option for local public housing authorities. However, after severe funding cuts during the 1980's, much of the public housing stock was in need of repair. In October 1992, a new housing bill and HUD appropriations bill changed the law to make funding available for demolition and redevelopment of distressed public housing developments. The program created by the law eventually became known as HOPE VI (the sixth iteration of a program identified by an acronym for "Housing Opportunities for People Everywhere").[4] One of the objectives of the HOPE VI program is to "provide housing that will avoid or decrease the concentration of very-low-income families."[5]

During the period from 1993 through 2006, the HOPE VI program awarded the Chicago Housing Authority (CHA) $258M in revitalization grants (representing 4.4% of the total). The CHA received an even larger share of the demolition grants. Of the 127 housing authorities awarded demolition grants from 1996 through 2003, the CHA received $83.4M earmarked for 12,500 units of public housing, representing about

[4]Polikoff (2006). HOPE VI program information is available at: http://www.hud.gov/offices/pih/programs/ph/hope6/about/

[5]Popkin et al. (2002).

21% of the total HOPE VI demolition grants awarded.

The scope of the HOPE VI program was broadened when, in 1996, the United States Congress passed a law that required local housing authorities to remove any units from their stock that cost more to maintain than the combined cost of demolition and provision of voucher-based private sector rental assistance (known as Section 8 Vouchers or Housing Choice Vouchers). As a result, in 1998, the CHA announced that all of Chicago's gallery-style high-rise public housing developments had failed the viability test and were slated for demolition. In February 2000, the CHA's Plan for Transformation was approved by HUD. The plan called for the demolition of roughly 22,000 units of public housing out of an existing stock of about 40,000 units. The remaining units were to be rehabilitated and an additional 8,000 units were to be constructed, leaving the city with approximately 25,000 new or revitalized units by the end of the ten-year plan, equivalent to the number of units that were occupied at the time the plan was drawn up. The proposed redevelopments focused on mixed-income housing employing private developers and management companies.[6] Under the plan, lease compliant households as of 1999 would be given the option of taking private market housing vouchers or relocating to low-rise public housing (not slated for demolition), either permanently or temporarily until new mixed-income developments were completed. The desire to remain lease compliant, and thus continue to receive a housing subsidy, provided a strong incentive to move when the CHA closed buildings (rather than moving preemptively in expectation of building closure).

What led to this drastic policy change in Chicago? Throughout the 1970s and early 1980s, a number of factors contributed to a change in the demographics of the tenants of public housing from a mixture of working-class and poor households to a more uniformly poor population. According to Hunt (2009), limits on nominal tenant income combined with the inflation of the late 1970s pushed out working class households. The Brooke Amendments requiring that rents be set at no more than 25% of tenant's income made public housing more attractive to very-low-income households and less attractive to working class households. This meant less rental revenue for the CHA (Hunt (2009), p. 204). By the 1990s, maintenance problems had become so bad that many units sat vacant, as they were in such a state of disrepair

[6]More information on the CHA's Plan for Transformation can be found at http://www.thecha.org/transformplan/files/plan_for_transformation_brochure.pdf. Rosenbaum et al. (1998) study Lake Parc Place, one of the first low-income public housing developments in Chicago that was converted to mixed-income housing.

as to not meet the minimum standards for the CHA to rent them out.

In 1991, prior to the passage of HOPE VI, tenants in the Henry Horner development (one of the developments containing high-rise public housing buildings on the West Side of Chicago) filed suit "accusing the CHA of intentionally not repairing and rerenting apartments" (Hunt (2009), p. 274). This suit resulted in a plan to demolish some of the Henry Horner high-rises and renovate the mid-rises. The timing of demolitions was subject both to uncertain legal negotiations and logistical concerns regarding displacement of tenants. Around the same time, similar legal challenges resulted in demolition and renovation plans at the ABLA, Cabrini, Lakefront, and Wells developments (Hunt (2009), p. 275). In each case, the timing of the execution of these plans was influenced by several exogenous factors. The first was that any plan to construct public housing in predominantly African-American neighborhoods in Chicago required court approval due to earlier litigation.[7] Second, the removal of the federal requirement of one-for-one replacement of any demolished public housing units meant that, beginning in the mid-1990's, new units did not have to be available before old buildings were demolished. The interaction of these two external factors combined with negotiations between tenant groups and the CHA added a large degree of uncertainty regarding the planning and execution of public housing closures and demolitions. Some of this uncertainty abated with the Plan for Transformation in 2000, but further uncertainty was introduced with the logistical challenges of closing and demolishing such a large number of public housing units. Planned closure and demolition dates often did not coincide with the actual date, sometimes with lags (and even leads) of several years.[8]

How might the closure and demolition of Chicago's high-rise public housing affect crime near the high-rises and throughout the rest of the city? There are a number of potential mechanisms which we classify into four categories. The closures may have brought about: 1) changes in residential location affecting employment opportunities. 2) changes in building structure affecting the probability of crime detection. 3) changes in peer groups possibly interacting with economies of agglomeration in crime. 4) a reduction in the number of places in the city where social norms and controls have broken down. We discuss these potential mechanisms in light of our results in the discussion section.

[7]See Polikoff (2006).

[8]See CHA Annual Reports 2000 - 2010.

3 Data and Descriptive Statistics

3.1 Public Housing Buildings and Closure Data

We use three sources to assemble a dataset on the stock of public housing units in Chicago from 1990 - 2011. The first source is an address-level inventory from the CHA of all public housing buildings, containing the number of units at each address. The second source is a list of building closures prior to demolition covering the period from 1990 - 2000. These first two sources were provided to us by Brian Jacob.[9] The third source is our compilation of high-rise public housing buildings closed prior to demolition from 2000 through 2011 taken from the CHA's annual plans and reports.

The building list provides the addresses of 42,681 units of public housing. Of these, 32,707 are family housing and 9,974 are senior housing. These datasets contain a total of 174 non-senior high-rise public housing addresses, which we define as addresses with 45 or more units.[10] We observe the year of closure for 161 of the 174 high-rise addresses during the period from 1990 to 2011.[11] The number of units in these high-rise addresses ranges from 47 to 203 with a mean of 112. The buildings are spread across 90 Census blocks. Figure 1 shows the annual number of high-rise building units closed prior to demolition by development. This figure illustrates the variation in timing of closures both within and across public housing developments. The data also contain occupancy rates for 156 of the high-rise buildings in 1990. The mean occupancy rate was 35%, while the 25th and 75th percentile were 28% and 42%, respectively.[12]

Neighborhoods with high-rise public housing developments were exceptionally poor. Figure 2 shows the population-weighted distribution of Census block group poverty rates for the city of Chicago from the 1990 Census and the 2005-2009 American Community Survey tabulations. Block groups containing high-rise public housing

[9]Brian Jacob is currently the Walter H. Annenberg Professor of Education Policy, Professor of Economics, and Professor of Education at the University of Michigan.

[10]We exclude 6 addresses with 45 or more units that are, in fact, not high-rise buildings, but smaller scattered site buildings which are still in use. There are also about 60 high-rise public housing buildings reserved for occupancy by senior citizens. These buildings are mostly scattered-site buildings and were not closed and demolished.

[11]The 13 remaining addresses are in the Dearborn Homes development, which was originally slated for demolition, but was instead renovated beginning in 2009.

[12]The existence of high vacancy rates despite a waiting list for public housing units was due to the fact that a large number of units had such profound maintenance problems that they were not being rented out.

are shown in light gray. The left panel of the figure reveals a mass of extreme poverty (80% to 90% poverty rates) mostly made up of high-rise public housing block groups in 1990. The right panel shows that toward the end of the sample period, the population living in high-rise public housing was almost negligible and the mass of population that had been living in extremely poor Census block groups was now more evenly spread across the distribution.

Table 1 shows that, in addition to poverty rates, other characteristics of areas with high-rises were quite exceptional when compared with other areas in the city. The table provides means of demographic characteristics for the areas containing high-rise public housing (left panel) and for the city as a whole (right panel). The means come from the 1990 and 2000 Censuses by way of the National Historical Geographic Information System (NHGIS) and are compiled for Census tracts, block groups, and Census blocks.[13]

The mean population in the high-rise block groups fell sharply from 1,861 in 1990 to 1,223 in 2000. This was accompanied by a reduction in the mean number of housing units from 758 to 583. In contrast, for the city as a whole, the population and number of housing units increased slightly from 1990 to 2000.

Housing unit vacancy rates were also quite high in the high-rise areas. Vacancy rates were already 25% in high-rise block groups in 1990 and rose to 30% by 2000. Part of this can be attributed to the lag between closures and building demolition. The mean block level vacancy rate for the 90 high-rise blocks was about 36% in 2000, compared to only about 8% for the city as a whole. High-rise blocks also had much lower owner-occupancy rates, higher shares of African-American residents, and lower shares of Hispanic residents than the city as a whole. In both 1990 and 2000, the means of the median home value, median rent, and median household income in the high-rise block groups were about half or less than those means for the city as a whole.

3.2 Crime Data

We use crime data from two sources. The first is an extended version of Block and Block's Homicides in Chicago (Block et al. (1998)) that contains an annual count of homicides. These are administrative data compiled from the records of the Chicago

[13]Block level tabulations for 1990 are limited and are unavailable from the NHGIS (Minnesota Population Center (2011)).

Police Department.[14] This dataset has homicide counts at the tract level from 1970 to 2011 and at the block level from 1991 to 2011. In Chicago, there were around 800 homicides per year within the city limits from 1970 through the mid-1990's, when that number began to fall. The decline ended in 2004. Since then, there have been about 450 to 500 homicides per year in Chicago.

Until 1995, the data also contain additional information such as the "causal factor" of the homicide. When thinking about the nature of homicide in Chicago during this period, it is important to understand that gang altercations were the dominant causal factor. From 1990 to 1995, 26% of homicides were attributed to gang altercations. In contrast, only 7.4% were attributed to domestic altercations. A total of 58% of homicides were attributed to gangs, drugs, money, theft, robbery, and retaliation during this period.

