An Econometric Analysis of the Impact of Affordable Housing Policy on Crime Rates

Emma Nelson

Econometrics Thesis

Professor Mei

March 22, 2024

Abstract

This paper plans to expand upon previous literature by investigating how low-income housing policy, particularly Qualified Census Tract (QCT) status, impacts crime rates. This question is explored in three parts: firstly, QCT status is aggregated at the county level, and FBI crime data for 2007, 2010, 2013, and 2016 are used. Secondly, QCTs within Chicago and Chicago Uniform Crime Data for the same years are used. Thirdly, the number of newly allocated housing units within Chicago census tracts and Chicago Uniform Crime Data for 2006-2016 are used. The study employs several fixed effects models and finds a statistically significant negative correlation between crime and QCTs at the county level. Such findings suggest that low-income housing policy may effectively revitalize distressed neighborhoods and reduce county crime. Using the same fixed effects model but looking at census tract data from Chicago, the study finds no statistically significant relationship between QCT status or the number of newly allocated units and crime. These findings suggest that while low-income policy does not reduce crime within neighborhoods, it does not increase it either.
Keywords: Low-income housing policy, Crime rates, Fixed-effects model, Crime reduction

JEL Codes: C50

Introduction

Subsidized housing policy and affordable housing development, both large and small, are often met with significant contention, frequently involving a debate over the fear of their perceived association with increased crime rates. However, the connection between low-income housing and crime must be better understood. John Macdonald, a researcher in community design and crime, suggests that "zoning, designs of streets and housing, locations of public transit, and land uses shape the built environment in ways that can increase or reduce crime" (Macdonald, 2015). Low-income housing aims to help communities by generating positive externalities, like financial security and stability, that help reinvigorate declining neighborhoods. Concurrently, low-income housing may also generate negative externalities. For example, affordable housing increases the concentration of poverty, which has potentially detrimental effects. Notably, higher concentrations of poverty limit access to appropriate schools, satisfactory jobs, and other means of upward economic and social mobility.

Another significant externality associated with low-income housing developments is its implications for criminal activity. Researchers have yet to reach a consensus on the impacts of affordable housing and its associated policy on crime rates. Researchers cite several opposing phenomenons through which low-income housing could affect crime rates. Firstly, affordable housing is explicitly selected for low-income people, and low-income people are more likely to partake in criminal behavior. Several studies have found a strong correlation between income and the likelihood of partaking in criminal behavior. Bejerk (2007) finds that economic resources are a stronger predictor of youth criminal activity than gender. Secondly, affordable housing concentrates low-income people, which magnifies network effects that promote criminal activity. A large body of research suggests that youth and adults living in disadvantaged communities are at elevated risk of engaging in criminal behavior even after controlling for individual socio-demographic characteristics and other risk factors (Sciandra et al., 2013). Thirdly, affordable housing improves living standards and provides financial security, which reduces criminal activity. Literature suggests that criminal activity is often motivated by insecurity and inequality; as someone's sentiments towards these two factors decrease, so does their likelihood of committing a crime.

These opposing theories highlight the need for a deeper understanding of how low-income housing interacts with the external environment. This paper investigates how low-income housing policy, specifically the Low-Income Housing Tax Credit (LIHTC) program, impacts crime rates. The question is first explored through a multivariate fixed effects model, a statistical method that controls for unobserved time-invariant factors, such as local culture or geography, that could influence independent and dependent variables. This model controls for year and county, where the independent variable of interest is QCT status, and the dependent variable is county crime rates. Violent crime and property crime are separated to provide more insight into the causal mechanisms behind how low-income housing impacts crime rates. This analysis finds that QCT status aggregated at the county level negatively correlates with violent and property crimes. Such findings suggest that as more census tracts within a county qualify for QCT status, there is an associated decrease in property and violent crime rates.

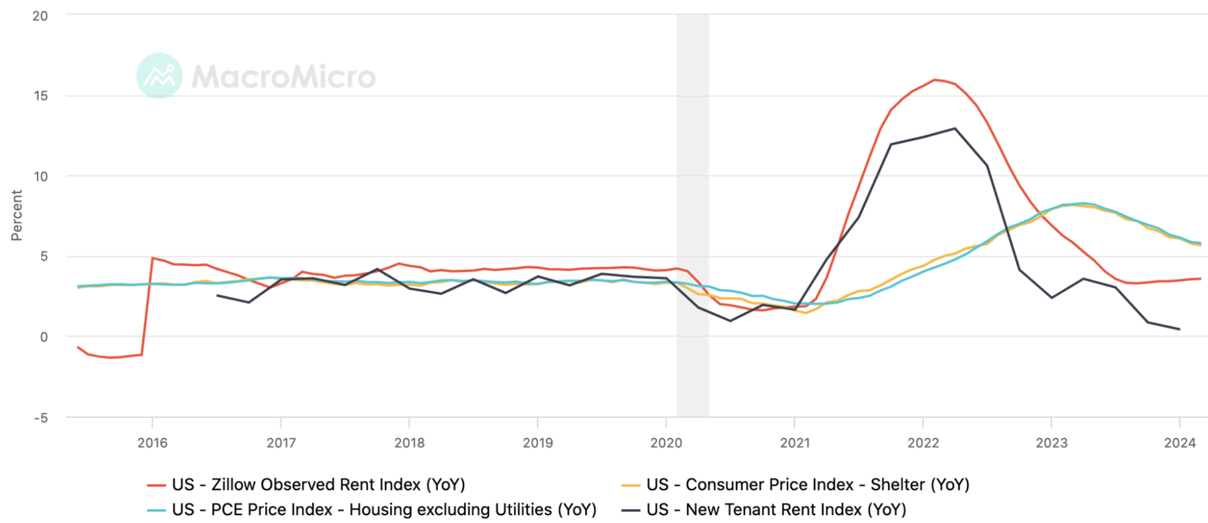
Our second investigation takes a more detailed approach. We use a multivariate fixed effects model that controls for year and census tracts. The independent variable of interest is QCT status, and the dependent variable is tract crime rates. By focusing on the more specific types of crime at the census tract level, we aim to provide a deeper understanding of the causal mechanisms between low-income housing and crime rates. Our analysis finds that QCT has no statistically significant impact on violent, property, and other crimes.

This question is thirdly investigated through a multivariate fixed effects model that controls for year and census tracts, where the independent variable of interest is the number of newly allocated low-income housing units, and the dependent variable is changes in tract crime rates. Investigating how the number of units, not just how qualifying for the policy, impacts crime rates provides even deeper insight into the causal mechanisms between low-income housing and crime rates. This analysis finds that the number of newly allocated units has no statistical impact on violent, property, and other crimes. This research aims to offer data-driven insights that help more thoroughly inform policymakers, urban planners, and the public alike on the impacts of low-income housing.

Background

The rising cost of living is one of the most widely discussed macroeconomic phenomena today. Despite inflation starting to normalize around the Federal Reserve's target of 2 percent in recent months, inflation still presents a pressing issue to many families across the United States. One area that is being disproportionately impacted by inflation is the housing market. In the United States, rent prices have surged by 208% since 1985, going from $378 a month to $1,163. This sharp increase is especially troublesome given that rent is the most significant expense for most Americans, with the typical individual allocating roughly 30% of their monthly income toward rent or mortgage payments. One effective way the government can ease the burden of surging rent prices is through low-income housing programs.

Figure 1: This graph shows the average monthly percent increase in rent cost since 2015 in the United States. (Data: MacroMicro)



Low-income housing started in the US during the Great Depression in the 1930s. The first policy introduced was the National Housing Act (NHA) of 1934. The NHA was part of the New Deal and aimed to aid families struggling because of the great depression and revitalize lower-income communities. In 1965, the Housing and Development Act created the United States Department of Housing and Urban Development Department (HUD). The HUD currently provides 970,000 households with public housing units. They also offer additional services to those who qualify. Now, HUD most commonly provides affordable housing through the Low-Income Housing Tax Credit (LIHTC) program. This program gives tax breaks to developers who build low-income housing in qualifying neighborhoods. The Tax Reform Act of 1986 created this program, giving state and local LIHTC-allocating agencies approximately $9 billion annually. This tax credit incentivizes developers to invest in qualified census tracts, though there are instances where LIHTC projects can be constructed outside these areas. To qualify for QCT status, a tract must have 50% household incomes below 60% of the Area Median Gross Income or have a poverty rate of 25% or more. While there are other low-income housing initiatives like public housing, this paper aims to examine this policy specifically as it is the most used policy. Examining this policy and its implications for crime rates is exceptionally timely and can change the public discourse surrounding affordable housing policy.

Literature review

Existing research in the econometric field suggests that crime is heavily influenced by the "built environment" and offers conflicting conclusions regarding its correlation with low-income housing and its associated policy. Existing research from Fagan and Davies (2000) regarding public housing in Bronx County, New York, suggests a positive correlation with crime. They base their study on the "Broken Windows" theory, which suggests that neighborhoods with a greater concentration of physical and social disorder have higher crime incidences, especially “quality of life' ' crimes. The "Broken Windows" theory produces two predictions concerning the impacts of increased public housing in an area. Public housing can provide residents with an increased sense of security, leading to increased social order, which theoretically should reduce crime. However, at the same time, public housing potentially displaces high concentrations of poor individuals into more affluent areas. An influx of lower-income individuals into higher-income neighborhoods would result in an increased sense of inequality and decreased social order, increasing crime. These conflicting mechanisms make it difficult to predict the results of Fagan and Davies's study.

Through their study Fagan and Davies find that the rate of public housing in a census tract area was significantly correlated with rape, robbery, assault, and murder, controlling for fundamental demographic differences between regions. However, a limitation of their research is that it is unclear whether public housing is a crime generator or whether higher concentrations of poor individuals create more opportunities for robbery and homicide. Further complicating the findings, the paper finds that stop-and-frisks happen more frequently within poorer and minority neighborhoods. Changes in stop-and-frisk rates within census tracts would result in biased results. As more people are stopped within a census tract, the likelihood of being convicted of a crime simultaneously increases. Therefore, the changes in crime rates may not be because of an actual increase in crimes being committed but rather because of increased policing.

Freedman and Owens (2011) find conflicting results to the Fagan and Davies study. They found that increases in the low-income housing stock are associated with crime reduction. They used variations in tax credits to real estate developers generated by changes in Department of Housing and Urban Development (HUD) program rules as an exogenous source of variation in low-income housing development. They used this data to conduct a quasi-experimental study. Freedman and Owens found that increases in low-income housing were associated with reduced robbery and assaults at the county level. This study suggests that public investment in private, affordable housing can reduce crime, but the mechanisms by which this occurs are unclear.

Freedman and Owens cite that a limitation of their paper is that they focus on county-level crime. No national dataset contains crime at the census tract level, and crime data at a more micro level is only available for a select few cities. By aggregating crime to the county level, the dependent variable contains crimes occurring in wealthier areas that may bias the results. The impacts of low-income housing on people's behavior are highly localized, as crime-reducing effects of "local amenities have been shown to dissipate rapidly over space" (Freedman and Owens, 2011, 16). Localized impacts indicate that the housing stock available to low-income individuals may reduce crime in particular census tracts and nowhere else, suggesting that using smaller units of analysis may be more useful in identifying casual effects.

Freedman and Owens's discussion regarding the limitations of utilizing data aggregated to the county level motivates the next steps of my analysis. Rather than aggregating crime to the county level, I intend to look at city data that includes more micro-level data on crime, specifically at the census tract level. Looking at microdata is also supported by the findings of

Glaser and Sacredote (1999), who investigated several causal links between cities and crime. One causal mechanism they find is the "opportunity hypothesis." This hypothesis suggests that high population density implies that urban criminals do not have to travel far to steal valuable items. Therefore, if QCTs draw low-income housing and crime-prone residents away from weather areas, any observed reduction in the county-level analysis may be driven by decreased crime in non-QCT areas.

Conversely, if LIHTC developments displace the most criminal-prone from higher-income areas, this may reduce crime in wealthier neighborhoods while increasing it in QCTs. Therefore, by not aggregating QCTs to the county level and looking at changes in crime at the census tract level, I will eliminate the bias introduced by geographic distribution to distinguish better between these alternative mechanisms. While there is only such micro-level data for select cities, comparing the results across cities within the US will provide greater insight into the impacts of low-income housing policy by helping eliminate bias introduced by geographic distribution.

Woo conducts a similar study examining how the Low-Income Housing Tax Credit (LIHTC) program impacts neighborhood crime rates. He estimated the levels and trends in neighborhood crime before and after LIHTC developments based on crime incidents from 2000 to 2009 in Austin, Texas, using the Adjusted Interrupted Time Series-Difference in Difference approach. The study found that LIHTC subsidized housing tended to be developed in neighborhoods that previously had high crime rates. He further found that LIHTC developments decreased neighborhood crime. This paper takes a more microgeographic approach to eliminate bias introduced through changes in the geographic distribution of crime, as Glaser, Sacredote, Freedman, and Owens discussed.

A limitation is that the study cannot control for alternative explanations for decreases in crime. When there are redevelopments through LIHTC within a census tract, various other revitalization programs are often implemented. The simultaneous implementation suggests that if the start of LIHTC developments coincides with other revitalization programs within a neighborhood, it is plausible that these other programs explain changes in neighborhood crime rates. To address the endogeneity introduced by additional revitalization programs, it is advisable in future research to control for other rejuvenation programs, not just the LIHTC program. Additionally, this study only examines LIHTC developments in one city. Only looking at one city makes it difficult to assess the external validity of these findings. Despite these limitations, LIHTC's relationship with crime has tremendous policy implications, suggesting that LIHTC developments may effectively revitalize distressed neighborhoods and reduce neighborhood crime.

Another form of affordable housing policy is subsidizing housing. Lens (2013) examined changes in crime rates in response to changes in rates of subsidized housing. He used panel data on over 200 US cities using city-fixed effects to control for unobserved differences among cities that may lead to endogeneity. The paper focused on the Housing Choice Voucher Program and FBI county and city-level crime data. Lens found that vouchers have a weak negative relationship with violent crime rates in cities, although these estimates are not particularly robust. In suburban areas, there was no observed relationship between housing vouchers and crime. Further, Lens found no association between housing vouchers and property crime. