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

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Abstract

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. Beginning in October 1992, the United States Department of Housing and Urban Development (HUD) began to award grants to local public housing authorities that could be used for demolition and revitalization of public housing through a program that has come to be known as HOPE VI.\(^1\) By 2003, HUD had awarded over $390M in demolition grants to local public housing authorities. In addition, HUD awarded more than $5.8B in revitalization grants from 1993 through 2006. Under HOPE VI, more than 50,000 units of distressed public housing will be demolished and about the same number of units will be developed. 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.”\(^2\) To that end, public housing in the United States is moving away from the old model of large developments including high-rise buildings and moving toward more low-rise, scattered-site, and mixed-income developments.

Chicago, which received a large share of HUD’s demolition grant money, began closing and demolishing high-rise public housing buildings in the mid-1990’s. The demolition program was formalized in the Chicago Housing Authority’s 1999 Plan for Transformation, which called for the demolition of virtually all non-senior-citizen high-rise public housing in Chicago. 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 went up where high-rise developments were demolished. This massive change to one of the largest public housing systems in the United States (third only to New York City and Puerto Rico in terms of numbers of units) has had a profound impact on the city. The impact has been felt by the public housing residents who have been displaced as well as in their former neighborhoods and in the neighborhoods to which they have relocated.

This paper evaluates the impact this massive demolition and relocation program had on crime throughout the city of Chicago. Economic theory does not provide a clear prediction about the expected impact of this program on crime. On one

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\(^1\)HOPE VI program information is available at: http://www.hud.gov/offices/pih/programs/ph/hope6/about/

\(^2\)Popkin et al. (2002)
hand, if there are peer effects in crime, then one might expect demolishing high-rise public housing to reduce city-wide crime by decreasing the density of poverty in neighborhoods where poverty has been most concentrated. By similar reasoning, public housing demolition could lead to a reduction in city-wide crime if dispersing subsidized housing more evenly throughout the city results in fewer areas where formal or informal social controls break down. One might also expect public housing demolition to 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. On the other hand, the level of city-wide crime could remain the same if crime is simply displaced from neighborhoods that are being revitalized to other neighborhoods. Finally, net crime might even be expected to increase if displaced residents have a hard time adapting to their new neighborhoods, rival gangs are pushed into each other’s territory, or police find it hard to adapt to new spatial patterns of criminal activity.

In Becker’s theory of criminal behavior (Becker (1968)) individuals weigh the expected benefits against the expected costs when deciding how much crime to commit.

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3Bayer et al. (2008a) show evidence of the existence of criminal peer effects in juvenile correctional facilities in Florida. Case and Katz (1991) find evidence that residence in a neighborhood where peers are involved in crime increases the probability that an individual is involved in crime. Glaeser et al. (1996) analyze a social interaction model with multiple equilibria to explain spatial variation in crime rates. Freeman et al. (1996) propose a different model, also featuring multiple equilibria, in which spatial variation in crime rates is driven by the assumption that a criminal’s chance of being caught is a decreasing function of the number of other criminals operating in the same area. Card et al. (2008) document evidence of thresholds in the minority share of a neighborhood beyond which the neighborhood may move toward an equilibrium with either 0% or 100% minority share. These non-linearities are consistent with the existence of multiple equilibria predicted by social interaction models.

4Geographic concentration of poor households may lead to breakdowns in informal social controls, exacerbating crime problems. See Skogan (1990). Sampson and Raudenbush (1999) and Morenoff et al. (2001) find that the degree of “collective efficacy”, social control of public space, is associated with the level of violent crime. Hirschfield and Bowers (1997) study the connection between crime and “social cohesion”. See Sampson et al. (2002) for an overview of “neighborhood effects” in the sociology literature. Joseph et al. (2007) discuss the theoretical justifications for mixed-income development as a means to address poverty.

5This explanation is related to the theory of defensible space as popularized by Newman (1972).

6Evidence of crime displacement due to weather shocks has been shown by Jacob et al. (2007).

7Kling et al. (2005) find that male youths randomly selected to relocate to lower-poverty areas have lower probabilities of arrest for violent crime, but higher probabilities of arrest for property crime. Hagedorn and Rauch (2007) suggest that the fall in Chicago’s homicide rate may have been delayed by conflicts created when gang members displaced by public housing closures relocated to rival gang territory. Rosin (2008) argues that police were not prepared for the new spatial distribution of crime caused by public housing demolition and resident relocation.
In this case the costs depend on the probability of being punished and the opportunity cost of punishment (e.g. lost wages). One prediction of this model is that the propensity for criminal activity should be higher for individuals that face lower market wages. If this were that only factor at play, then one would not expect a change in aggregate crime levels in response to a policy which removed pockets of concentrated poverty by dispersing the poor more evenly throughout the city. Crime would simply move from some areas to others. In contrast, if the expected costs and benefits depend on characteristics of an individual’s neighborhood, then this policy could have an effect on aggregate crime levels as well as the spatial distribution of crime. Changing neighborhoods might provide access to employment, thus changing market wages. Changing neighborhoods could also alter the set of opportunities for criminals. One way of interpreting the peer effects discussed in the previous paragraph is as economies of agglomeration in criminal human capital. Specifically 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.\footnote{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.} Although we are not able to definitively distinguish among these theoretical mechanisms, we believe our results are most inline with the presence of agglomeration economies in criminal human capital.

We split our analysis into two parts. In the first part, we estimate the effect of high-rise public housing closures and subsequent demolition on the geographic areas (defined as census tracts, block groups, or blocks) in which the high-rises were located and on the geographic areas that were within a certain distance of the high-rises (our preferred specification uses 0.5 miles). We exploit variation in the timing of the building closures throughout the course of the sample period to identify how changes in the number of high-rises impacts the level of crime. We include geographic fixed effects to control for persistent differences in crime across places in the city. We also include time dummies to control for secular trends in crime in our sample, which in some specifications is the entire city, and in others is just the areas that contained high-rise buildings. If each geographic unit had at most one high-rise closure, then our specification would be an event study.\footnote{See Jacobson et al. (1993)} 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, geographic 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.

There are several potential reasons why one would want to be cautious in interpreting our estimates as the causal effect of high-rise closure on crime. One possibility is that rather than randomly selecting high-rises for closure, public housing officials chose buildings that had particularly high crime rates. There are several reasons why we do not think that this type of endogeneity bias is problematic. The first is that we do not find a correlation between crime and the order of building closures. The second is that the demolition program included the entire stock of non-senior high-rise public housing buildings.\textsuperscript{10} Another reason is that estimates of the effect of demolition on crime do not appear to be sensitive to the sample period used. In particular we find similar estimates of the effect of high-rises on homicide (the best measured crime category) when we use a sample covering the whole demolition period (1991-2011) as we find with a sample that just covers the Plan for Transformation period (1999-2011). 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. Another worry is that there may be other factors that influence both crime and public housing demolitions. An example might be the vacancy rate of a high-rise building. A building with a high number of vacant units might be especially prone to crime and also may be easier to close than a more heavily occupied building. However, we find no relationship between occupancy rates and crime, and our estimates do not vary systematically with occupancy rate. Another potential unobserved factor could be gentrification. There may be more pressure on the city to demolish public housing in neighborhoods that are gentrifying than in neighborhoods that will remain low-income, and gentrification may bring lower crime rates at the same time. Importantly, our estimates are robust to excluding the development with the highest

\textsuperscript{10}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.
land value (Cabrini-Green). Again, the fact that all high-rise public housing buildings were slated for closure and demolition during this period means that housing authorities were not able to select only the least occupied buildings for closure or only the buildings in neighborhoods that were gentrifying. In fact, 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.\textsuperscript{11} We provide corroborating 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 the prevalence of homicide in the early 1990’s and the order in which buildings were closed.

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 33\% to 86\% drops 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 block, but the reductions are smaller in magnitude, ranging from 7\% to 40\% of 1999 crime levels in this set of blocks. The exceptions are drug activity, gang activity, and auto theft, which do not show clear signs of changing in response to high-rise closures.

These results are comparable with Popkin et al. (2012); who attribute a 60\% decrease in violent crime and a 49\% drop in property crime to public housing demolition in tracts where public housing was demolished. 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.\textsuperscript{12}

In the second part of our analysis, we focus on estimating the effect that displaced public housing households have on crime in the census blocks to which they move, and the census blocks within a half mile of where they move. Once again we use variation in the timing of the arrival (and possible departure) of households that were displaced


\textsuperscript{12}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.
when high-rise public housing buildings were closed. We use credit report history data to track households that had been living in census blocks where public housing closed and subsequently moved. We estimate the effect of displaced public housing households on crime in the census block to which they move and the nearby census blocks with a specification that is very similar to the one we use in the first part of our analysis. Again, we use block fixed effects to control for persistent differences in crime across different areas of the city. We also include time dummies to control for secular trends in crime. 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 of crime in the block before receiving a displaced household. Also, if the household moved out of the block before the end of the sample period, those remaining periods would also count toward the pre-displaced household average. 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.

As with the public housing closures, there are also several potential reasons why one would want to be cautious in interpreting our estimates as the causal effect of displaced public housing households on crime. In this case, households may select neighborhoods based on pre-existing trends that influence both their likelihood of locating there and the level of crime. Also, pre-existing trends in the level of crime could directly influence the likelihood of receiving displaced high-rise households. One way this could happen is if crime were increasing in the receiving neighborhood, rents might fall in response, making it more feasible for displaced households to afford to move there with vouchers (which have a cap that is binding in many Chicago neighborhoods). This particular scenario would cause us to falsely attribute rising crime levels to displaced high-rise households. Other plausible scenarios could lead to bias in the other direction. Either way, selection of neighborhoods by displaced households based on pre-existing neighborhood trends poses a serious threat to identification of the causal effect of displaced households on neighborhood crime.

To assess the degree to which this type of selection may bias our results, we estimate an alternative specification motivated by Bayer et al. (2008b). The identifying assumption in this alternative specification is that the housing market is very thin at the census block level. Thus, while a displaced household may be able to pick the
block group to which they relocate, the particular block within the block group where a unit is available is random. We show that observable block-level variables (aside from the number of housing units in the block) are not predictive of which block within a block group receives displaced high-rise households. We find that the results of the alternative specification are similar to those of our main specification, implying that selection on unobserved neighborhood attributes or trends in those attributes is not likely to be a source of bias in our estimates.

However, there are still reasons for caution in interpreting the displacement effects in a causal manner. The reasons are due to measurement issues related to the coverage and sampling in the credit report data set. Allaying these concerns is the fact that we find a lack of correlation between coverage rates in our sample and crime. Although all of our specification tests lend themselves to supporting a causal interpretation of the displacement estimates, we nevertheless caution that the causal interpretation of the displacement estimates is less definitive than the causal interpretation of the local estimates.

For about half of the crime types, we find an increase in crime associated with the arrival of displaced high-rise households in the census blocks to which they relocate. These effects range from about 3% to 30% 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. But these increases represent only 2% to 3% 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, disturbance, and truancy or curfew crimes attributed to public housing closure. The one category of crime that showed a possible increase at the city level due to closures was gang activity.

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. One 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.

2 Background on Public Housing, HOPE VI, and Chicago’s Plan for Transformation

2.1 A Brief History of Public Housing

In the United States, federally provided public housing dates back to 1918, when 16,000 units were built for workers during World War I. The passage of the 1937 National Housing Act established the current system of local, independent housing authorities that receive federal money and perform the tasks of building and managing public housing. Under this program, and continuing through World War II, the federal government financed the construction of 365,000 permanent housing units and an even greater number of temporary units. As World War II veterans returned, and African-American migration from the rural south to northern cities continued, urban housing was in short supply. In 1949, a new Housing Act was passed, providing loans and subsidies for the construction of about 810,000 units of low-rent housing.\(^{13}\) While the pace of building and the uptake rate of federal funds varied from city to city, a large number of federally subsidized, low-rent housing units were built over the next fifteen years. However, from the mid-1970’s through the early 1990’s, conditions in public housing deteriorated significantly. Problems associated with public housing included high crime and low educational and employment outcomes of residents. Furthermore, much of the stock of public housing was in disrepair. Funding had been cut during the 1980’s, resulting in deferred maintenance, and contributing to the large and growing costs of rehabilitation.\(^{14}\)

In Chicago, site selection for new public housing units to be constructed during the 1950’s was a contentious issue.\(^{15}\) The CHA initially proposed some sites on vacant land in outlying neighborhoods that were predominantly white and other sites in

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\(^{13}\)Meyerson and Banfield (1955).

\(^{14}\)Polikoff (2006).

\(^{15}\)Hunt (2009) provides an excellent history of public housing in Chicago from the 1937 Housing Act through the CHA’s Plan for Transformation.
poor African-American neighborhoods closer to the center of the city, which were not vacant but were deemed to be “blighted slums”. This classification was meant to indicate areas where housing was not structurally sound and living conditions were deemed to be unsanitary. Many of the city council members whose wards contained the sites that were proposed in the outlying areas organized an opposition which threatened to derail the entire plan of building up to 40,000 new units of housing over a six-year period. In the end, the CHA was denied the use of most of the vacant land sites. Construction of public housing that took place from 1950 to 1964 was either as an extension of an existing development, or on a site that was in a poor African-American neighborhood. The public housing buildings built in Chicago during this time were almost all high-rises.16

2.2 The HOPE VI Program and Chicago’s Plan for Transformation

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 which stood for “Housing Opportunities for People Everywhere”).17

During the period from 1993 through 2006, the HOPE VI program awarded the CHA $258M in revitalization grants representing 4.4% of the total $5.8B awarded to local housing agencies. Of the 127 housing authorities that were awarded HOPE VI demolition grants from 1996 through 2003, the CHA received $83.4M of grant money for the demolition of 12,500 units of public housing, representing about 21% of the total HOPE VI demolition grants awarded in terms of dollars or numbers of units.

The scope of the HOPE VI program was broadened when, in 1996, the United States Congress passed the Omnibus Consolidated Rescissions and Appropriations

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Act. Section 202 of this law 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 Chicago’s gallery-style high-rise public housing developments had failed the viability test and were slated for demolition. In February 2000, the Chicago Housing Authority’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.  

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.”19 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.20 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.21 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.

18 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.  
21See Polikoff (2006)
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.  

3 Local Effects

3.1 Identification Strategy

Our goal is to determine the average change in crime due to public housing demolitions during the CHA’s Plan for Transformation, both in the areas near the demolished buildings and in the areas to which displaced residents moved. A key obstacle to achieving this goal is identifying the correct counterfactual level of crime had public housing not been demolished. The problem is that the observed correlation between public housing demolition and crime reduction does not necessarily indicate that public housing demolition is causing crime to fall. Levitt (2004) identifies increases in the number of police, the rising prison population, the receding crack epidemic, and the lagged effect of the legalization of abortion in 1973 as factors contributing to the nationwide decline in crime during the 1990’s. All of these factors may have contributed to the decrease in crime in Chicago during the period in which public housing was being demolished, and could potentially contribute to a spurious correlation between public housing demolition and decreases in crime. In addition, it is likely that other, unobserved factors also had an influence on both the level and distribution of crime in Chicago during this period.

3.1.1 Why We Do Not Exploit Cross-Sectional Variation in Building Closures

One standard approach to identifying counterfactual levels of crime would be to compare the change in crime in neighborhoods where public housing was demolished with the contemporaneous change in crime in similar neighborhoods where public housing was not demolished. However, employing such a difference-in-differences estimator is

\[^{22}\text{See CHA Annual Reports 2000 - 2010.}\]
unlikely to be a valid identification strategy because the neighborhoods where high-rise public housing units were built are not comparable to other neighborhoods in Chicago. This problem is rooted in the historical process by which original sites were selected for high-rise public housing in the 1950’s and 1960’s. The site selection process was extremely contentious. In the end, high-rise public housing was built either as an extension to existing low-rise public housing developments or in low-income African-American neighborhoods which had been designated as “blighted”. This meant that almost all high-rise public housing in Chicago was built in the neighborhoods closest to the downtown central business district or along a contiguous stretch of land extending directly south of downtown (see Figure 1).

One hypothetical solution for implementing a difference-in-differences estimator would be to use as controls neighborhoods where high-rise public housing sites were proposed but rejected. However, even this use of cross-sectional identifying variation is problematic, as the sites that were rejected had quite different characteristics than the sites that were eventually built upon. The sites rejected by both the CHA and the City Council were located in neighborhoods that had relatively fewer African-Americans, had higher income, had lower population density, and were farther from the central business district than the sites where high-rise public housing was eventually built. Site selection was a politically-charged process. Initially the CHA investigated sites on the far North Side, far South Side, and in the center of the city. It rejected the far North Side sites based largely upon high land costs. The remaining sites were then presented to the Mayor and City Council, who eventually rejected the far South Side sites at the behest of the aldermen who represented these neighborhoods and who wanted to prevent African-Americans from moving in.

Another hypothetical solution for implementing a difference-in-differences estimator would be to use as controls neighborhoods that look similar at a more recent date, say 1990, to those in which there were public housing demolitions. Unfortunately, this implementation of a difference-in-differences estimator is also unlikely to be a valid identification strategy. From 1960 through 1990, the neighborhoods containing high-rise public housing remained relatively low-income and predominantly African-American. Selecting suitable neighborhoods for comparison based on 1990

\[^{23}\text{Bowly, Jr. (1978), Meyerson and Banfield (1955), and Polikoff (2006).}\]
\[^{24}\text{See online appendix. Meyerson and Banfield (1955) present a detailed case study of the site selection process and the political wrangling that was associated with it.}\]
characteristics is difficult, because all other neighborhoods that are situated as close to downtown as the high-rise neighborhoods tend to be predominantly White, Hispanic, or Asian, and higher income. There are other low-income African-American neighborhoods in Chicago, but they are much farther from downtown and a number of them contain low-rise family public housing. It is important to exclude the neighborhoods with low-rise public housing from any possible set of comparison neighborhoods because they have been affected by the Plan for Transformation in an unsystematic fashion. Some have been closed and demolished, while others have been filled with relocatees from the high-rises.

Attempting to address the differences between neighborhoods where high-rise public housing buildings were demolished and a set of comparison neighborhoods via statistical methods of re-weighting or matching (such as by the propensity score) is not feasible due to the lack of common support (Heckman et al. (1998)). The neighborhoods with high-rise public housing have quite different characteristics than those without high-rise public housing.\(^\text{25}\)

### 3.1.2 Estimators That Exploit the Timing of Building Closures

Due to these obstacles to using cross-sectional variation to identify the effects of interest, our proposed solution is to 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 in this identification strategy is that the timing of public housing demolitions is exogenous, which motivates our econometric specification. We present the estimated specifications in Sections 3.3 and 4.2, which are generalizations of the following specification:

\[
Y_{i,t} = \alpha_i + \gamma_t + \beta H_{i,t} + \epsilon_{i,t},
\]

where \(Y_{i,t}\) denotes an outcome in neighborhood \(i\) in year \(t\).\(^\text{26}\) \(H_{i,t}\) represents the number of housing units in neighborhood \(i\) at time \(t\). The \(\alpha_i\) are fixed-effects for

\(^{25}\)See online appendix.

\(^{26}\)For example, under one definition of neighborhood and using outcomes from one data set, \(Y_{i,t}\) could be the number of homicides in census tract \(i\) in 1999.
neighborhoods \( i \in \{1, \ldots, I\} \), and the \( \gamma_t \) are year-effects. Finally, \( \epsilon_{i,t} \) represents unobserved determinants of outcome \( Y_{i,t} \).

The \( \beta \) parameter is identified by variation in the timing and number of unit closures across the affected geographic areas. It is important to note that inclusion of neighborhood fixed-effects will absorb any unobservable characteristics of neighborhoods which are time-invariant. For example, these fixed-effects will control for persistent differences in population density between neighborhoods which may affect crime. Furthermore, the year-effects will absorb common transitory shocks to the set of neighborhoods in the sample. For example, aggregate changes and trends in crime will be controlled for.

Any time-varying omitted variables which affect the outcome variable and are correlated with the timing and number of closures but not absorbed by the year effects will cause ordinary least squares (OLS) estimates of the \( \beta \) parameter to be biased. A possible scenario in which OLS estimates may be biased is if the order of high-rise building closures were determined by the crime level in the neighborhood of the buildings in the years prior to the closure. If this were the case, then a serially correlated shock to crime in a particular neighborhood would cause crime to rise and might induce building closures. Building closures would occur at the same time that crime is falling back to its mean level; hence, the effect of public housing closures on lowering crime would be overstated. This problem is mitigated by the fact that almost all of the high-rise buildings in the family public housing developments are slated to be demolished eventually. There is anecdotal evidence that in a few cases buildings that were perceived to have high crime may have been closed for that reason in the earlier period of the sample.\(^\text{27}\) However, it is unclear that these were temporary spikes in crime prior to the closure. It is more likely that these buildings had experienced persistently high crime. Also, beginning in 2000, the Plan for Transformation laid out the timetable for the remaining demolitions, so there was less opportunity to selectively close buildings in response to crime shocks from 2000 on.

As one way of checking whether buildings tended to be selected for closure in response to elevated crime in prior years, we look for a correlation between crime rates prior to closures and the year that the high-rise closure occurred. Neither the

\(^{27}\)Jacob (2004) and Polikoff (2006) mention incidents such as the shooting of seven-year-old Dantrell Davis prior to demolitions at Cabrini-Green and demolition of three Robert Taylor buildings known as “the hole” due to entrenched gang problems.
average level of homicides in the 1990’s nor the number of violent crimes in 1999 are correlated with the year of the first high-rise closure at the census block-level.\footnote{See online appendix for figures.} 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.

3.2 Data and Descriptive Statistics

3.2.1 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 Chicago Housing Authority 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.\footnote{Brian Jacob is the Walter H. Annenberg Professor of Education Policy in the Gerald R. Ford School of Public Policy at the University of Michigan.} The third source is our compilation of high-rise public housing buildings closed prior to demolition from 2000 through 2011 taken from the Chicago Housing Authority’s annual plans and reports.

These datasets contain a total of 180 high-rise public housing addresses, which we define as addresses with 45 or more units. The first 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, and we restrict the analysis to the family housing buildings. We observe the year of closure for 161 of the 180 high-rise addresses during the period from 1990 to 2011. The remaining 19 addresses were not demolished.\footnote{Thirteen of these remaining addresses are in the Dearborn Homes development, which was originally slated for demolition, but was instead renovated beginning in 2009. The other 6 addresses are in fact not high-rise buildings, but smaller scattered site buildings which are still in use.} 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, 38 census block groups, or 31
census tracts. Figure 2 shows the annual number of high-rise building units closed by development. The data also contain occupancy rates for 156 of the high-rise buildings in 1990. The mean occupancy rate is 35% while the 25th and 75th percentile are 28% and 42%, respectively.

Table 1 shows that the demographic characteristics of areas with high-rises were quite exceptional when compared with other areas in the city or the country as a whole. The Table provides means of demographic characteristics for the areas containing high-rise public housing (upper panel) and for the city as a whole (lower 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.\textsuperscript{31}

The mean population in the high-rise tracts fell sharply from 2,929 in 1990 to 2,233 in 2000. This was accompanied by a reduction in the mean number of housing units from about 1,200 to about 1,000. Similarly, large reductions in population and housing units are evident when examining just the block groups where high-rises were located. In contrast, for the city as a whole, the mean population and mean number of housing units increase slightly from 1990 to 2000 at both the block group and tract level.

Housing unit vacancy rates (shown in column (7)) are also quite high in the high-rise areas. Vacancy rates were already 25 percent in high-rise block groups in 1990 and rose to 30 percent 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 percent in 2000, compared to only about 8 percent for the city as a whole.

Finally, high-rise tracts had extreme levels of important socioeconomic characteristics. For example, high-rise tracts had extreme poverty rates: The mean for these tracts in 1990 was above 70 percent, while the 99th percentile for the nation as a whole was 55 percent. The mean poverty rate had fallen to about 55 percent by 2000, yet this was still far higher than the mean for all tracts or block groups in the city.\textsuperscript{32} High-rise blocks also had much lower owner-occupancy rates, higher proportions of African-American residents, and lower proportions of Hispanic residents than the city as a whole. In both 1990 and 2000, the means of the median home value, median rent,

\textsuperscript{31}Block level tabulations for 1990 are limited and are unavailable from the NHGIS.
\textsuperscript{32}This was also still above the 99th percentile for the country as a whole, 50 percent.
and median household income in the high-rise blocks were about half or less than the means of the median home value, median rent, and median household income were for the city as a whole. It was also the case that the mean proportion of residents ages 25 or older that had a bachelor’s degree or higher in the high-rise tracts was less than half of the mean for the city as a whole.

3.2.2 Crime Data

We use crime data from two sources. The first is an extended version of Block and Block’s Homicides in Chicago that contains an annual count of homicides. This dataset has homicide counts at the tract level from 1970 to 2011 and at the block level from 1991 to 2011. 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.

Figure 3 shows both the number of units that have not yet been closed prior to demolition in high-rise public housing buildings (blue line, left axis) and the number of homicides per year in the whole city of Chicago (red line, right axis). The figure reveals that there were around 800 homicides per year in the city from 1970 through the mid-1990’s, when that number began to fall. During this period, the number of units in high-rise public housing buildings that had not yet been closed prior to demolition also fell sharply from about 19,000 units to about 1,000 units.

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.

The lower panel of Table 1 shows mean counts of calls for service by type of crime for 1999 and 2011 in the high-rise blocks (upper portion of lower panel) and in the city as a whole (lower portion of lower panel). The mean number of calls for service fell sharply for each type of crime in the high-rise blocks from 1999 to 2011. For most

33See http://www.icpsr.umich.edu/icpsrweb/content/NACJD/guides/hc.html for details.
types of crime, the mean numbers of calls-for-service were still higher in the high-rise blocks than in the city as a whole in 2011. The exceptions were burglaries, which were at about the same level, and calls regarding gang activity, which were actually lower in the high-rise blocks in 2011 than in the city as a whole.

3.3 Local Effects Model Specification

Suppose Chicago has \( I \) neighborhoods. For each neighborhood \( i \in \{1, \ldots, I\} \), define neighborhood \( i^* \in \{1, \ldots, i - 1, i + 1, \ldots I\} \) as the nearest neighborhood containing a public housing high-rise.\(^{34}\) We specify crime \( Y \) for neighborhood \( i \) at time \( t \) to be determined as follows:

\[
Y_{i,t} = \alpha_i + \gamma_t + \beta_D H_{i,t} + \beta_N H_{i^*,t} + \epsilon_{i,t}. \tag{2}
\]

Here \( H_{i,t} \) is the number of units of high-rise public housing that are still open in neighborhood \( i \) at time \( t \).\(^{35}\) 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. \( \alpha_i \) and \( \gamma_t \) are neighborhood \( i \) fixed-effects and year-effects, respectively. Finally, \( \epsilon_{i,t} \) represents unobserved determinants of outcome \( Y_{i,t} \).

We define \( d(i, i^*) \) as the distance from neighborhood \( i \) to neighborhood \( i^* \), measured in miles. Then \( \beta_N \) represents an effect from one of four distance bands as follows:

\[
\beta_N H_{i^*,t} \equiv \beta_{N1} 1\{0 < d(i, i^*) \leq 0.25\}H_{i^*,t} + \beta_{N2} 1\{0.25 < d(i, i^*) \leq 0.5\}H_{i^*,t} \\
+ \beta_{N3} 1\{0.5 < d(i, i^*) \leq 1.0\}H_{i^*,t} + \beta_{N4} 1\{1.0 < d(i, i^*) \leq 1.5\}H_{i^*,t}.
\]

Alternatively, define \( i_1 \in \{1, \ldots, i - 1, i + 1, \ldots I\} \) as the nearest neighborhood to \( i \) containing a public housing high-rise, \( i_2 \) in the same set as the second-nearest neighborhood, and \( i_3 \) as the third-nearest neighborhood containing a public housing

\(^{34}\)We define nearest as the smallest distance measured between the centroids of each neighborhood.

\(^{35}\)By “open”, we mean that they have not yet been closed prior to demolition.
high-rise. In this case we can re-specify Equation 2 as:

\[ Y_{i,t} = \alpha_i + \gamma_t + \zeta_D H_{i,t} + \zeta_N H_{i1-i3,t} + \epsilon_{i,t}, \]

(3)

where

\[ \zeta_N H_{i1-i3,t} \equiv \zeta_{N1} \{ 0 < d(i, i_1) < 0.5 \} \ln(d(i, i_1)) H_{i1,t} + \]

\[ \zeta_{N2} \{ 0 < d(i, i_2) < 0.5 \} \ln(d(i, i_2)) H_{i2,t} + \]

\[ \zeta_{N3} \{ 0 < d(i, i_3) < 0.5 \} \ln(d(i, i_3)) H_{i3,t}. \]

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 nearby effect using the parameters of our model as either:

\[ \Lambda_D \equiv \sum_{i=1}^{I} \beta_D (H_{i,T} - H_{i,1}) \]

\[ \Lambda_N \equiv \sum_{i=1}^{I} \beta_N (H_{i^*,T} - H_{i^*,1}) \]

or

\[ \Lambda_D \equiv \sum_{i=1}^{I} \zeta_D (H_{i,T} - H_{i,1}) \]

\[ \Lambda_N \equiv \sum_{i=1}^{I} \sum_{j=1}^{3} \zeta_{Nj} \{ 0 < d(i, i_j) < 0.5 \} \ln(d(i, i_j)) (H_{i_j,T} - H_{i_j,1}), \]

depending on the model estimated. These parameters are especially useful for comparing effects using parameter estimates from models estimated under different definitions of neighborhood (tract, block group, and block). In some instances it will also be useful to decompose \( \Lambda_N \) by its distance bands. For example, we define

\[ \Lambda^1_N \equiv \sum_{i=1}^{I} \beta_{N1} \{ 0 < d(i, i^*) \leq 0.25 \} (H_{i^*,T} - H_{i^*,1}), \]
with $\Lambda_2^N$ through $\Lambda_4^N$ defined analogously.

In order to give a sense of their magnitudes, we also report the parameters $\Lambda^D$ and $\Lambda^N$ normalized by the number of crimes in various geographic areas in year $t = 1$ (either 1991 for the long panel or 1999 for the short panel). The first normalization we report is to divide $\Lambda^D$ and $\Lambda^N$ by the total number of crimes in all neighborhoods $i \in \{1, \ldots, I\}$ city-wide:

$$
\Lambda_{CW}^D \equiv \frac{\Lambda^D}{\sum_{i=1}^{I} Y_{it=1}}
$$
$$
\Lambda_{CW}^N \equiv \frac{\Lambda^N}{\sum_{i=1}^{I} Y_{it=1}}
$$

We also report $\Lambda^D$ and $\Lambda^N$ normalized by the total crime occurring in affected areas, which for $\Lambda^D$ are all neighborhoods $k \in \{1, \ldots, K^1\}$ containing highrises, and for $\Lambda^N$ are all neighborhoods $k \in \{1, \ldots, K^2\}$ within 0.5 miles of a neighborhood with a highrise:

$$
\Lambda_{AA}^D \equiv \frac{\Lambda^D}{\sum_{k=1}^{K^1} Y_{it=1}}
$$
$$
\Lambda_{AA}^N \equiv \frac{\Lambda^N}{\sum_{k=1}^{K^2} Y_{it=1}}
$$

Finally, at times we discuss the aggregate local effect of high-rise building closures for the city of Chicago, which we define as:

$$
\Lambda^L \equiv \Lambda^D + \Lambda^N.
$$

### 3.4 Local Effects Estimation Results

#### 3.4.1 Local Effects on Homicides: Results using Long Panel (1991-2011)

Our main estimates of the local effect of high-concentration public housing on homicide are presented in Table 2. The top panel shows OLS regression coefficient estimates of $\beta_D$ and $\beta_N$ from Equation 2 and $\zeta_D$ and $\zeta_N$ from Equation 3 where $Y$ is the annual number of homicides per neighborhood using the long panel of data from 1991 through 2011. The $\beta$ parameters in the first three columns are estimated under different definitions of neighborhoods, and the $\zeta$ parameters in the last two columns
are estimated under different assumptions about effects from nearby neighborhoods.

The results in the first column of Table 2 show that on average, closing high-rise public housing buildings led to large decreases in homicides in areas containing and near those buildings. The approximately 18,000 units of high-rise public housing closures from 1990 to 2011 was associated with a drop of about 40 homicides per year in the 30 census tracts containing high-rise public housing buildings. These closures were also associated with a drop of about 28 homicides per year in the census tracts within a 1.5-mile radius of the closed units. These effects are of a large magnitude: The direct effect of the closures represents a decrease in homicides equal to 4.4 percent of all homicides in Chicago in 1991, and the effect on nearby census tracts is a decrease equal to 3.0 percent of city-wide homicides in 1991.

With the exception of $\beta_{N4}$, the magnitudes of the estimates are in line with our expectations. The largest magnitude is on $\beta_D$ (the direct effect), and the estimates decrease in magnitude as distance to the nearest high-rise area increases. Note that in this first specification, when neighborhoods are defined as census tracts, only the direct effect (shown in the first row) is statistically different from zero.

The main results presented in the first column of Table 2 are robust to specifications that measure neighborhoods as smaller geographic areas than census tracts. The magnitude of the overall local effect (direct plus nearby) does not change dramatically between the largest-area tract specification and the more localized block group and block specifications (adding the direct and nearby effects for columns (1), (2), and (3) yield, 7.34%, 8.57%, and 7.85%, respectively). What does change between these specifications is whether the effect is distributed directly in the geographic area containing the high-rises, or in the nearby areas. This implies that some of the effect being attributed to the direct effect in the tract-level specification is actually a nearby effect which can be better measured at the finer block level.

It is also worth noting that the magnitude of the nearby coefficients in column (3) decreases as distance increases for the first three coefficients. The first two coefficients, measuring the effects in the nearest half-mile, are both statistically different from zero. However, the last two nearby coefficients are not distinguishable from zero.

Using these estimates as motivation, column (4) presents estimates of the $\zeta$ parameters in Equation 3. This specification replaces the first two distance indicator variables with log distance to the nearest high-rise block, still interacted with the number of high-rise units that remain open in the nearest block with high-rise public
housing. We also interact this variable with an indicator for being within a half-mile of the nearest high-rise block motivated by the fact that only the $\beta_{N1}$ and $\beta_{N2}$ coefficients were significant in column (3). The negative sign on the log distance coefficient in the top panel implies that within the first half mile of a block with a high-rise, the effect of public housing closure on homicides drops as distance to the nearest high-rise block increases. Also, comparing the implied nearby effect in column (4) (-34.9) to the first two nearby effects in column (3) (-20.3 + -12.3 = -32.6) reveals that the log specification does a reasonably good job parameterizing the decay of the effect as distance increases within the first half-mile.

In Column (5), we add two more explanatory variables. The first is the log distance to the second nearest high-rise block interacted with the number of units still open in the second nearest high-rise block. The third explanatory variable is the same thing but for the third nearest high-rise block. The idea behind these additional variables is that some blocks are close to multiple high-rise blocks and may be affected by changes in the stock of public housing in more than just the nearest high-rise block. In fact, the coefficients on the nearest and second nearest are both statistically different from zero while the third nearest is not.\textsuperscript{36}

The specification presented in column (5) of Table 2 is our preferred specification for measuring the local impact of high-rise public housing closures from 1991 to 2011. It attributes a drop of about 26 homicides per year to the direct effect of high-rise public housing closures and a drop of about 43 to 46 homicides per year to the nearby effect of closures.

### 3.4.2 Local Effects on Homicides: Robustness

Our preferred specification is shown in column (5) of Table 2. These results are robust to a number of variations in sample and specification.\textsuperscript{37} We experimented with a number of different ways in which to cluster the standard errors. Throughout the paper we take the “conservative” approach, and cluster by community area as the resulting standard errors are slightly larger than the other clustering options. Our results are also robust to excluding the developments whose tenants were party to law suits aimed at halting or slowing demolitions (ABLA, Henry Horner, and Cabrini-.....

\textsuperscript{36}We also tried specifications with up to the five nearest high-rise blocks included, but the first two remained the most important, statistically and economically.

\textsuperscript{37}A robustness table and discussion of results are presented in the online appendix.
Green), excluding the highest land value development (Cabrini-Green), excluding the largest development (Robert Taylor). Our estimates do not vary systematically by the occupancy rate of the development (as measured in 1990) or over time. These results are also robust to limiting the sample to blocks within a half mile of high-rise blocks, or just to high-rise blocks. Finally, the results are robust to using a conditional fixed effect Negative Binomial count data model rather than OLS. We do not use logged crime counts as an outcome, because there are many block-time observations in which the number of crimes is zero. Similarly we are not able use crime rate as the dependent variable, since we don’t have an accurate measure of population during intercensal years.\textsuperscript{38}

3.4.3 Local Effects on Homicides: Timing

While the main specification shown in Equation 2 exploits the timing in public housing building closures to estimate the parameter $\beta_D$, the estimate of this parameter represents an average effect of high-rise public housing on crime throughout the sample period. In order to further study the dynamics of the impact of high-rise public housing closures on crime, we estimate the following specification,

$$Y_{i,t} = \alpha_i + \gamma_t + \sum_{j=a}^{b} \theta_j F_{i,t-j} + \epsilon_{i,t}. \quad (4)$$

where $\alpha_i$ are still block fixed effects, $\gamma_t$ are city-wide year effects, and $F_{i,t-j}$ are leads and lags of the number of high-rise building closures in year $t$ in block $i$. In practice, we estimate leads from $j = -9$ through lags of $j = 9$. We omit the lead of two years prior to the high-rise closure; thus all estimates can be interpreted as relative to two years prior to closure. We use two years prior to closure in case the closure process occurred slowly; in which case the year prior to closure would be contaminated by the closure. We also include variables that indicate the maximum number of closures that occurred ten lags or more ago and ten leads or more into the future, as incidental parameters. Since these specifications focus only on the direct effects, we estimate the parameters using a sample of only the high-rise public housing blocks.\textsuperscript{39}

\textsuperscript{38}We do include a proxy for population as an explanatory variable in the next section, but it is only available from 1999-2011.

\textsuperscript{39}We have also estimated the same specification using the broader sample of all Chicago blocks, and the results are almost exactly the same.
The estimates of the $\theta_j$ parameters are presented in Figure 4. In this figure the x-axis labels index $j$. The dotted lines above and below show the 95% confidence bands. Figure 4 shows that with the exception of a statistically insignificant spike 8 years prior to building closure, the estimates of the $\theta_j$ prior to the year of closure are quite close to zero. This provides some assurance that the results presented in Tables 2 are not driven by block specific pre-existing trends with respect to the building closure year.

Of further importance is the fact that the estimates appear to be somewhat stable at around -0.1 in all years after the closure. In effect, this means that the $\beta$ parameter estimates presented in Tables 2 provide a good summary of a fairly stable average effect of high-rise closure.

### 3.4.4 Local Effects on Various Crimes: Results Using the Short Panel (1999-2011)

In addition to the local effects of high-rise closures on homicide for the long panel dataset (1991-2011) presented in Tables 2, we also present estimates of local effects for a variety of crime categories using the short panel dataset (1999-2011) in Table 3. In order to facilitate comparison across crimes, column (1) of Table 3 presents the estimates using homicide as the dependent variable. This specification is equivalent to the specification used in column (5) of Table 2; the only difference is that here the panel runs from only 1999 to 2011 versus 1991 to 2011. We are reassured that the coefficients (shown in the upper panel) change only slightly when moving from the long panel sample (1991-2011) to the short panel sample (1999-2011). The Plan for Transformation was formulated in 1999; thus building closures during the short panel sample are even less likely than those before 1999 to have been slated for closure due to reasons that might be related to crime.

When comparing the middle and lower panels in Table 3 and Table 2, it is important to remember that the total number of high-rise units that closed from 1999 to 2011 is less than the total that closed from 1991 to 2011. This is why very similar coefficients in the top panels of of column (1) of Table 3 and column (5) of Table 2 translate into different numbers in the middle panels. Finally, the percent reductions shown in the bottom panel compare the numbers in the middle panel to the total number of homicides in Chicago in the initial year of the sample, which was lower in 1999 than it was in 1991.
The local effects of high-rise public housing closures vary by type of crime and tend to be largest for violent crimes. For homicide, shots fired, vice and prostitution, and trespassing, we find a statistically significant reduction in crime of 2% or more for both the direct and nearby effects. For assault and battery, the direct effects are slightly larger than a 2% reduction, while the nearby effects are about a 1.6% reduction. 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 direct and nearby effects. These include burglary, theft, vandalism, and disturbance. The overall effects are also relatively small for other non-violent crimes. Truancy and curfew shows a statistically significant direct effect of about -3%, but no discernable nearby effect. Drug crimes and auto thefts show small statistically significant direct effects but no significant nearby effects.

A final crime category considered is gang activity; building closures appear to have no measurable effect on calls for service regarding gang activity. We discuss some possible explanations for this result in Section 4.

3.5 Discussion

The results presented in Tables 2-3 suggest that closing high-rise public housing buildings decreases violent crimes (homicides, shots fired, and assault and battery) in the buildings’ precise location, and does not simply displace those crimes to nearby areas. It is important to note that, for both the direct and nearby effects, the only statistically significant results show that high-rise public housing closures are associated with reductions in crime. Thus, it does not seem to be the case that crime is simply displaced from high-rise blocks to nearby blocks.

However, a remaining question is whether crime is displaced to other areas farther away from the original units. One possibility is that crime may have followed displaced public housing residents to more distant South side and West side neighborhoods. The next section attempts to address this question.
4 Displacement Effects on Receiving Neighborhoods

In this section we attempt estimate the overall effect of high-rise public housing closures on crime in Chicago. We continue to measure the local effects of closures as we did in the previous section. In addition, we are interested in the effect that residents displaced by high-rise closures had on the neighborhoods to which they moved. The sum of these two effects yields an estimate of the total effect of the closures on crime in Chicago.

Estimating the effect of displaced public housing residents on crime in the neighborhoods to which they moved is difficult for a number of reasons. One reason is that data on individuals who lived in high-rise public housing prior to the closures and where they moved after the closures is not readily available. Popkin et al. (2012) were able to obtain restricted access data regarding where displaced public housing residents that opted for private market vouchers relocated. However, they are unable to observe where households moved that opted to move to low-rise developments or where households that did not remain lease compliant and wound up without a subsidy moved. There is some reason to believe that the group of displaced households that were able to remain lease compliant and that opted for private market vouchers rather than moving to a low-rise development may be a positively selected set of households.

To avoid this selection problem, we use credit history data to track individuals displaced from high-rise public housing buildings due to closure. Specifically, we use the Federal Reserve Bank of New York Consumer Credit Panel / Equifax data (FRBNYCCP/Equifax) which contains a 5% sample of the United States population conditional on having a credit history. The FRBNYCCP/Equifax data contain quarterly data on the census block where each individual in the sample lives and span the period from 1999 through 2012. These data have the potential to follow former high-rise residents whether or not they opt for private market vouchers, low-rise public housing, or stay lease-compliant.

However there are a number of potential concerns that arise when attempting to measure the number of displaced public housing households that have relocated to other census blocks using this data. The first concern is whether we can actually identify which households are displaced by closures. We show evidence that most of the census blocks that had high-rise public housing did not have other types of housing
units. Another potential concern is that many public housing residents may not have credit histories and that the segment of the population without a credit history may have a higher (or lower) propensity for criminal activity. We find that as of 2000, 60% of adults in high-rise census blocks had credit histories. Furthermore, even though there is considerable variation in credit history coverage rates across the high-rise blocks (the difference in coverage rates between the 25th and 75th percentile block is 32 percentage points) we find no association between variation in coverage and crime rates. A further concern arises due to the fact that we only have 5% sample rather than the universe of high-rise individuals that have a credit history. This is likely to be a source of measurement error. We show that instrumenting for the number of households that have relocated to a block from high-rise public housing does not change our conclusions regarding the degree to which homicides (the best-measured crime category) are associated with the number of relocated households in a block. A final concern is that displaced high-rise households may select the block to which they moved based upon, possibly unobservable, pre-existing trends. We discuss this concern and show that our estimates are largely robust to this type of selection in a robustness check in section 4.3.2.

4.1 Data and Descriptive Statistics

In order to assess the impact of households displaced by high-rise public housing building closures on the neighborhoods that they move to, we use the FRBNY-CCP/Equifax. The FRBNYCCP/Equifax 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.\footnote{See Avery et al. (2003) for general analysis of credit reporting data.} A key feature of the FRBNYCCP/Equifax 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 FRBNYCCP/Equifax 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.41 Another potential concern is that large proportions 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 FRBNYCCP/Equifax data is important for our purposes. The current policy of the CHA is to run credit checks on applicants.42 While it is unclear how long this has been CHA policy, this is one way that CHA residents could show up in the FRBNYCCP/Equifax. Figure 5 shows the ratio of population implied by the FRBNYCCP/Equifax data to counts from the 2000 Census for eight different age ranges. The blue bars show this ratio for the entire city of Chicago, the red bars for the high-rise tracts, the green bars for the high-rise block groups, and the purple bars for the high-rise blocks. The main thing to notice in the figure is that for individuals aged 25 to 44, it appears that 80% or more counted by the Census in the high-rise blocks have credit histories. As one might expect, the coverage ratio is lower (near 40%) for those aged 18 to 24, and about zero for people under 18. However, these coverage rates are about the same in the high-rise blocks as they are for the city as a whole. The biggest discrepancies in coverage rates between the high-rise blocks and the city as a whole are among older individuals. These discrepancies are not extremely concerning because according to the 2000 Census, only 7% of the population in the high-rise blocks were 55 or older.

Looking across the distribution, comparing the numbers in the FRBNYCCP/Equifax to the 2000 Census, suggests that about 60% of people age 18 or older that live in

41See online appendix.
42See http://www.thecha.org/pages/family_wait_list_lottery_faqs/76.php
the high-rise blocks have credit histories. For those 25 and older the rate jumps up to 67%. We find it reassuring that such a high proportion of the year 2000 population in the high-rise census blocks appears in the FRBNYCCP/Equifax data.

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 FRBNYCCP/Equifax coverage rate and the crime rate of the high-rise blocks which have 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 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 5 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.\(^{43}\) These results make us less worried that we are systematically missing the most crime prone individuals in our measure of displaced public housing residents.

Looking across the full sample period of the FRBNYCCP/Equifax (1999Q1 - 2012Q4), we identify 752 individuals in the primary sample (representing a population of 15,040) 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 in the year of or either of the two years prior to a closure as the last quarter before a closure induced “move”. We are able to observe 646 of the 752 individuals in the data one year after the “move” and 620 individuals two years after the “move”.\(^{44}\) 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

\(^{43}\) The only exception is for auto theft, where the coefficient is marginally statistically significant.

\(^{44}\) This translates into about 86% of the individuals remaining in the sample after a year. The corresponding rate for the entire FRBNYCCP/Equifax data is about 95%. The discrepancy may be associated with individuals showing up in the credit report data while occupying a CHA unit (due to CHA credit checks) but dropping out after moving out.
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 Westside, and farther to the south on the Southside.\textsuperscript{45}

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 row of the table. The group in the first row consists of the 426 blocks where displaced households moved that had no public housing. The second row shows means for the 162 blocks where displaced households moved that had low-rise public housing buildings. The last row 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 than the high-rise blocks. The last three columns of Table 4 report means of median rent, median income and poverty rate.\textsuperscript{46} The high-rise blocks have much lower median rents and incomes than the receiving blocks. Finally, the last column reveals that households displaced from high-rise blocks are moving from extremely high-poverty blocks (62%) to somewhat high-poverty blocks (32% or 27%). These moves look similar to those made by the Section 8 voucher recipients observed in Moving to Opportunity (Kling et al. (2007)).

\textsuperscript{45}See online appendix for more detail.
\textsuperscript{46}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.
4.2 Identification Strategy and Model Specification

In order to assess the degree to which crime is associated with the arrival of displaced public housing residents in their new neighborhoods, we use the dataset described above 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 neighborhood $i$ at time $t$. Defining $j_1$ to be the nearest neighborhood to $i$ receiving public housing migrants, we augment Equation 2 as

$$Y_{i,t} = \alpha_i + \gamma_t + \beta_D H_{i,t} + \beta_N H_{i',t} + \beta_M M_{i,t} + \epsilon_{i,t}, \tag{5}$$

where

$$\beta_M M_{i,t} \equiv \beta_{M1} \mathbf{1}\{\text{no public housing in nbd } i\} M_{i,t}$$

$$+ \beta_{M2} \mathbf{1}\{\text{low-rise public housing in nbd } i\} M_{i,t}$$

$$+ \beta_{M3} \mathbf{1}\{0 < d(i, j_1) \leq 0.5\} \ln(d(i, j_1)) M_{j_1,t}. \tag{6}$$

Equation 3 is augmented similarly.

Analogously to the definition of local effects in Section 3.3, we also decompose the effect from displaced migrants from high-rise building closures into a direct and nearby effect:

$$\Delta^D \equiv \sum_{i=1}^{I} \beta_{M1} \mathbf{1}\{\text{no public housing in nbd } i\} (M_{i,T} - M_{i,1}). \tag{7}$$

$$\Delta^N \equiv \sum_{i=1}^{I} \beta_{M3} \mathbf{1}\{0 < d(i, j_1) \leq 0.5\} \ln(d(i, j_1)) (M_{j_1,T} - M_{j_1,1}). \tag{8}$$

The city-wide displacement parameters $\Delta^D_{CW}$ and $\Delta^N_{CW}$ are defined analogously to the local parameters in Section 3.3 as well. The affected area parameters, however, are defined slightly different here. We report $\Delta^D$ and $\Delta^N$ normalized by the total crime occurring in affected areas, which for $\Delta^D$ are all neighborhoods $k \in \{1, \ldots, K^3\}$ to which displaced households moved, and for $\Delta^N$ are all neighborhoods $k \in \{1, \ldots, K^4\}$. 
within 0.5 miles of a neighborhood to which a displaced household moved:

\[ \Delta_{AA}^D = \frac{\Delta^D}{\sum_{k=1}^{K^3} Y_{it}=1} \]

\[ \Delta_{AA}^N = \frac{\Delta^N}{\sum_{k=1}^{K^4} Y_{it}=1} \]

There are at least three important issues with this specification that merit attention. First, this specification assumes that migrants only commit crimes in neighborhoods within a half-mile radius of their new neighborhood of residence. Second, \( M_{i,t} \) in our data does not truly measure the number of migrants in a neighborhood, but instead is a five-percent sample. This means that a one unit increase in \( M_{i,t} \) represents 20 additional people displaced from high-rise public housing and the estimated coefficients should be interpreted accordingly. This also means that measurement error is a potential concern. Motivated by Black et al. (2000), we experimented with instrumenting for the number of displaced public housing residents that had relocated to each block to address this concern (one instrumental variable was distance from each block to each of the 13 high-rise public housing developments interacted with the cumulative number of people that had been displaced from the development due to closures interacted with the median block group rent and the proportion of the block that is African American). We excluded any blocks within 0.5 miles of a development from the instrument as crime in those blocks is likely to be directly influenced by the high-rise closures rather than through the displacement of residents caused by the closures. The first stage of this regression passes the relevant tests for weak instruments and the sign of the coefficient of number of displaced residents on predicted number of displaced residents is positive (as one would expect). Instrumenting for displaced residents in this manner still leads us to conclude that the displaced residents have no effect on homicide (the best measured crime category).\(^{47}\) This serves to allay our concern regarding bias due to measurement error arising from the 5% sample.\(^ {48}\) A final concern is that this specification ignores the counterfactual flows from displacement neighborhoods to the original public housing neighborhoods that might have

\(^{47}\)The point estimate from the instrumental variable estimate of displaced households on homicide was -0.108 (with a standard error of 0.073). In comparison, the OLS estimates shown in Table 6 are -0.007 (standard error of 0.008) and -0.009 (standard error of 0.006).

\(^{48}\)We also investigated the possibility of purchasing the full universe of the credit histories from Equifax for Chicago for our sample period, but at the moment this does not appear to be feasible.
occurred had the high-rise public housing buildings not been closed. However, we believe these flows would have been negligible, due to the long waiting lists to get into public housing, and the low exit rate from it.

4.3 Displacement Effects Estimation Results

4.3.1 Displacement Effects on Various Crimes: Results Using Short Panel (1999-2011)

Table 6 presents estimation results of the specification in Equation 5. We also add a count of the mean number of observations in each block per year in the FRBNY-CCP/Equifax data as a control for changes in population. Inclusion or omission of this variable has very little impact on our estimates and no impact on our conclusions.

The first thing to note is that the additional explanatory variables have almost no effect on the estimates of the local effects of high-rise closures (the direct and nearby effects). In fact, the first four rows of coefficients and standard errors shown in Table 6 are almost exactly the same as those shown in Table 3. Due to this fact, all of the conclusions regarding the local impact of public housing closures on crime in the previous section remain unchanged. Therefore, we will focus on the displacement effects in this section.

We estimate a displacement effect that is positive and statistically different from zero for the following types of crime: assault and battery, gang activity, burglary, and trespassing. This means that for these 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. For assault and battery and burglary, the increase in crime in the receiving neighborhoods associated with displaced public housing households was about 2% of the total city-wide level of that crime type in 1999. For gang activity and trespassing, the increase associated with displaced high-rise public housing residents was about 5.5% and 4.3% 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 neighborhoods were offset by (or outweighed by in the case of assault and battery) decreases in crime associated with building closure in or nearby 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 5.25%. 

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It is worth noting, though, that this estimate is not statistically distinguishable from zero at the 5% significance level.

While there is broad interest in the degree to which changes to public housing under HOPE VI and Chicago’s Plan for Transformation contributed to the reduction in crime during the past decade and the degree to which it reallocated crime from certain neighborhoods to others, it has been difficult to answer these questions. Because the displacement effect has been hard to measure, it has been hard to assess the total effect of high-rise demolition on crime in Chicago. The last row of the upper panel of Table 6 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 5-10% of 1999 crime levels. The program had more modest effects on assault and battery, theft, vandalism, disturbance, and truancy and curfew: The reductions in these categories range from about 1-4% of 1999 crime levels. Finally, the changes associated with drug activity, burglary, auto theft, or trespassing were small and not statistically significant. The one category that did increase by an economically significant amount was gang activity.

While the increase in gang activity associated with the closure and displacement of residents from high-rise public housing to other neighborhoods is an interesting result and is consistent with predictions that displacement of gang members to rival gang territory could increase crime, it is important to keep two things in mind. First, as mentioned above, this result is not statistically significant at the 5% level. Second, this is likely to be one of the categories most affected by reporting bias. It may be that the threshold for calling 911 regarding gang loitering or gang drug sales was much higher in the high-rises than it is in the neighborhoods where displaced households relocated.

When assessing these results, a possible concern is that policing patterns may have changed in response to high-rise closures and the relocation of public housing residents. To assess the degree to which this could influence our results, we re-estimated all but the last column of Table 6 including truancy and curfew as a control for policing intensity. Truancy and curfew violations are most likely the result of police officers stopping children rather than calls to 911. For this reason, the truancy and curfew measure may serve as a proxy for the presence of police. We find that coefficient on truancy and curfew is positive and statistically significant for all crime
types, but its inclusion has very little effect on our estimates.\(^{49}\) For this reason, we do not believe that our results are primarily driven by changes in policing intensity. However, to the extent that truancy and curfew is an imperfect proxy for policing, our results can be thought of as the effect of the changes in public housing, allowing policing strategy to respond endogenously.

4.3.2 Displacement Effects: Robustness

We also estimate a second specification of Equation 5 as a robustness check. We label the first specification, which is estimated above, the block fixed effect (BFE) specification. The second specification, which we label the Bayer-Ross-Topa (BRT) specification in reference to Bayer et al. (2008b), allows for block-group-specific time trends:

\[
Y_{i,t} = \alpha_{BG_{i,t}} + \beta_{D}H_{i,t} + \beta_{N}H_{i,t} + \beta_{M}M_{i,t} + \epsilon_{i,t}.
\]  

(9)

The key assumption in the BRT specification is that individuals displaced from high-rise public housing can choose the block group into which they move, but that the housing market is too thin for them to choose the specific block within that block group. In the BRT specification, \(\beta_{M2}\) will capture the additional effects of migrants on the specific block to which they move relative to the block group to which they move. Any increase in crime in the entire block group will be captured in \(\alpha_{BG_{i,t}}\), which is a block group * year effect. Thus, \(\beta_{M2}\) in this specification will not capture the full magnitude of displacement effects.

When interpreting the BRT results, it is important to remember that as shown in Equation 6 we distinguish between those displaced residents who are likely to be using private market vouchers (or are living in private market housing without the aid of vouchers), and those who are likely to have relocated to a low-rise public housing unit. We do so because those who move into a low-rise public housing unit are unlikely to satisfy the identifying assumptions in the BRT specification, and therefore in the BRT specification we focus on the effects of private voucher movers. The estimates of the direct and nearby effects shown in both the BFE and BRT specifications are almost identical, and thus also quite similar to the estimation results from the local

\(^{49}\)Due to space constraints, these results are available from the authors upon request.
effects specifications presented earlier.\(^{50}\)

We find support for the assumptions of the BRT specification. If housing markets are indeed too thin for households to pick the specific block within a block group to which they move, then observable characteristics of the census blocks should not be predictive of an increased probability of receiving displaced households relative to the surrounding block group. We find that in the sample of block groups with no low-rise public housing units, census block characteristics are not predictive of whether the block receives displaced high-rise public housing households above what is predicted by the number of housing units in the block. In contrast, in block groups that have low-rise public housing units, the proportion of housing units that are owner-occupied and the proportion of the population that are African-American are both predictive of which block within the block group will receive displaced high-rise public housing residents.\(^{51}\)

For the most part, the estimated displacement effects shown in the last three rows of Table 6 are similar to those shown in upper panel of Table 6. The biggest difference is for drug activity where the displacement effect is not statistically different from zero in Table 6, but is both statistically significant and somewhat large economically in last row of Table 6. One possible explanation for the differences in these estimates is that, on average, displaced households moved to blocks where drug activity was trending down, but in fact the effect of their arrival was to increase drug activity; thus the observed combination of the pre-trend and the effect we would like to measure is the zero (-0.20%) that is observed. In contrast, under this explanation the BRT specification reveals the true effect of displaced households on drug activity.

One worry regarding the BRT specification mentioned above was that when we observe moves to block groups that contain low-rise public housing, it is unlikely that the block picked within the block group is random; it is probably the case that the household is relocating to a public housing unit so they are more likely to end up in the block that has the public housing. If this were a problem, we might expect the estimate of \(\beta_{M1}\) to be quite different from that of \(\beta_{M2}\). For the the most part, however, these estimates are quite similar. The fact that the BRT specification estimate of \(\Delta_{CW}^{D}\) does not change by much relative to the total effect estimate in the BFE specification, except, possibly, for drug activity, provides a degree of reassurance

\(^{50}\) These estimates are shown in the full BRT table in the online appendix.

\(^{51}\) See online appendix for details.
that selection of neighborhoods by displaced public housing residents on pre-existing
neighborhood trends is not severely biasing our results.

5 Conclusion

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 where the buildings were located
and in the neighborhoods where households displaced by the closures moved. We
find that the 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.

We find evidence that increases in a few of these crime measures are associated
with the arrival of households displaced from closed high-rise public housing buildings.
The increases in assault and battery and burglary in the areas within a half-mile of
blocks to which displaced households moved are equivalent to about 2% of their city-
wide level in 1999, while gang activity and trespassing show somewhat larger increases
associated with the arrival of displaced households from the high-rises (about 4% to
5.5% of 1999 city-wide level).

In our preferred specifications, the net effect for the city as a whole is large reduc-
tions in homicide, shots fired, vice and prostitution, and smaller reductions in assault
and battery, theft, vandalism, disturbance, and truancy or curfew. The total effect
on drug activity, burglary, auto theft, and trespassing is about zero. The total effect
on gang activity is an increase.

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**Hartley, Daniel A.**, *Essays in urban economics*, University of California, Berkeley, 2009.


Figure 1: Distance to Nearest Block with High-rise Public Housing Building

Figure 2: Units Closed in High-rise Buildings by Development
Figure 3: Total Homicides and High-rise Public Housing Units Over Time

Figure 4: Event Study Coefficients of High-rise Building Closure on Homicide
Figure 5: FRBNYCCP / Equifax Coverage Compared to 2000 Census Population
## Table 1: Mean Demographic Characteristics and Crime Counts for High-rise Areas and Entire City

<table>
<thead>
<tr>
<th>Sample</th>
<th>Obs.</th>
<th>Geography</th>
<th>Year</th>
<th>Population</th>
<th>Housing Units</th>
<th>Vacancy Rate</th>
<th>Owner-Occupancy Rate</th>
<th>African-American</th>
<th>Median Home Value</th>
<th>Median Rent</th>
<th>Median Household Income</th>
<th>Poverty Rate</th>
<th>High School Degree or Higher</th>
<th>College Degree or Higher</th>
</tr>
</thead>
<tbody>
<tr>
<td>High-rise</td>
<td>31</td>
<td>Tract</td>
<td>1990</td>
<td>2,929</td>
<td>1,216</td>
<td>24.0%</td>
<td>6.3%</td>
<td>91.1%</td>
<td>$121,215</td>
<td>$277</td>
<td>$13,656</td>
<td>71.0%</td>
<td>44.6%</td>
<td>6.3%</td>
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<td></td>
<td>38</td>
<td>Block Group</td>
<td>1990</td>
<td>1,861</td>
<td>758</td>
<td>25.3%</td>
<td>6.0%</td>
<td>93.0%</td>
<td>$78,506</td>
<td>$243</td>
<td>$13,444</td>
<td>72.4%</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>31</td>
<td>Tract</td>
<td>2000</td>
<td>2,233</td>
<td>1,044</td>
<td>29.1%</td>
<td>12.5%</td>
<td>86.1%</td>
<td>$185,512</td>
<td>$328</td>
<td>$23,445</td>
<td>55.9%</td>
<td>55.8%</td>
<td>11.4%</td>
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<tr>
<td></td>
<td>38</td>
<td>Block Group</td>
<td>2000</td>
<td>1,223</td>
<td>583</td>
<td>30.3%</td>
<td>9.0%</td>
<td>91.2%</td>
<td>$167,805</td>
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<td>53.9%</td>
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</tr>
<tr>
<td></td>
<td>90</td>
<td>Block</td>
<td>2000</td>
<td>302</td>
<td>142</td>
<td>35.9%</td>
<td>7.4%</td>
<td>95.5%</td>
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<td></td>
</tr>
<tr>
<td>Chicago</td>
<td>875</td>
<td>Tract</td>
<td>1990</td>
<td>3,237</td>
<td>1,316</td>
<td>10.5%</td>
<td>39.6%</td>
<td>42.4%</td>
<td>$154,616</td>
<td>$603</td>
<td>$36,197</td>
<td>25.2%</td>
<td>61.8%</td>
<td>16.8%</td>
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<td></td>
<td>2,477</td>
<td>Block Group</td>
<td>1990</td>
<td>1,134</td>
<td>463</td>
<td>9.5%</td>
<td>46.4%</td>
<td>41.9%</td>
<td>$143,051</td>
<td>$618</td>
<td>$44,314</td>
<td>21.3%</td>
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<td></td>
<td>875</td>
<td>Tract</td>
<td>2000</td>
<td>3,369</td>
<td>1,340</td>
<td>9.6%</td>
<td>42.2%</td>
<td>42.7%</td>
<td>$213,897</td>
<td>$671</td>
<td>$47,365</td>
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<td>2000</td>
<td>1,176</td>
<td>468</td>
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<td>48.0%</td>
<td>43.4%</td>
<td>$188,605</td>
<td>$664</td>
<td>$49,873</td>
<td>16.8%</td>
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<td></td>
<td>24,639</td>
<td>Block</td>
<td>2000</td>
<td>118</td>
<td>47</td>
<td>8.3%</td>
<td>54.9%</td>
<td>40.8%</td>
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<table>
<thead>
<tr>
<th>Sample</th>
<th>Obs.</th>
<th>Year</th>
<th>Homicide</th>
<th>Shots Fired</th>
<th>Assault and Battery</th>
<th>Gang Activity</th>
<th>Drugs</th>
<th>Vice and Prostitution</th>
<th>Burglary</th>
<th>Theft</th>
<th>Auto Theft</th>
<th>Vandalism</th>
<th>Trespassing</th>
<th>Disturbance</th>
<th>Truancy and Curfew</th>
</tr>
</thead>
<tbody>
<tr>
<td>High-rise</td>
<td>90</td>
<td>1999</td>
<td>0.27</td>
<td>38.84</td>
<td>134.74</td>
<td>6.18</td>
<td>24.00</td>
<td>23.74</td>
<td>12.07</td>
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<td>4.81</td>
<td>14.94</td>
<td>7.64</td>
<td>70.22</td>
<td>3.82</td>
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<tr>
<td></td>
<td>90</td>
<td>2011</td>
<td>0.04</td>
<td>4.09</td>
<td>24.08</td>
<td>1.16</td>
<td>5.09</td>
<td>2.56</td>
<td>2.41</td>
<td>4.96</td>
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<td>1.60</td>
<td>18.23</td>
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<tr>
<td>Chicago</td>
<td>24,661</td>
<td>1999</td>
<td>0.02</td>
<td>3.08</td>
<td>12.13</td>
<td>1.34</td>
<td>3.65</td>
<td>2.13</td>
<td>2.42</td>
<td>4.38</td>
<td>1.72</td>
<td>2.56</td>
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<td>15.33</td>
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<tr>
<td></td>
<td>24,661</td>
<td>2011</td>
<td>0.02</td>
<td>3.64</td>
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<td>1.99</td>
<td>3.56</td>
<td>1.42</td>
<td>2.40</td>
<td>3.99</td>
<td>1.39</td>
<td>2.45</td>
<td>1.01</td>
<td>13.43</td>
<td>0.18</td>
</tr>
</tbody>
</table>

**Note:** All dollar denominated amounts 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 mean block distance to CBD (State and Madison) is 3.2 miles. Chicago block mean distance to CBD is 7.5 miles. Lower panel shows mean crime counts.
Table 2: Local Effects of Public Housing Demolition on Homicide

<table>
<thead>
<tr>
<th>Geography</th>
<th>Tract</th>
<th>Block Group</th>
<th>Block</th>
<th>Block</th>
<th>Block</th>
</tr>
</thead>
<tbody>
<tr>
<td>HR Units (100s), $\beta_D$ or $\zeta_D$</td>
<td>0.234 (0.026)</td>
<td>0.237 (0.019)</td>
<td>0.156 (0.027)</td>
<td>0.156 (0.027)</td>
<td>0.149 (0.027)</td>
</tr>
<tr>
<td>$D(0.0,0.25)$ * Nearest HR Units (100s), $\beta_{N1}$</td>
<td>0.022 (0.026)</td>
<td>0.011 (0.010)</td>
<td>0.012 (0.003)</td>
<td>0.012 (0.003)</td>
<td>0.012 (0.003)</td>
</tr>
<tr>
<td>$D(0.25,0.5)$ * Nearest HR Units (100s), $\beta_{N2}$</td>
<td>0.017 (0.012)</td>
<td>0.019 (0.006)</td>
<td>0.005 (0.002)</td>
<td>0.005 (0.002)</td>
<td>0.005 (0.002)</td>
</tr>
<tr>
<td>$D(0.5,1)$ * Nearest HR Units (100s), $\beta_{N3}$</td>
<td>0.006 (0.010)</td>
<td>0.008 (0.005)</td>
<td>0.001 (0.002)</td>
<td>0.001 (0.002)</td>
<td>0.001 (0.002)</td>
</tr>
<tr>
<td>$D(1,1.5)$ * Nearest HR Units (100s), $\beta_{N4}$</td>
<td>0.033 (0.021)</td>
<td>0.015 (0.008)</td>
<td>0.003 (0.003)</td>
<td>0.003 (0.003)</td>
<td>0.003 (0.003)</td>
</tr>
<tr>
<td>$D(0,0.5)$ * log(D) * Nearest HR Units (100s), $\zeta_{N1}$</td>
<td>-0.006 (0.001)</td>
<td>-0.004 (0.001)</td>
<td>-0.004 (0.002)</td>
<td>-0.004 (0.002)</td>
<td>-0.004 (0.002)</td>
</tr>
<tr>
<td>$D2(0,0.5)$ * log(D2) * 2nd Nearest HR Units (100s), $\zeta_{N2}$</td>
<td>-0.004 (0.002)</td>
<td>-0.004 (0.002)</td>
<td>-0.004 (0.002)</td>
<td>-0.004 (0.002)</td>
<td>-0.004 (0.002)</td>
</tr>
<tr>
<td>$D3(0,0.5)$ * log(D3) * 3rd Nearest HR Units (100s), $\zeta_{N3}$</td>
<td>-0.001 (0.002)</td>
<td>-0.001 (0.002)</td>
<td>-0.001 (0.002)</td>
<td>-0.001 (0.002)</td>
<td>-0.001 (0.002)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>$R^2$</th>
<th>Obs</th>
<th>14,396</th>
<th>52,059</th>
<th>517,881</th>
<th>517,881</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\Lambda^D$</td>
<td>-40.1 (4.4)</td>
<td>-40.7 (3.3)</td>
<td>-26.8 (4.7)</td>
<td>-26.7 (4.7)</td>
<td>-25.6 (4.6)</td>
</tr>
<tr>
<td>$D(0,0.25)$ * Nearest HR Units (100s), $\Lambda^N_{N1}$</td>
<td>-2.3 (2.7)</td>
<td>-3.0 (2.8)</td>
<td>-20.3 (5.6)</td>
<td>-20.3 (5.6)</td>
<td>-20.3 (5.6)</td>
</tr>
<tr>
<td>$D(0.25,0.5)$ * Nearest HR Units (100s), $\Lambda^N_{N2}$</td>
<td>-7.9 (5.4)</td>
<td>-14.2 (4.2)</td>
<td>-12.3 (5.8)</td>
<td>-12.3 (5.8)</td>
<td>-12.3 (5.8)</td>
</tr>
<tr>
<td>$D(0.5,1)$ * Nearest HR Units (100s), $\Lambda^N_{N3}$</td>
<td>-4.1 (7.0)</td>
<td>-8.7 (5.1)</td>
<td>-5.4 (8.7)</td>
<td>-5.4 (8.7)</td>
<td>-5.4 (8.7)</td>
</tr>
<tr>
<td>$D(1,1.5)$ * Nearest HR Units (100s), $\Lambda^N_{N4}$</td>
<td>-13.4 (8.5)</td>
<td>-11.6 (6.4)</td>
<td>-6.8 (7.7)</td>
<td>-6.8 (7.7)</td>
<td>-6.8 (7.7)</td>
</tr>
</tbody>
</table>

| $D(0,0.5)$ * log(D) * Nearest HR Units (100s), $\Lambda^N_{N1}$ | -34.9 (7.3) | -21.3 (5.7) |
| $D2(0,0.5)$ * log(D2) * 2nd Nearest HR Units (100s), $\Lambda^N_{N2}$ | -21.2 (9.9) |
| $D3(0,0.5)$ * log(D3) * 3rd Nearest HR Units (100s), $\Lambda^N_{N3}$ | -4.4 (5.6) |

| $\Lambda^N$ | -27.6 (15.2) | -37.4 (11.6) | -44.8 (15.5) | -34.9 (7.3) | -46.9 (11.0) |

<table>
<thead>
<tr>
<th>Affected Area</th>
<th>Tract</th>
<th>Block Group</th>
<th>Block</th>
<th>Block</th>
</tr>
</thead>
<tbody>
<tr>
<td>Direct, $\Lambda^D_{AA}$</td>
<td>-34.31% (3.80%)</td>
<td>-38.75% (3.15%)</td>
<td>-41.16% (7.19%)</td>
<td>-41.14% (7.18%)</td>
</tr>
<tr>
<td>Nearby, $\Lambda^D_{AA}$</td>
<td>-8.10% (4.44%)</td>
<td>-10.43% (3.22%)</td>
<td>-11.18% (3.86%)</td>
<td>-20.78% (4.35%)</td>
</tr>
<tr>
<td>City-wide</td>
<td>Tract</td>
<td>Block Group</td>
<td>Block</td>
<td>Block</td>
</tr>
<tr>
<td>Direct, $\Lambda^D_{CW}$</td>
<td>-4.35% (0.48%)</td>
<td>-4.41% (0.30%)</td>
<td>-2.90% (0.51%)</td>
<td>-2.90% (0.51%)</td>
</tr>
<tr>
<td>Nearby, $\Lambda^D_{CW}$</td>
<td>-2.99% (1.64%)</td>
<td>-4.06% (1.25%)</td>
<td>-4.86% (1.68%)</td>
<td>-3.78% (0.79%)</td>
</tr>
</tbody>
</table>

Note: Heteroskedasticity robust standard errors clustered by community area in parentheses.
Table 3: Local Effects of Public Housing Demolition on All Crimes - 1999-2011

<table>
<thead>
<tr>
<th></th>
<th>Homicide</th>
<th>Shots Fired</th>
<th>Assault and Battery</th>
<th>Gang Activity</th>
<th>Drugs</th>
<th>Vice and Prostitution</th>
<th>Burglary</th>
<th>Theft</th>
<th>Auto Theft</th>
<th>Vandalism</th>
<th>Trespassing</th>
<th>Disturbance</th>
<th>Truancy and Curfew</th>
</tr>
</thead>
<tbody>
<tr>
<td>HR Units (100s), ζ_0</td>
<td>0.148</td>
<td>29.339</td>
<td>77.773</td>
<td>3.050</td>
<td>14.505</td>
<td>17.908</td>
<td>4.826</td>
<td>4.969</td>
<td>1.869</td>
<td>6.904</td>
<td>5.695</td>
<td>35.588</td>
<td>1.853</td>
</tr>
<tr>
<td>(0.036)</td>
<td>(3.637)</td>
<td>(8.259)</td>
<td>(0.424)</td>
<td>(1.386)</td>
<td>(0.702)</td>
<td>(0.476)</td>
<td>(0.766)</td>
<td>(0.208)</td>
<td>(0.427)</td>
<td>(0.927)</td>
<td>(0.224)</td>
<td>(1.449)</td>
<td>(0.331)</td>
</tr>
<tr>
<td>D(0,0.5) * log(D) *</td>
<td>-0.005</td>
<td>0.104</td>
<td>-0.425</td>
<td>0.020</td>
<td>-0.010</td>
<td>-0.324</td>
<td>-0.145</td>
<td>-0.098</td>
<td>0.001</td>
<td>-0.158</td>
<td>-0.118</td>
<td>-0.981</td>
<td>-0.099</td>
</tr>
<tr>
<td>&amp; Λ</td>
<td>(0.001)</td>
<td>(0.055)</td>
<td>(0.026)</td>
<td>(0.019)</td>
<td>(0.011)</td>
<td>(0.036)</td>
<td>(0.074)</td>
<td>(0.025)</td>
<td>(0.076)</td>
<td>(0.038)</td>
<td>(0.029)</td>
<td>(0.016)</td>
<td></td>
</tr>
<tr>
<td>D(0,0.5) * log(D) *</td>
<td>-0.005</td>
<td>-0.823</td>
<td>-1.308</td>
<td>-0.173</td>
<td>-0.324</td>
<td>-0.312</td>
<td>-0.145</td>
<td>-0.098</td>
<td>0.001</td>
<td>-0.158</td>
<td>-0.118</td>
<td>-0.981</td>
<td>-0.099</td>
</tr>
<tr>
<td>&amp; Λ</td>
<td>(0.001)</td>
<td>(0.055)</td>
<td>(0.026)</td>
<td>(0.019)</td>
<td>(0.011)</td>
<td>(0.036)</td>
<td>(0.074)</td>
<td>(0.025)</td>
<td>(0.076)</td>
<td>(0.038)</td>
<td>(0.029)</td>
<td>(0.016)</td>
<td></td>
</tr>
<tr>
<td>D(0,0.5) * log(D) *</td>
<td>0.010</td>
<td>0.155</td>
<td>0.317</td>
<td>0.037</td>
<td>-0.077</td>
<td>-0.084</td>
<td>-0.162</td>
<td>0.011</td>
<td>0.012</td>
<td>0.017</td>
<td>-0.016</td>
<td>0.078</td>
<td>0.021</td>
</tr>
<tr>
<td>&amp; Λ</td>
<td>(0.001)</td>
<td>(0.055)</td>
<td>(0.026)</td>
<td>(0.019)</td>
<td>(0.011)</td>
<td>(0.036)</td>
<td>(0.074)</td>
<td>(0.025)</td>
<td>(0.076)</td>
<td>(0.038)</td>
<td>(0.029)</td>
<td>(0.016)</td>
<td></td>
</tr>
<tr>
<td>R2</td>
<td>0.110</td>
<td>0.611</td>
<td>0.825</td>
<td>0.387</td>
<td>0.576</td>
<td>0.575</td>
<td>0.572</td>
<td>0.737</td>
<td>0.533</td>
<td>0.587</td>
<td>0.420</td>
<td>0.750</td>
<td>0.430</td>
</tr>
<tr>
<td>Obs</td>
<td>320,593</td>
<td>320,593</td>
<td>320,593</td>
<td>320,593</td>
<td>320,593</td>
<td>320,593</td>
<td>320,593</td>
<td>320,593</td>
<td>320,593</td>
<td>320,593</td>
<td>320,593</td>
<td>320,593</td>
<td>320,593</td>
</tr>
</tbody>
</table>

Note: Heteroskedasticity robust standard errors clustered by community area in parentheses.
### Table 4: Census 2000 Block Characteristics

<table>
<thead>
<tr>
<th>Observations</th>
<th>Housing Units</th>
<th>Population Density</th>
<th>Vacancy Rate</th>
<th>Proportion Owner-Occupied</th>
<th>Proportion African-American</th>
<th>Proportion Hispanic</th>
<th>Distance to CBD (miles)</th>
<th>Median Rent</th>
<th>Median Income</th>
<th>Poverty Rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Relocation Blocks - in Block Groups with No Public Housing</td>
<td>426</td>
<td>81</td>
<td>23,634</td>
<td>13.1%</td>
<td>37.9%</td>
<td>89.0%</td>
<td>5.6%</td>
<td>7.35</td>
<td>$612</td>
<td>$38.4K</td>
</tr>
<tr>
<td>Relocation Blocks - in Block Groups with Low-rise Public Housing</td>
<td>162</td>
<td>83</td>
<td>25,487</td>
<td>13.0%</td>
<td>30.0%</td>
<td>85.4%</td>
<td>10.6%</td>
<td>6.58</td>
<td>$541</td>
<td>$32.8K</td>
</tr>
<tr>
<td>High-rise Blocks</td>
<td>90</td>
<td>142</td>
<td>36,981</td>
<td>35.9%</td>
<td>7.4%</td>
<td>95.5%</td>
<td>2.7%</td>
<td>3.22</td>
<td>$239</td>
<td>$16.1K</td>
</tr>
</tbody>
</table>

Note: Median rent, median household income, and poverty rate are measured at the block group level.

### Table 5: Lack of Relationship between Credit Panel Coverage Rate and Crime Rate in High Rise Blocks in 2000

<table>
<thead>
<tr>
<th>Homicide</th>
<th>Shots Fired</th>
<th>Assault and Battery</th>
<th>Gang Activity</th>
<th>Drugs</th>
<th>Vice and Prostitution</th>
<th>Burglary</th>
<th>Theft</th>
<th>Auto Theft</th>
<th>Vandalism</th>
<th>Trespassing</th>
<th>Disturbance</th>
<th>Truancy and Curfew</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCP Coverage</td>
<td>0.0017</td>
<td>(0.0011)</td>
<td>0.96</td>
<td>(0.86)</td>
<td>0.059</td>
<td>(0.042)</td>
<td>0.25</td>
<td>(0.21)</td>
<td>0.94</td>
<td>(0.84)</td>
<td>0.14</td>
<td>(0.11)</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.097</td>
<td>0.073</td>
<td>0.083</td>
<td>0.101</td>
<td>0.078</td>
<td>0.075</td>
<td>0.085</td>
<td>0.021</td>
<td>0.116</td>
<td>0.081</td>
<td>0.089</td>
<td>0.080</td>
</tr>
</tbody>
</table>

Note: Each column 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 6: Total Effect of Public Housing Demolition on All Crimes - 1999-2011 - Block FE

<table>
<thead>
<tr>
<th></th>
<th>R2</th>
<th>Robustness - BRT</th>
</tr>
</thead>
<tbody>
<tr>
<td>Local -</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Displaced HH</td>
<td>0.110</td>
<td>0.612</td>
</tr>
<tr>
<td></td>
<td>(1.98%)</td>
<td>(1.84%)</td>
</tr>
<tr>
<td>Direct, $\Delta_D$</td>
<td>0.000</td>
<td>2.431</td>
</tr>
<tr>
<td>(0.98%)</td>
<td>(0.355)</td>
<td>(1.562)</td>
</tr>
<tr>
<td>Low-rise, $\beta_{M2}$</td>
<td>-0.020</td>
<td>0.925</td>
</tr>
<tr>
<td>(0.05%)</td>
<td>(0.135)</td>
<td>(0.753)</td>
</tr>
<tr>
<td>Nearby, $\Delta_N$</td>
<td>0.017</td>
<td>0.825</td>
</tr>
<tr>
<td>(0.18%)</td>
<td>(0.170)</td>
<td>(0.753)</td>
</tr>
<tr>
<td>Total -</td>
<td>-7.66%</td>
<td>-5.38%</td>
</tr>
<tr>
<td>(1.98%)</td>
<td>(1.84%)</td>
<td>(0.85%)</td>
</tr>
</tbody>
</table>

Note: all specifications have 320,593 observations. Heteroskedasticity robust standard errors clustered by community area in parentheses.