The second dataset contains calls-for-service data regarding certain categories of crime from the City of Chicago's Office of Emergency Management and Communications (OEMC) for the years 1999 through 2011. The OEMC operates the city's 911 services. Calls for service regarding crime can originate from individuals calling 911, police officers' reports of incidents in the field, or reports from other public agencies.

Calls-for-service data are a standard measure used in the criminology literature (Maxfield and Babbie (2005)). While crime data are publicly available for the city of Chicago, they have several limitations compared to calls for service. The biggest limitation is that the last two digits of the address are withheld in the crime data whereas the full address is present in the calls-for-service data. This means that crimes can only be coded down to the 100s-level of the block's address, usually about an eighth of a mile, and also that determining the Census block where the crime occurred is not possible. The second limitation is that the first full year of crime data is 2002; the calls-for-service data give us 3 additional years of data (1999-2001).[15] Additionally, using the shorter panel of crime data results in 24% fewer high-rise blocks with a pre-closure sample period. Finally, the calls-for-service data provide a better measure of crimes that have low arrest rates, for example, calls reporting shots fired. While concerned citizens are likely to call the police to report hearing shots fired, it is unlikely that police actually find the culprit. In 2005, the mean number of shots fired calls was 107 per Census tract, while the mean number of weapons crimes

[14] See http://www.icpsr.umich.edu/icpsrweb/content/NACJD/guides/hc.html for details.

[15] For these reasons, specifications using block-level data yield extremely noisy estimates.

was only 5 per Census tract. However, these two measures have a correlation of 0.72 across tracts.[16] A similar pattern is evident when comparing drug, prostitution, and trespassing-related calls for service and crimes. For most of the other crime categories where we have both calls for service and crime statistics, the tract-level correlations are quite high, ranging from 0.8 to 0.9 for vandalism, auto theft, burglary, and assault and battery.

We use numbers of homicides or numbers of calls-for-service as our outcome measures rather than per capita rates of these variables for several reasons. The first reason is that we are interested in measuring how crime is distributed throughout space in the city, not necessarily its intensity with respect to population. Ihlanfeldt and Mayock (2010) argue that crimes per unit of land area better reflect the probability of exposure to crime (defined as being a victim or witness) than crime rates at levels of geography smaller than city-level.[17] Also, with geographic areas as small as Census blocks, variation in population by block may not reflect where people spend their time. Furthermore, precise population measures are not available at an annual frequency at the Census tract, block group, or block level. However, we control for population using a proxy (calculated using the credit history data described in the next subsection) as an explanatory variable. We also show that our results are robust to using a noisy measure of crime rates as the dependent variable, constructed by dividing crime counts by the population proxy.

Table 2 shows mean counts of homicides and calls for service by type of crime for 1999 and 2011 in the high-rise blocks (left panel) and in the city as a whole (right panel). The means fell sharply for each type of crime in the high-rise blocks from 1999 to 2011. For most types of crime, the means were still higher in the high-rise blocks than in the city as a whole in 2011. The exceptions were burglary, which was at about the same level, and gang activity, which was actually lower in the high-rise blocks in 2011 than in the city as a whole.

3.3 Relocation Data

In order to assess the impact of households displaced by high-rise public housing building closures on the geographic areas which they move to, we use the Federal

[16]These numbers are almost the same for the entire 10-year period from 2002 through 2011.

[17]We show that our results are similar when we use crime per unit of area as the dependent variable. See Section B of the online appendix.

Reserve Bank of New York Consumer Credit Panel / Equifax data (CCP). The CCP is based upon a five-percent sample of social security numbers in the United States. To appear in the data, a person must also have a credit history. In general, people will have a credit history if they have or have had credit accounts or are an authorized user of a credit account. They will also have a credit history if they have credit-related public records, such as a foreclosure or bankruptcy. Finally, people may have a credit history if credit-related inquiries have been made regarding them.[18] A key feature of the CCP data is that it provides quarterly observations of the Census block in which the individual lives. The only other demographic information that is provided is the age of the individual. The other variables focus on various types of debt accounts and the balances of those accounts.

We use the CCP data to track the movement of households that are displaced by high-rise public housing closures by forming the set of all individuals that were living in high-rise blocks during the year of a closure or during either of the two years prior to a closure. One potential concern is that Census blocks may contain housing other than the high-rise public housing buildings. However, for most of the blocks which still had high-rises as of 2000, the number of high-rise units in the CHA building list is equal to the number of total housing units in the 2000 Census. Another potential concern is that large portions of public housing residents could be absent from the data if they either do not have a social security number or do not have a credit history. The first concern is allayed by the fact that the high-rise population is overwhelmingly native-born African American and thus very likely to have a social security number.

There is still the concern that individuals may be absent from the data due to not having a credit history. The fact that credit inquiries can cause people to appear in the CCP data is important for our purposes. The current policy of the CHA is to run credit checks on applicants.[19] This policy has been in place dating back at least to 1990 (McRoberts (1995)). This is one way that CHA residents could show up in the CCP. Figure 3 shows population counts from the 2000 Census and the population implied by the CCP in the high-rise blocks for eight different age ranges. The main thing to notice in Figure 3 is that of individuals aged 25 to 54, a high share of residents in these blocks have credit histories (72%). As one might expect, the coverage ratio is lower (near 40%) for those aged 18 to 24, and about zero for people under 18.

[18]See Avery et al. (2003) for a general analysis of credit reporting data.

[19]See http://www.thecha.org/pages/family_wait_list_lottery_faqs/76.php

However, Census Public Use Microdata from 2000 show that, for a population that is likely to live in high-rise public housing, 85% of those aged 12-24 (the portion of the age range that is not well represented in the CCP and that is old enough to be involved in crime) live with a head of household who is between the ages of 25 and 54.[20] Thus, while the age ranges that may be most likely to engage in crime are not well represented in the CCP, these individuals are likely to reside with a household head in an age range that is well represented in the CCP.[21] While it is possible that households may break up when families displaced by high-rise public housing closures move, our hope is that by being able to track the head of household to their new place of residence, we are also able to track younger members of the household.

Despite the fairly high coverage rate of adults, there is still a concern that individuals with the highest propensity to commit crime may be the least likely to have a credit history. To address this concern, we examine the relationship between the CCP coverage rate and the crime rate of the high-rise blocks that had not yet been closed as of 2000. This leaves 53 blocks. We form the CCP coverage rate by multiplying the number of observations in the CCP in the first quarter of 2000 by 20 and dividing that by the Census population count (for all ages) for the block for 2000. There is considerable variation in this coverage rate. The 25th percentile block has 30% coverage, while the 75th percentile block has 62% coverage. Table 3 presents results of a univariate regression of the crime rate in 2000 for each type of crime on the CCP coverage rate using the sample of 53 blocks that still had high-rise public housing. If it were the case that high-rise residents without credit histories had a higher propensity for crime, then we would expect a negative relationship between CCP coverage and crime rates. Instead, the coefficient on CCP coverage is positive but not statistically different from zero for each type of crime.[22] These results make us less worried that we are missing the most crime-prone individuals in our measure of displaced public housing residents.

[20]While the Census does not ask about public housing directly, we construct a set of households that are likely to live in high-rise public housing by limiting the sample to African Americans living in Chicago in 50+ unit buildings built prior to 1970, paying gross rents less than $500 per month, and with household annual incomes less than $17,500. The weighted count of households in this group is 15,688, slightly larger but not too far from the number of high-rise public housing households that we would expect in 2000 (when about 12,500 units in non-senior-citizen high-rise public housing buildings had not yet been closed for demolition). Source: Ruggles et al. (2010).

[21]Furthermore, young heads of household are less likely to reside in high-rise blocks due to long waiting lists for public housing in Chicago.

[22]The only exception is for auto theft, where the coefficient is marginally statistically significant.

Looking across the full sample period of the CCP (1999Q1 - 2012Q4), we identify 765 individuals in the 5% sample (representing a population of 15,300) living in high-rise Census blocks in the year of closure or either of the two years prior. We flag the last quarter that we see these individuals living in a high-rise block as the last quarter before a closure-induced "move." We are able to observe 694 of the 765 individuals in the data one year after the "move" and 622 individuals two years after the "move." Of the people that we did observe one and two years later, about 93% were still living in Illinois, and about 89% were still living in the city of Chicago (with very little change in the share still in Illinois and the share still in Chicago between one and two years after the move). Two years after closures occurred, 42% of these individuals were still in a Census block that contained or had contained high-rises (that drops to 31% three years after closure), 15% were in a block that contained low-rise public housing, and 43% were in a block that did not contain any public housing (that rises to 55% at three years after closure).

The mean distance moved after two years was 4.3 miles for people who moved to blocks without public housing, 1.1 miles for people who moved to blocks that contain low-rise public housing, and 0.0 miles for people who remained in blocks with high-rise public housing. In general, people displaced by the high-rise closures wound up dispersed farther to the west on the West Side, and farther to the south on the South Side. There are two potential ways households could be observed living in blocks with high-rise public housing after a high-rise closure. One possibility is that they were living in Census blocks with more than one high-rise public housing building and thus even after one building closed, they were among a set of households that were still living in the block but in another building. The other possibility is that there are households for which Equifax did not receive an updated address.[23] We note that it is in the interest of creditors to discover any new addresses of people that they have extended credit to as quickly as possible. However, there is still some chance that addresses will be updated with a lag. If the location choices of the households whose addresses do not get updated are highly correlated with those that do get updated, then delay in updating addresses would simply add measurement error to the explanatory variables measuring the number of displaced public housing households in each Census block. However, if these households move to different

[23]Equifax uses a proprietary algorithm to update addresses based on aggregating changes of addresses reported by creditors.

neighborhoods, then we would most likely miss any crime changes associated with their arrival in those neighborhoods until their address was actually updated.[24]

Table 4 shows mean demographic characteristics from the 2000 Census for three groups of Census blocks within the city of Chicago. The means for each group make up a column of the table. The group in the first column consists of the 393 blocks where displaced households moved that had no public housing. The second column shows means for the 168 blocks where displaced households moved that had low-rise public housing buildings. The last column shows means of the 90 blocks that had high-rise public housing buildings as of 1990 (before demolitions began). The table reveals that the blocks to which displaced households moved had fewer housing units, lower population density, lower vacancy rates, and a higher fraction of owner-occupied units than the high-rise blocks. The receiving blocks also had a slightly lower share of African American residents, a higher share of Hispanic residents, and were farther from the central business district (CBD) than the high-rise blocks. The last three rows of Table 4 report means of median rent, median income and poverty rate.[25] The high-rise blocks have much lower median rents and incomes than the receiving blocks. Finally, the last row reveals that households displaced from high-rise blocks are moving from extremely high-poverty blocks (62%) to somewhat high-poverty blocks (34% or 27%). These moves look similar to those made by the Section 8 voucher recipients observed in Moving to Opportunity (Kling et al. (2007)).

Finally, we note that the displaced public housing residents make up a large enough share of the total population in the Census blocks to which they move that it is plausible that we might be able to measure their impact on crime. In blocks to which migrants move, they account for 11.3% of the total population in the years they are present.

[24]We discuss measurement error in our measure of the number of displaced households and a check that helps allay some concern about this issue in the online appendix. However, there is still a concern that bias induced by these measurement issues could lead us to understate the displacement effects on crime.

[25]These variables are only available at the block group level: The means reported are with respect to blocks, but the variation is at the block group level.

4 Identification Strategy

Our goal is to determine the average change in crime due to public housing closures prior to demolition. A key obstacle to achieving this goal is identifying the correct counterfactual level of crime had public housing not been closed. A host of other factors may have contributed to the decrease in crime in Chicago during the period in which public housing was being demolished, and could potentially lead to a spurious correlation between public housing demolition and decreases in crime.

In this context, standard difference-in-differences estimators are unlikely to yield valid identification because the neighborhoods where high-rise public housing units were built are not comparable to other neighborhoods in Chicago. Comparing neighborhoods via statistical methods of re-weighting or matching (such as by the propensity score) is not feasible due to a lack of common support (Heckman et al. (1998)), even when control neighborhoods are chosen from neighborhoods where high-rise public housing sites were proposed but rejected.[26] Due to these obstacles to using cross-sectional variation to identify the effects of interest, instead, we exploit variation in the timing and number of building closures prior to demolition among the neighborhoods which had high-rise public housing. Instead of comparisons at the same point in time between different neighborhoods with and without high-rise public housing, we compare neighborhoods with high-rise public housing to themselves before and after building closures.

The key assumption for this identification strategy is that the timing of public housing closures is not related to prior trends in crime or other factors influencing crime trends. Figure 4 presents evidence in support of this assumption. Our block-level homicide data go back to 1991, allowing us to examine whether trends in the average number of homicides were different in blocks where high-rise closures began earlier rather than later. We break the sample into roughly equal groups based on the year of the first high-rise closure in the block. The first group spans the first 6 years of the demolition program while the next two span the subsequent two 5 year periods. Figure 4 reveals that the mean number of homicides in each group was quite similar (and much higher than for blocks elsewhere in the city) and exhibited the same downward trend through the 1990s. These time trends represent pre-trends for blocks where closures began in 1999 or later (the dotted and dashed lines). While

[26] See Section A of the online appendix.

there is a difference in levels between the dotted and dashed lines in 1991, they are quite similar from 1992 onward.[27] It appears that whether closures began early or late in the 2000s did not depend on the levels or trends of homicides in the 1990s. The trend is also remarkably similar for the group of blocks in which closures began in the 1990s (the solid line).

There are several reasons why we think that public housing officials did not select buildings for closure based on characteristics that could be correlated with crime: 1) The demolition program included the entire stock of non-senior high-rise public housing buildings.[28] 2) Our estimates of the effect of demolition on crime do not appear to be sensitive to the sample period used. If more-crime-ridden buildings were targeted first, we would expect to get larger estimates when using the full sample than when using only the later years. 3) Our estimates are robust to excluding the development with the highest land value (Cabrini-Green), suggesting that our results are not driven by gentrification.[29] Furthermore, the year of the first high-rise closure in each high-rise block is not correlated with levels of mean household income in 1990 in the Census tracts containing the high-rise blocks or trends in mean household income from 1980 to 1990 in those Census tracts.[30]

Consistent with the evidence shown in Figure 4, anecdotal evidence suggests that the logistical challenge of closing large public housing developments combined with various legal challenges put forth by some of the tenants' organizations generated randomness in the timing of high-rise building closures.[31] As one way of checking whether buildings tended to be selected for closure in response to elevated crime

[27]The trends are statistically indistinguishable whether we group blocks in this way, by roughly equal numbers of years, or by roughly equal population counts from 1990. The latter grouping breaks the blocks up into groups which had their first high-rise closure from 1993-1995, 1996-2001, and 2002-2008.

[28]The exception was the Dearborn Homes, which did not end up being demolished. However, this decision was not made until the very end of the sample period after several buildings had already been closed in advance of what was thought to be their demolition.

[29]See Section B of the online appendix.

[30]Another possible test is to see whether levels and trends of property values are correlated with the year of the first high-rise closure. However, fewer than half of the Census tracts in which the high-rise Census blocks are located have enough owner-occupied housing units for the Census to report home value tabulations. There is a weak relationship between the year of the first high-rise closure in a Census block and the distance of the block to the CBD. However, the relationship implies that on average blocks with later closure dates were closer to the CBD, the opposite of what one would expect if gentrification emanating from the CBD outward were driving high-rise closure decisions.

[31] See Hunt (2009) pages 274 - 275.

in prior years, we look for a correlation between crime rates prior to closures and the year that the high-rise closure occurred. We found no cross-sectional correlation within high-rise Census blocks between the year that the first high-rise was closed for demolition and the number of homicides (averaged over the 1990's) or the number of violent crimes (in 1999).[32] We also investigate the relationship between high-rise building occupancy rates in 1990 and homicides from 1991 through 1995. We find no relationship between the two. However, we do find a relationship between the 1990 building occupancy rate and the year that the high-rise was closed prior to demolition. On average, a percentage point increase in a building's 1990 occupancy rate is associated with a 0.117 year (standard error 0.043) later closure date. Thus, it appears that the order of building closures had more to do with the logistical concerns of relocating residents than a strategy of closing high-crime buildings.

The parameters of interest are identified by variation in the timing and number of unit closures across the affected geographic areas. It is important to note that inclusion of geographic area fixed-effects will absorb any unobservable characteristics of geographic areas that are time-invariant. For example, these fixed-effects will control for persistent differences in land use or city structure between geographic areas which may affect crime. Furthermore, the year-effects will absorb common transitory shocks to the set of geographic areas in the sample. For example, aggregate changes and trends in crime will be controlled for. If each geographic unit had at most one high-rise closure, then our specification would be an event study.[33] As such, our estimates can be thought of as the average of the differences between post-closure crime levels and pre-closure crime levels. Our preferred, block-level, specification is very close to a pure event study since many of the blocks that have high-rises have only one high-rise building. We enrich the specification by interacting the closure event with the number of units in the building being closed. To identify the effect of closures on nearby areas, we exploit the same variation in timing of the closures as well as variation in the distance to the nearest (and second- and third-nearest) high-rise area.

Analogously, our estimates of the impact of displaced households on crime in the blocks to which they move can be thought of as the difference between the average level of crime in the block after receiving a displaced household and the average level

[32] The correlations are -0.13 and -0.14, respectively; neither is statistically different from zero.
[33] See Jacobson et al. (1993).

of crime in the block before receiving a displaced household. To estimate the effect of displaced households on crime in nearby blocks, we again use the timing of the arrival of the displaced household and interact that with the distance to the nearest block that receives a displaced household.

The key identifying assumption for estimating the effect that residents displaced by high-rise closures had on the geographic areas to which they moved, is that households are not selecting geographic areas based on pre-existing trends. Figure 5 shows mean homicides per block for the set of blocks with low-rise public housing (solid line), the set of remaining blocks where displaced public housing households move, labelled "Private Market Blocks" (dashed line), and a third set of blocks that is over 50% African American and is above the median poverty rate for the City of Chicago (dotted line).[34] While the number of homicides is lower on average for this third set of blocks, a linear trend from 1995 through 1999 is statistically indistinguishable from the voucher blocks. Thus, it appears that there were similar trends in homicides, in the years leading up to the start of the CCP data sample period, in the blocks that were chosen by displaced public housing residents with private market vouchers and in those that were not chosen, but may have been in the choice set. Differences in levels will be swept out by block fixed effects. While our main specification requires a common trend assumption (which is supported by the evidence in Figure 5), robustness specifications in Section C of the online appendix allow for either linear or arbitrary time trends for each block group.

A second concern is due to measurement issues related to the coverage and sampling in the credit report data set. The two main issues are selection bias, due to only observing individuals with a credit history, and measurement error, due to observing only a random sample of that group. We addressed the issue of selection bias in Section 3.3. We discuss specification checks to address the issue of measurement error in Section C of the online appendix.

5 Empirical Specification

Our econometric specification aims to measure the overall effect of high-rise public housing closures on crime in Chicago. We are interested in measuring the direct

[34]The mean of the remaining blocks (excluding high-rise blocks) in Chicago is shown for reference (alternating dashes and dots).

effect of high-rise public housing closures on crime in the geographic areas where the high-rises are located (and spillover effects nearby) and the direct effect that residents displaced by high-rise closures had on the geographic areas to which they moved (and spillover effects nearby). The sum of these effects yields an estimate of the total effect of the closures on crime in Chicago.

Suppose Chicago has I geographic areas. For each geographic area $i \in \{1, \ldots, I\}$, define geographic area $i_1 \in \{1, \ldots, i-1, i+1, \ldots I\}$ as the nearest geographic area containing a public housing high-rise, i_2 in the same set as the second-nearest geographic area, and i_3 as the third-nearest geographic area containing a public housing high-rise.[35] We specify crime for geographic area i at time t to be determined as follows:

$$Crime_{i,t} = \beta_D H_{i,t} + \beta_S H_{i_1-i_3,t} + \beta_M M_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}, \tag{1}$$

The first term represents the direct effect of the closures in the geographic areas in which the high-rises were located. $H_{i,t}$ is the number of units of high-rise public housing that are still open in geographic area i at time t.[36] This variable is expressed in terms of 100's of units. In areas where closures occur, $H_{i,t}$ will take on a positive value in the beginning of the sample period and decrease (possibly to zero) by the end of the sample period. For all other areas in the city, $H_{i,t}$ will be equal to zero throughout the entire period.

The second term represents the spillover effects to the areas nearby the high-rise closures and is defined as[37]

$$\begin{aligned}\beta_S H_{i_1-i_3,t} \equiv \beta_{S1} ln(d(i,i_1)) H_{i_1,t} \mathbf{1}\{0 < d(i,i_1) < 0.5\} + \\ \beta_{S2} ln(d(i,i_2)) H_{i_2,t} \mathbf{1}\{0 < d(i,i_2) < 0.5\} + \\ \beta_{S3} ln(d(i,i_3)) H_{i_3,t} \mathbf{1}\{0 < d(i,i_3) < 0.5\}.\end{aligned}$$

where $d(i, i_k)$ is the distance, measured in miles, from geographic area i to geographic area i_k.

The third term represents the displacement effects (including both the direct and

[35] We define nearest as the smallest distance between the centroids of each geographic area.

[36] By "open," we mean that they have not yet been closed prior to demolition.

[37] In Section B of the online appendix, we show that the log function characterizes the dissipation of the effects with distance well and that the effects are near zero at distances greater than 0.5 miles.

spillover effects). In order to assess the degree to which crime is associated with the arrival of displaced public housing residents in their new geographic areas, we use the CCP to identify a sample of individuals living in high-rise blocks in the year of, or either of the two years prior to, a building closure. We then follow this sample from the time of the closure through 2011. From this sample, we construct M_{it}, to be the number of displaced sampled migrants who move into Census block i at time t.

The third term is defined as

$$\begin{aligned}\beta_M M_{i,t} \equiv \beta_{M1} M_{i,t} \mathbf{1}\{\text{no public housing in block } i\} \\ + \beta_{M2} M_{i,t} \mathbf{1}\{\text{low-rise public housing in block } i\} \\ + \beta_{M3} \ln(d(i, j_1)) M_{j_1,t} \mathbf{1}\{0 < d(i, j_1) \leq 0.5\}.\end{aligned}$$

where j_1 is the nearest Census block to block i receiving public housing migrants.

We include block fixed effects (α_i) to control for persistent differences in crime across different areas of the city and time dummies (γ_t) to control for secular trends in crime. Finally, $\epsilon_{i,t}$ represents unobserved determinants of crime in geographic area i at time t.

In addition to regression coefficients, we also report estimation results in terms of the change in the annual number of homicides in the city associated with the change in the number of high-rise units from the beginning to the end of the sample period. We decompose the local effect of high-rise building closures for the city of Chicago into a direct effect and an analogous spillover effect using the parameters of our model as:

$$\begin{aligned}\Lambda^D &\equiv \sum_{i=1}^{I} \beta_D (H_{i,T} - H_{i,1}) \\ \Lambda^S &\equiv \sum_{i=1}^{I} \sum_{j=1}^{3} \beta_{Sj} ln(d(i, i_j))(H_{i_j,T} - H_{i_j,1}) \mathbf{1}\{0 < d(i, i_j) < 0.5\}\end{aligned}$$

In order to give a sense of their magnitudes, we also report the parameters Λ^D and Λ^S normalized by the number of crimes in various geographic areas in year $t = 1$ (the initial year of the sample). The first normalization we report divides Λ^D and Λ^S by the total number of crimes in the affected areas (denoted by subscript AA), which for Λ^D are all geographic areas $k \in \{1, \ldots, K^1\}$ containing high-rises, and for Λ^S are all geographic areas $k \in \{1, \ldots, K^2\}$ within 0.5 miles of a geographic area with a high-rise (excluding the high-rise areas):

$$\overline{\Lambda}^D_{AA} \equiv \frac{\Lambda^D}{\sum_{k=1}^{K^1} Crime_{it=1}}$$
$$\overline{\Lambda}^S_{AA} \equiv \frac{\Lambda^S}{\sum_{k=1}^{K^2} Crime_{it=1}}$$

We also report Λ^D and Λ^N normalized by the total crime occurring in all geographic areas $i \in \{1, \ldots, I\}$ city-wide (denoted by subscript CW):

$$\overline{\Lambda}^D_{CW} \equiv \frac{\Lambda^D}{\sum_{i=1}^{I} Crime_{it=1}}$$
$$\overline{\Lambda}^S_{CW} \equiv \frac{\Lambda^S}{\sum_{i=1}^{I} Crime_{it=1}}$$

Analogously to the definitions above, we also decompose the effect of displaced migrants from high-rise building closures into a direct and spillover effect:

$$\Delta^D \equiv \sum_{i=1}^{I} [\beta_{M1}(M_{i,T} - M_{i,1})\mathbf{1}\{\text{no public housing in block } i\} + \beta_{M2}(M_{i,T} - M_{i,1})\mathbf{1}\{\text{low-rise public housing in block } i\}]. \quad (2)$$

$$\Delta^S \equiv \sum_{i=1}^{I} \beta_{M3} \ln(d(i, j_1))(M_{j_1,T} - M_{j_1,1})\mathbf{1}\{0 < d(i, j_1) \leq 0.5\}. \quad (3)$$

The affected area ($\overline{\Delta}^D_{AA}$ and $\overline{\Delta}^S_{AA}$) and city-wide ($\overline{\Delta}^D_{CW}$ and $\overline{\Delta}^S_{CW}$) displacement parameters are defined analogously to the local parameters defined above except that the affected areas are all geographic areas to which displaced households moved and all geographic areas within 0.5 miles of those areas (excluding the areas to which displaced households moved).

6 Results

6.1 Main Results

Table 5 shows all estimated coefficients and controls of Equation 1 using homicide as the dependent variable.[38] We present only the full specification for one crime type due to space constraints. We choose to show these estimates for homicide as it is the most accurately reported type of crime. The first column shows OLS regression coefficient estimates of β_D, β_S, and β_M using a sample of all census blocks in the City of Chicago from 1999 - 2011. The second column displays the implied change in the number of homicides per year associated with the total change in each of the explanatory variables from 1999 to 2011. For example, the coefficient on high-rise units, β_D, reported in the first row implies that closing 100 units of high-rise public housing is associated with about 0.15 fewer homicides per year. From 1999 through 2011, 10,182 units of high-rise public housing were closed implying the drop of 15.1 homicides per year shown in the righthand column. The coefficients shown in the second and third rows are both negative and statistically different from zero. This implies that closures of high-rise units can explain drops in homicide in blocks within a half mile of the high-rises and that the effect decays with distance. Given the changes in the number of high-rises in the nearest and second nearest high-rise blocks and the distance (measured in miles) to those blocks, the estimated coefficients imply that the effects of the closures spill over into blocks within a half mile of the high-rise blocks and can explain reductions of 14.7 and 15.6 homicides per year, respectively. The coefficient on distance to the third nearest high-rise block is not statistically different from zero.

The fifth and sixth rows report the coefficients and the number of displaced households interacted by indicators of whether the block contains no public housing, or low-rise public housing, respectively. The seventh row shows the estimate of the coefficient on distance to the nearest block that received households displaced from high-rise public housing interacted with the number of those households in the receiving block. The estimates reveal that there are no statistically significant changes in homicides attributed to receiving displaced households from high-rise public housing in the census blocks to which they move. Also, there are no detectable spillovers to

[38] Estimates of the coefficients using other crime types as the dependent variable are shown in Section C of the online appendix.

blocks within a half mile of the blocks that receive households displaced from public housing.

We also add a count of the mean number of observations in each block per year in the CCP data as a control for changes in population. We add this control to alleviate concern that the changes in crime that we are measuring in the high-rise blocks and in the blocks to which displaced households move may be driven by a mechanical relationship between population and crime. The coefficient on this CCP population proxy is shown in the eighth row. While it is not statistically significant when homicide is the dependent variable, it is positive and significant for some of the other crime types. However, inclusion or omission of this variable has very little impact on our estimates of the coefficients of interest and no impact on our conclusions. As a robustness check, we also estimate specifications using crime rates as outcomes. We form the crime rate variables by dividing our crime counts by the number of observations in the CCP data. These alternative specifications do not change our conclusions regarding the city-wide effect of the demolition program.[39]

To get a sense of the size of the changes in crime associated with public housing closures and resident displacement, we are interested in comparing the implied changes in crime shown in the righthand column of Table 5 to the 1999 levels of crime in both the affected geographical areas and the City of Chicago as a whole. Note, that the implied change in homicides shown in the first row corresponds to the Λ^D parameter defined in the previous section. The sum of the implied changes in rows 2 through 4 are equal to Λ^S. Δ^D is found by summing the implied changes in rows 5 and 6, and Δ^S is the implied change shown in the seventh row. Also following the definitions in the previous section, we normalize the four parameters: Λ^D, Λ^S, Δ^D, and Δ^S by the total number of homicides that occurred in 1999 in either the affected areas (the high-rise blocks, the blocks within a half mile of the high-rise blocks, the blocks where displaced households relocated, and the blocks within a half mile of those) or in the entire City of Chicago. We report these normalized effects in Tables 6 and 7.

[39]For homicide per capita, our estimate of the total city-wide effect of the demolition program is -6.6% if we count block * years with no homicides and no population as 0 homicides per capita (this drops 0.2% of the total observations). If we drop all observations with zero CCP observations, the effect is -11.6% (this drops 31.2% of the total observations). Both results are statistically significant at the 5% level. These numbers are comparable to the total effect of -8.5% reported in the homicide column of Table 6.

Focussing on the local effects of high-rise closures, Table 6 shows that these normalized effects are of a large magnitude for the affected areas (upper panel). The change in the number of high-rise units explains a 63 percent drop in the number of homicides per year occurring in the blocks in which high rise buildings were closed ($\overline{\Lambda}_{AA}^{D}$), and a 39 percent drop in the number of homicides per year occurring in the blocks within a half mile of the high-rise blocks ($\overline{\Lambda}_{AA}^{S}$). Normalizing by the the total number of homicides, city-wide, these local effects represent 2.5 and 4.9 percent reductions in homicides per year from their 1999 levels for the City of Chicago as a whole ($\overline{\Lambda}_{CW}^{D}$ and $\overline{\Lambda}_{CW}^{S}$ in the lower panel).

The arrival of displaced residents was associated with statistically insignificant changes in homicides in the blocks to which they relocated ($\overline{\Delta}_{AA}^{D}$), as well as for the blocks within a half mile of those blocks ($\overline{\Delta}_{AA}^{N}$). When the local effects (direct and spillover) and displacement effects (direct and spillover) are all added up, the total reduction in homicides explained by the closure and demolition of high-rise public housing buildings was 8.5 percent of all homicides in Chicago in 1999 (shown in the last row of Table 6).

Looking across Tables 6 and 7 reveals that the local effects of high-rise public housing closures vary by type of crime and tend to be largest for violent crimes. The upper panels of Tables 6 and 7 show that high-rise closures are associated with large reductions in crime in Census blocks in which the high-rises were located; ranging from a 33% reduction in theft to an 86% reduction in shots fired, relative to crime levels in those blocks in 1999. For most types of crime, these reductions spill over into the Census blocks within a half-mile radius of the high-rise blocks, but the reductions are smaller in magnitude, ranging from 7%, for theft, to 40%, for homicide, relative to 1999 crime levels in these blocks.

The lower panels of Tables 6 and 7 show the same effects as the upper panels, except that they are relative to the city-wide levels of crime in 1999. For homicide, shots fired, assault and battery, vice and prostitution, and trespassing, both the local direct and local spillover effects show reductions of about 2% or more. These results are consistent with the findings in Hartley (2010) and Sandler (2012) suggesting that the largest effects of demolitions are on violent crimes.

Some property crimes show smaller, yet still statistically significant, reductions for both the local direct and local spillover effects. These include burglary, theft, vandalism, and disturbance. The overall effects are also relatively small for other non-violent crimes. Drug crimes and auto thefts show small statistically significant local direct effects but no significant local spillover effects.

We estimate a displacement direct effect that is positive and statistically different from zero for the following types of crime: assault and battery, gang activity, burglary, trespassing, and disturbance. These effects (shown on the third row of the upper panel of Tables 6 and 7) range from about 2%, for disturbance, to 30%, for gang activity, of 1999 crime levels in these affected blocks. For assault and battery and burglary, there is also a measurable increase in crime in blocks within a half mile of the blocks where high-rise households relocate. However, these increases represent only 2.5% to 3.5% of 1999 crime levels in this set of blocks (row 4 of the upper panel of Tables 6 and 7). Thus, for several crime categories, an influx of former high-rise public housing residents is associated with more crime in the block to which they move or in blocks within a half-mile of those blocks.

Rows 3 and 4 of the lower panels of Tables 6 and 7 show displacement effects relative to the total city-wide level of that crime type in 1999. For assault and battery and burglary, the increases in crime in the receiving blocks associated with displaced public housing households (i.e., the sum of displacement direct and displacement spillover effects, $\overline{\Delta}^{D}_{CW}+\overline{\Delta}^{N}_{CW}$) were a little over 2%. For gang activity and trespassing, the increases associated with displaced high-rise public housing residents were about 6.3% and 5.4% of 1999 city-wide crime levels, respectively. These are sizable increases. However, as the estimates presented in the final row show, the increases in assault and battery, burglary, and trespassing in the receiving blocks were offset by (or outweighed by in the case of assault and battery) decreases in crime associated with building closure in or within a half mile of the blocks where the high-rises had stood. The exception is gang activity, which our estimates indicate has increased in response to high-rise public housing closures by about 6.1%. However, this estimate is not statistically distinguishable from zero at the 5% significance level. Furthermore, this point estimate drops almost to zero when linear time trends at the Census-block-group level are added to the specification.[40]

The last row of the lower panels of Tables 6 and 7 presents our estimates of the total effect of the high-rise demolitions on the various crime types. On net, our estimates indicate that the program had large impacts; it reduced the city's homicide, shots fired, and vice and prostitution counts by about 5-10% of 1999 crime levels.

[40]See Section C of the online appendix for details.

The program had more modest effects on assault and battery, theft, vandalism, and disturbance: The reductions in these categories range from about 1-4% of 1999 crime levels. Finally, the changes associated with drug activity, burglary, auto theft, and trespassing were small and not statistically significant.

6.2 Robustness

The online appendix presents a wide array of robustness checks. We break these checks into two sections. The first section presents a set of specifications which serve to motivate our preferred specification, presented in the previous subsection. We focus on homicide as the dependent variable in the first part of the robustness checks for two reasons. The first is that it is the crime that is most accurately reported. The second is that it is the only crime for which we have a longer sample available, stretching back to 1970 at the tract-level and 1991 at the block-level. Thus, it covers the whole high-rise public housing closure and demolition period, which began in the early-to-mid 1990s.

We present robustness specifications showing the benefits of using fine geographical divisions of crime data (blocks, rather than tracts or block groups) and showing that using the log of distance to the nearest high-rise block works well to capture the decay of the effects of the high-rise closures on crime as distance increases. We also show that the results are robust to clustering the standard errors in a number of different ways. We demonstrate that the results do not change much when the sample is limited by dropping various public housing developments and when it is limited to the period from 1999 through 2011. The last restriction is important, as it is the only period for which we have the CCP and can measure the relocation of households displaced by the public housing closures. It is also the only period for which we have measures of crime other than homicide.

We show that the results are robust to estimation with a count data model rather than OLS. Finally, we present a figure showing event study coefficients, which reveal that the drop in homicide occurs in the closure year and persists, and a placebo test showing no estimated effect when closure years are randomly re-assigned across high-rise blocks.

The second part of the robustness checks in the online appendix serve to allay concern regarding three potential sources of bias in our displacement results: 1)

Displaced public housing households selecting their new location based on pre-existing trends in crime or its correlates. 2) Measurement error due to the fact that our measure of displaced public housing households is from a 5% sample rather than the entire population. 3) Endogenous responses in policing intensity motivated by the public housing demolitions.

7 Discussion

We estimate the effects of the closure and demolition of almost all of Chicago's high-rise public housing stock on crime in and near the Census blocks where the buildings were located and in and near the Census blocks where households displaced by the closures moved. Overall, reductions in violent crime in and near the areas where high-rises were demolished greatly outweighed increases in violent crime associated with the arrival of displaced residents in and near their new neighborhoods.

Similar to our results, Sandler (2012) finds larger decreases in violent crime associated with high-rise closures than in property crime. Also consistent with our findings, the effects that she measures dissipate as distance to the demolition site increases. One major difference between her study and ours is that she focuses solely on the local effects of high-rise closures on crime.[41]

Although it is hard to directly compare our results with Popkin et al. (2012), they also find evidence of crime increasing with the arrival of displaced public housing residents. Furthermore, they find that decreases in violent crime near demolitions outweighed increases due to displacement, implying a net reduction in violent crime. Also in line with our results, they find very small effects on property crime. The key distinction between our study and theirs is that our data allow us to follow households that left the public housing system or relocated to low-rises as well as those that obtained private market vouchers, whereas their data only follow the voucher holders. Since voucher users are likely to be a selected segment of the population and only comprise 35% of the displaced households, it is difficult to relate estimates from the sub-population of voucher users to the overall effect of displaced public housing

[41] Our results are also broadly consistent with the findings of an older version of this paper in which the analysis was conducted at the more aggregated community area level. See Hartley (2010) and Hartley (2009). In a somewhat different context, Plerhoples (2013) investigates the relationship between vacant housing demolition and crime.

households on crime.[42] Furthermore, our empirical strategy allows us to construct confidence intervals on the aggregate impact of demolitions on city-wide crime.

Our findings are consistent with those of Kling et al. (2005) who find that children in households that utilize randomly assigned private market vouchers and move from public housing to less poor neighborhoods show decreases in the probability of committing violent crime but show mixed results for property crime. Our results are also consistent with results described in Heller et al. (2011), in which households that win a lottery for a housing choice voucher while living in public housing show similar decreases in violent crime. While the previous two examples suggest partial equilibrium effects of lowering the poverty level to which an individual is exposed, we think that our results suggest it may be possible to achieve a city-level equilibrium with less violent crime by structuring public housing policy in such a way as to not encourage concentrations of extremely high poverty, as seen in Chicago prior to high-rise demolitions.

There are a number of possible mechanisms that could explain why de-concentration of public housing could lead to a decrease in crime: 1) Changing neighborhoods could provide better access to employment, thus raising market wages, making crime relatively less attractive. 2) Public housing demolition could decrease city-wide crime if the physical structure of high-rise public housing buildings provides a unique environment, which is hard to police and thus particularly suited to gang activity.[43] 3) The expected returns to criminal activity might be increasing functions of the spatial density of criminal human capital. If this were the case, then dispersing concentrated poverty could potentially lower aggregate crime.[44] This pattern could arise if there are peer effects in crime and changes in social networks due to relocation affect the mix of peers.[45] 4) Public housing demolition could lead to a reduction in city-wide

[42]A HUD Revitalization Grant Program Report for Chicago dated 12/31/2011 shows 35% of relocated households were using vouchers, 57% were in public housing, and the remaining 8% had left the system.

[43]This explanation is related to the theory of defensible space as popularized by Newman (1972).

[44]This is not to say that such an outcome would necessarily be a Pareto improvement. If individuals have sorted based upon willingness to tolerate crime, then even small increases in crime in some areas could have large negative welfare implications. Furthermore, the welfare costs due to being forced to move may be high for the households who must relocate from public housing.

[45]Bayer et al. (2008) show evidence of criminal peer effects in juvenile correctional facilities in Florida. Case and Katz (1991) find that residence in a neighborhood where peers are involved in crime increases the probability that an individual is involved in crime. Freeman et al. (1996) and Glaeser et al. (1996) consider models with social interactions and crime.

crime if dispersing subsidized housing more evenly throughout the city results in fewer areas where formal or informal social controls break down.[46] It is also possible that the CHA was particularly ineffective at managing the high-rise buildings, leading to an environment that was not conducive to preserving these informal social controls.

Our results are consistent with all four potential mechanisms, and could be the result of a combination of several of them. However, we find the last two mechanisms more plausible than the first two. The first possibility is that by changing neighborhoods, residents gained better access to employment opportunities, resulting in an increase in market wages, lowering the propensity for crime. We doubt this is the primary mechanism. Table 4 shows that displaced public housing households moved to places that, while not as bad as the high-rise blocks, were still quite economically disadvantaged compared to the city as a whole. Furthermore, the blocks that they moved to were on average twice as far away from downtown Chicago (and its array if potential employment opportunities) as the blocks they left.

A second possibility is that moving from high-rise to low-rise public housing and private market rental buildings that are typically no more than three stories tall resulted in fewer opportunities to commit crime and escape detection. Households moved to places that are easier to police, and thus the expectation of being caught when committing a crime increased. We do not think that this is likely to have been the primary mechanism either. Many clusters of high-rise buildings exist without high crime. However, it is possible that this may have interacted with and amplified the effects of the next two mechanisms.

The third and fourth possibilities relate to the decrease in the population exposed to extremely concentrated poverty. One possibility is that this led to fewer places where social norms had broken down. Another possibility is that this led to fewer chances for peer effects in criminal human capital to produce economies of agglomeration in crime production. Because the demolition program led to the removal of pockets of such extreme poverty (illustrated by the histograms in Figure 2), we find these last two mechanisms to be the most plausible for explaining how the closure and demolition of high-rise public housing could lead to a reduction in violent crime.

[46] Geographic concentration of poverty may lead to breakdowns in informal social controls, exacerbating crime. See Morenoff et al. (2001), Sampson and Raudenbush (1999), and Skogan (1990).

8 Conclusion

We find that high-rise public housing closures are associated with large local reductions in a set of crime measures including homicides, shots fired, assault and battery, drug activity, vice and prostitution, vandalism, and trespassing. There is evidence that the arrival of households displaced from closed high-rise public housing buildings is associated with increases in the Census blocks to which they move in a few of these crime measures: assault and battery, gang activity, burglary, trespassing, and disturbance. Increases in assault and battery and burglary that spill over into areas within a half-mile of blocks to which displaced households moved are also detectable.

The net effects for the city as a whole are large reductions in homicide, shots fired, vice and prostitution, and smaller reductions in assault and battery, theft, vandalism, and disturbance. The total effect on drug activity, burglary, auto theft, and trespassing is about zero.

Our results suggest that it may be possible to achieve a city-level equilibrium with less violent crime by structuring public housing policy in a way that does not result in concentrations of extremely high poverty.

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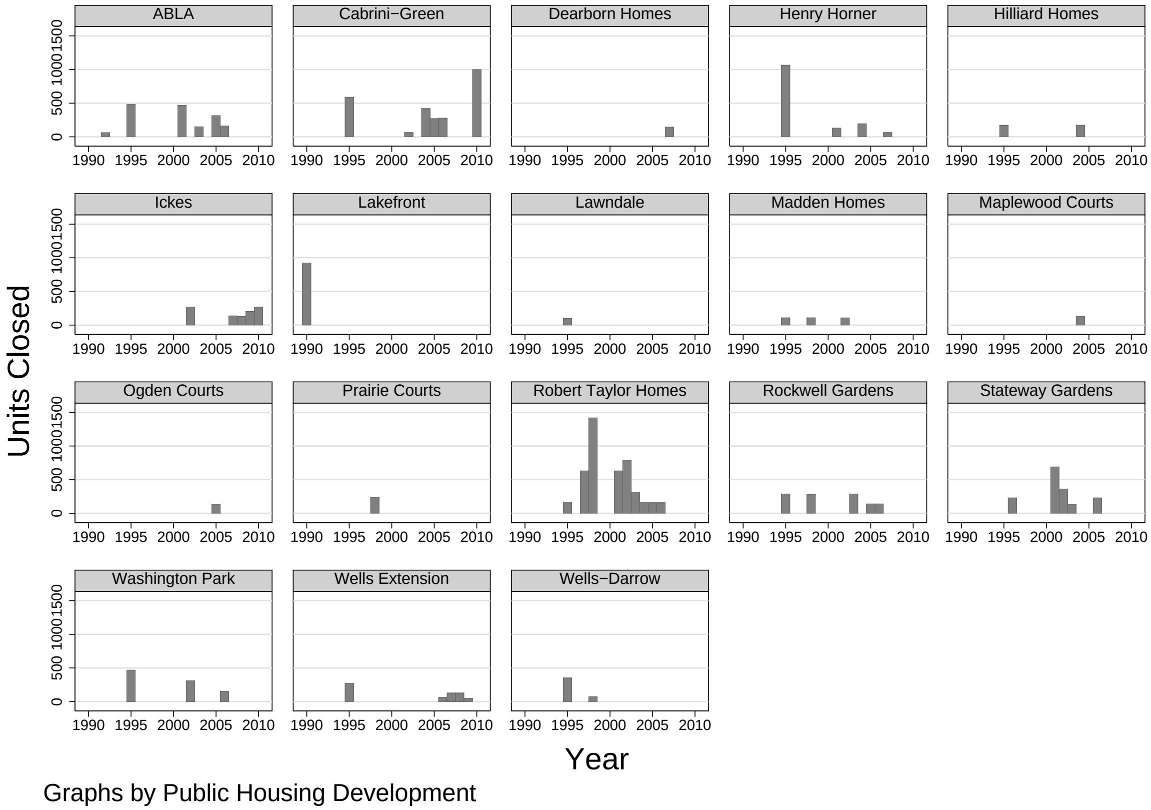

Figure 1: Yearly Number of Units Closed prior to Demolition in High-rise Buildings by Public Housing Development.

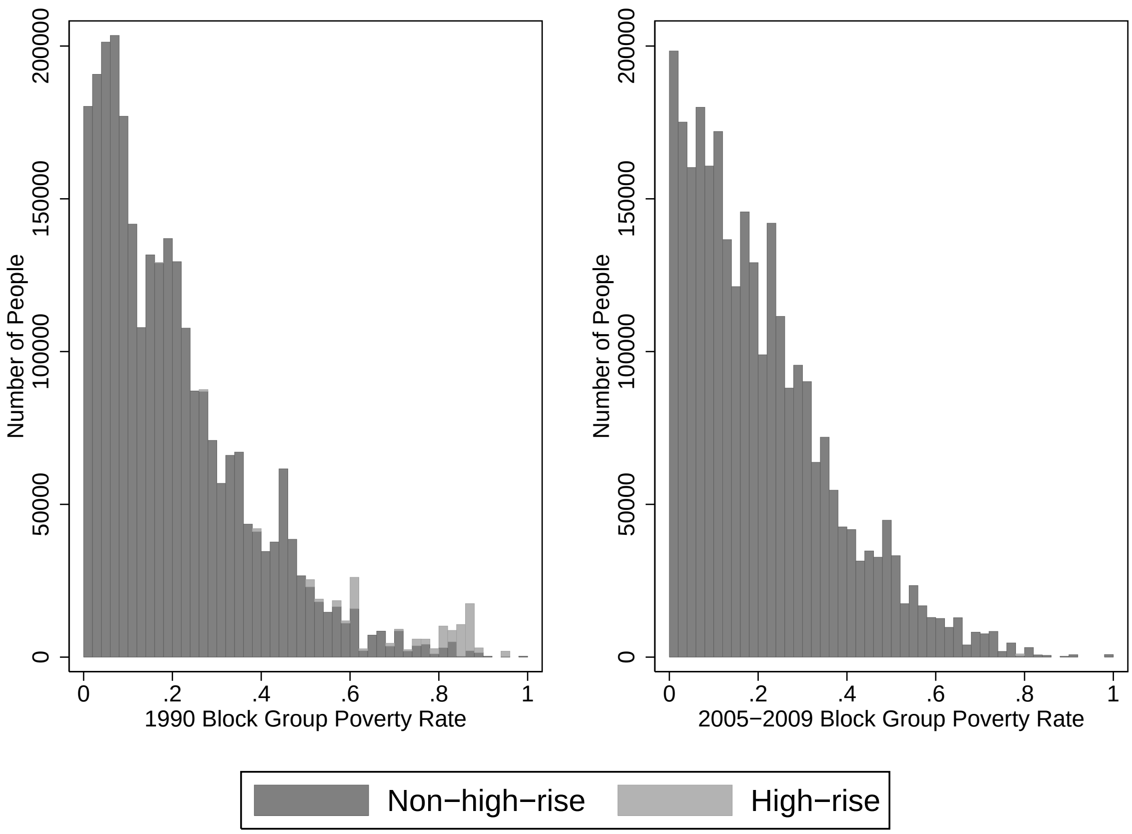

Figure 2: Population-weighted Distribution of Chicago Census Block Group Poverty Rates in 1990 and 2005-2009. Block groups containing high-rise public housing are indicated in light gray, all other block groups are dark gray. Each bin in a histogram indicates the number of people in the City of Chicago that are exposed to a block group poverty rate in the range covered by the bin in either 1990 or 2005-2009.

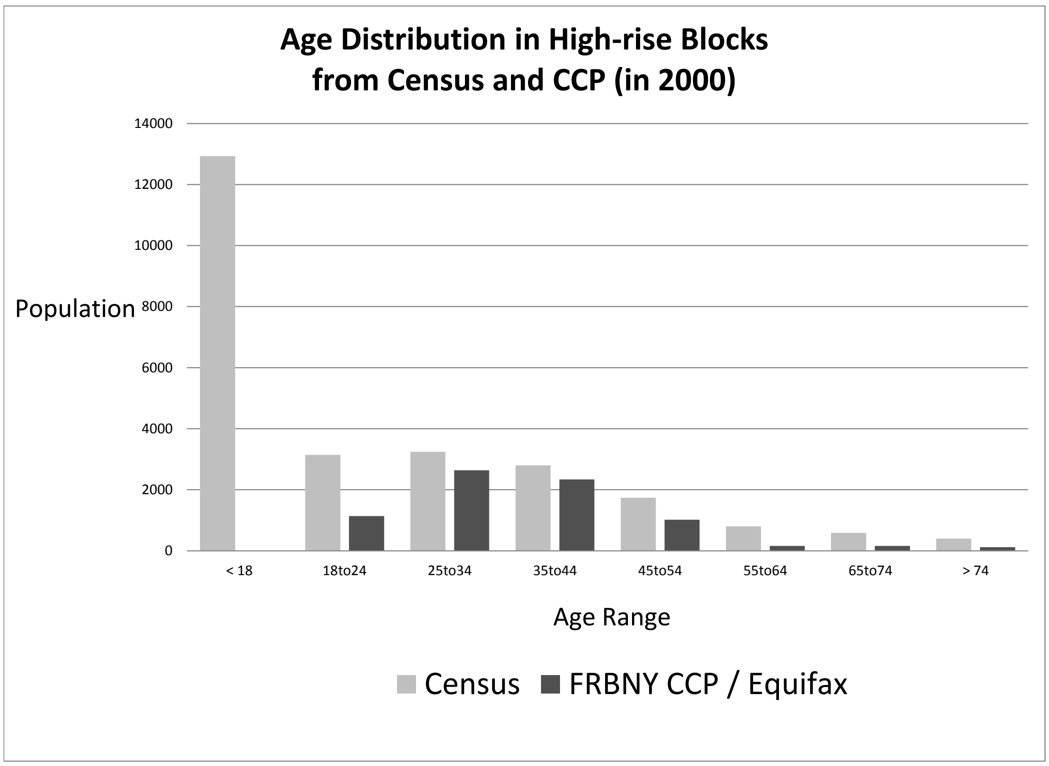

Figure 3: CCP Coverage Compared to 2000 Census Population in High-rise Blocks.

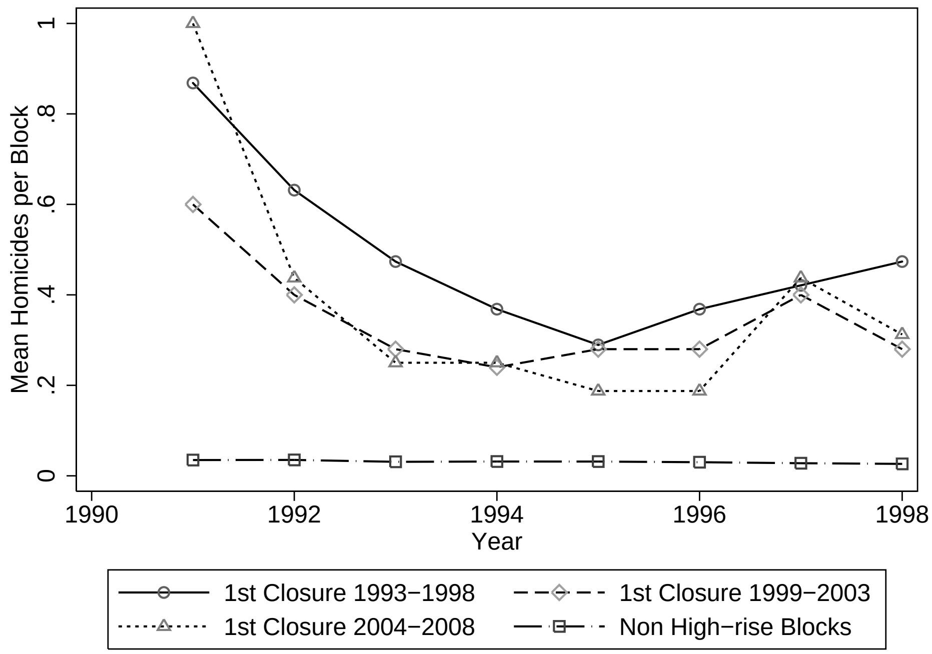

Figure 4: Trends in Homicides per Block Prior to Demolitions.

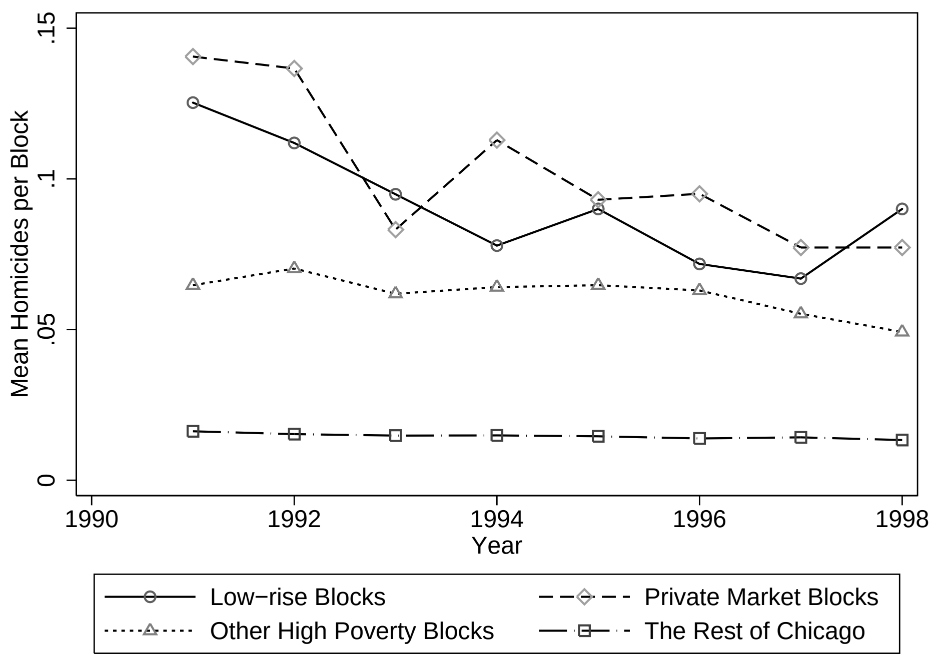

Figure 5: Trends in Homicides per Block Prior to Arrival of Displaced Households.

Table 1: Mean Demographic Characteristics for High-rise Areas and All Chicago Areas

Sample	High-rise	High-rise	High-rise	Chicago	Chicago	Chicago
Geography	Block Group	Block Group	Block	Block Group	Block Group	Block
Observations	38	38	90	2,477	2,477	24,639
Year	1990	2000	2000	1990	2000	2000
Population	1,861	1,223	302	1,134	1,176	118
Housing Units	758	583	142	463	468	47
Vacancy Rate	25.3%	30.3%	35.9%	9.5%	8.8%	8.3%
Owner-Occupancy Rate	6.0%	9.0%	7.4%	46.4%	48.0%	54.9%
Share African American	93.0%	91.2%	95.5%	41.9%	43.4%	40.8%
Share Hispanic	4.2%	4.8%	2.7%	17.3%	22.3%	21.4%
Poverty Rate	72.4%	53.9%		21.3%	16.8%	
Median Home Value	$78,506	$107,805		$143,051	$188,605	
Median Rent	$243	$303		$618	$664	
Median Household Income	$13,444	$23,503		$44,314	$49,873	

Note: All dollar-denominated numbers are in real 2010 dollars. High-rise mean block population density in 2000 is 36,981 people per square mile. Chicago mean block population density in 2000 is 16,571 people per square mile. High-rise block mean distance to Central Business District (CBD) is 3.2 miles. Chicago block mean distance to CBD is 7.5 miles.

Table 2: Mean Crime Counts for High-rise Blocks and All Chicago Blocks

Sample	High-rise	High-rise	Chicago	Chicago
Observations	90	90	24,661	24,661
Year	1999	2011	1999	2011
Homicide	0.27	0.04	0.02	0.02
Shots Fired	38.84	4.09	3.08	3.64
Assault and Battery	134.74	24.08	12.13	14.59
Gang Activity	5.18	1.16	1.34	1.99
Drugs	24.00	5.09	3.65	3.56
Vice and Prostitution	23.74	2.56	2.13	1.42
Burglary	12.07	2.41	2.42	2.40
Theft	17.06	4.96	4.38	3.99
Auto Theft	4.81	1.83	1.72	1.39
Vandalism	14.94	2.83	2.56	2.45
Trespassing	7.61	1.60	0.61	1.01
Disturbance	70.22	18.23	15.33	13.43

Note: Table shows mean crime counts in high-rise Census blocks (left panel) and for all city of Chicago Census blocks (right panel).

Table 3: Lack of Relationship between CCP Coverage Rate and Crime Rate in High-Rise Blocks in 2000

	CCP Coverage	R^2
Homicide	0.0017	0.097
	(0.0011)	
Shots Fired	0.95	0.073
	(0.86)	
Assault and Battery	1.06	0.083
	(0.88)	
Gang Activity	0.059	0.101
	(0.042)	
Drugs	0.25	0.078
	(0.21)	
Vice and Prostitution	0.94	0.075
	(0.84)	
Burglary	0.14	0.085
	(0.11)	
Theft	0.047	0.021
	(0.026)	
Auto Theft	0.016	0.116
	(0.008)	
Vandalism	0.16	0.081
	(0.14)	
Trespassing	0.14	0.089
	(0.11)	
Disturbance	0.62	0.080
	(0.52)	

Note: Each row presents a univariate regression of a per capita crime measure on the block-level credit panel coverage rate in 2000. N = 53. These are the 53 blocks that still had high-rise public housing buildings in 2000 and had positive population in the 2000 Census. The mean of the CCP Coverage variable for these 53 blocks is 47%, the 25th percentile is 30%, and the 75th percentile is 62%.

Table 4: Mean Demographic Characteristics of Blocks Sending and Receiving Displaced Households

	Relocation Blocks - in Block Groups with No Public Housing	Relocation Blocks - in Block Groups with Low-rise Public Housing	High-rise Blocks
Observations	393	168	90
Housing Units	82	88	142
Population Density	23,736	25,447	36,981
Vacancy Rate	13.1%	14.5%	35.9%
Owner-Occupancy Rate	37.6%	27.2%	7.4%
Share African American	89.7%	86.7%	95.5%
Share Hispanic	5.3%	9.7%	2.7%
Distance to CBD (miles)	7.35	6.24	3.22
Median Rent	$609	$510	$239
Median Household Income	$37.3K	$31.1K	$16.1K
Poverty Rate	27.0%	34.1%	62.2%

Note: The table reports means from 2000 Census. All dollar-denominated numbers are in real 2010 dollars. Median rent, median household income, and poverty rate are measured at the block group level.

Table 5: Total Effect of Public Housing Demolition on Homicide

	Coefficient	Total Implied Change in Homicides per Year
High-rise Units (in 100s)		
β_D: $H_{i,t}$	0.1483	-15.1
	(0.0359)	(3.7)
Miles to Nearest High-rise * High-rise Units (in 100s)		
β_{S1}: $ln(d(i,i_1)) * H_{i_1,t} * \mathbf{1}\{0 < d(i,i_1) < 0.5\}$	-0.0047	-14.7
	(0.0013)	(4.3)
Miles to 2nd Nearest High-rise * High-rise Units (in 100s)		
β_{S2}: $ln(d(i,i_2)) * H_{i_2,t} * \mathbf{1}\{0 < d(i,i_2) < 0.5\}$	-0.0052	-15.6
	(0.0022)	(6.6)
Miles to 3rd Nearest High-rise * High-rise Units (in 100s)		
β_{S3}: $ln(d(i,i_3)) * H_{i_3,t} * \mathbf{1}\{0 < d(i,i_3) < 0.5\}$	0.0002	0.4
	(0.0028)	(6.8)
Displaced Households in non-Public Housing Blocks		
β_{M1}: $M_{i,t} * \mathbf{1}\{\text{no ph in nbd } i\}$	-0.0080	-1.5
	(0.0099)	(1.9)
Displaced Households in Low-rise Public Housing Blocks		
β_{M2}: $M_{i,t} * \mathbf{1}\{\text{low-rise ph in nbd } i\}$	-0.0094	-1.1
	(0.0065)	(0.7)
Miles to Nearest Displaced Household * Displaced Households		
β_{M3}: $\ln(d(i,j_1)) * M_{j_1,t} * \mathbf{1}\{0 < d(i,j_1) \leq 0.5\}$	0.0004	-4.3
	(0.0006)	(5.4)
CCP Population Proxy	0.0000	
	(0.0002)	
Count of Year Indicators	12	
Count of Block Fixed Effects	24,661	
N	320,593	

Note: This table presents results of OLS estimates of the number of homicides per Census block per year on the number of high-rise public housing units still open, log distance (measure in miles) to nearest high-rise block interacted with number of high-rise units still open in the block, similar variables for the second- and third-nearest high-rise blocks, the number of displaced households measured in the CCP that have relocated to a non-public housing block, the number of displaced households that have relocated to a low-rise public housing block, and the log distance (measure in miles) to the nearest non-high-rise block with displaced households interacted with the number of displaced households. The number of households in the blocks * year as measured by the CCP is included to control for changes in population. Block fixed effects and year dummies are also included as controls. The sample runs from 1999 through 2011. Observations are at the Census block * year level. All blocks in the city of Chicago are included. Standard errors clustered by community area are in parentheses. The rightmost column shows the total change in the number of homicides per year implied by the change in each explanatory variable from the beginning of the sample period to the end of the sample period.

Table 6: Total Effect of Public Housing Demolition on Violent and Other Crimes

	Homicide	Shots Fired	Assault and Battery	Gang Activity	Drugs	Vice and Prostitution
Affected Area						
Local Direct	-63.10%	-86.05%	-65.26%	-65.20%	-63.10%	-85.19%
$\overline{\Lambda}_{AA}^{D}$	(15.26%)	(10.66%)	(6.95%)	(8.98%)	(6.51%)	(3.35%)
Local Spillover	-39.32%	-20.37%	-14.26%	8.63%	14.51%	-27.56%
$\overline{\Lambda}_{AA}^{N}$	(10.57%)	(8.50%)	(4.12%)	(21.24%)	(10.45%)	(6.30%)
Displacement Direct	-6.38%	3.04%	6.31%	30.21%	0.30%	-2.47%
$\overline{\Delta}_{AA}^{D}$	(5.74%)	(1.97%)	(0.97%)	(13.44%)	(3.51%)	(2.39%)
Displacement Spillover	-0.95%	2.14%	2.56%	8.31%	0.04%	-3.26%
$\overline{\Delta}_{AA}^{N}$	(1.18%)	(1.98%)	(0.90%)	(4.84%)	(1.48%)	(0.98%)
City-wide						
Local Direct	-2.48%	-3.97%	-2.65%	-0.92%	-1.51%	-3.47%
$\overline{\Lambda}_{CW}^{D}$	(0.60%)	(0.49%)	(0.28%)	(0.13%)	(0.16%)	(0.14%)
Local Spillover	-4.89%	-2.54%	-1.71%	0.70%	1.13%	-3.69%
$\overline{\Lambda}_{CW}^{N}$	(1.33%)	(1.02%)	(0.49%)	(1.68%)	(0.80%)	(0.83%)
Displacement Direct	-0.43%	0.25%	0.45%	1.70%	0.26%	-0.13%
$\overline{\Delta}_{CW}^{D}$	(0.32%)	(0.16%)	(0.07%)	(0.63%)	(0.39%)	(0.19%)
Displacement Spillover	-0.71%	1.46%	1.63%	4.65%	0.03%	-2.17%
$\overline{\Delta}_{CW}^{N}$	(0.88%)	(1.35%)	(0.57%)	(2.71%)	(1.16%)	(0.65%)
Total	-8.50%	-4.79%	-2.28%	6.13%	-0.09%	-9.46%
	(1.92%)	(1.61%)	(0.71%)	(3.44%)	(1.89%)	(1.22%)

Note: This table presents results obtained from OLS regressions of the number of crimes per Census block per year on explanatory variables. Explanatory variables include the number of high-rise public housing units still open, log distance (in miles) to nearest the high-rise block interacted with the number of high-rise units still open in the block, similar variables for the second- and third-nearest high-rise blocks, the number of displaced households measure in the CCP that have relocated to any non-high-rise block, log distance to the nearest non-high-rise block with displaced households interacted with the number of displaced households. Block fixed effects and year dummies are included as controls. The number of households in the block * year as measured by the CCP is included to control for changes in population. Sample runs from 1999 through 2011. Observations are at the Census block * year level. All blocks in the City of Chicago are included. All specifications have 320,593 observations. Standard errors clustered by community area are shown in parentheses.

Table 7: Total Effect of Public Housing Demolition on Property and Minor Crimes

	Burglary	Theft	Auto Theft	Vandalism	Trespassing	Disturbance
Affected Area						
Local Direct	-45.24%	-32.76%	-43.94%	-52.21%	-84.64%	-57.08%
$\overline{\Lambda}_{AA}^{D}$	(4.36%)	(5.10%)	(3.02%)	(7.02%)	(3.40%)	(2.38%)
Local Spillover	-12.72%	-7.44%	0.64%	-9.48%	-19.45%	-8.00%
$\overline{\Lambda}_{AA}^{N}$	(2.45%)	(1.82%)	(1.78%)	(3.32%)	(6.37%)	(2.63%)
Displacement Direct	5.60%	0.93%	-1.49%	1.00%	21.86%	2.46%
$\overline{\Delta}_{AA}^{D}$	(1.98%)	(0.74%)	(0.89%)	(0.73%)	(3.67%)	(1.05%)
Displacement Spillover	3.47%	0.73%	0.69%	0.47%	5.88%	0.14%
$\overline{\Delta}_{AA}^{N}$	(1.09%)	(0.44%)	(0.43%)	(0.42%)	(3.17%)	(0.36%)
City-wide						
Local Direct	-0.82%	-0.47%	-0.45%	-1.11%	-3.88%	-0.95%
$\overline{\Lambda}_{CW}^{D}$	(0.08%)	(0.07%)	(0.03%)	(0.15%)	(0.16%)	(0.04%)
Local Spillover	-1.41%	-1.02%	0.07%	-1.04%	-3.00%	-0.94%
$\overline{\Lambda}_{CW}^{N}$	(0.27%)	(0.25%)	(0.19%)	(0.37%)	(0.99%)	(0.32%)
Displacement Direct	0.33%	0.05%	-0.07%	0.06%	1.68%	0.19%
$\overline{\Delta}_{CW}^{D}$	(0.12%)	(0.03%)	(0.05%)	(0.04%)	(0.28%)	(0.06%)
Displacement Spillover	2.08%	0.41%	0.39%	0.26%	3.73%	0.08%
$\overline{\Delta}_{CW}^{N}$	(0.65%)	(0.25%)	(0.24%)	(0.24%)	(2.01%)	(0.21%)
Total	0.18%	-1.02%	-0.05%	-1.83%	-1.47%	-1.62%
	(0.71%)	(0.39%)	(0.31%)	(0.49%)	(2.67%)	(0.39%)

Note: See notes for Table 6.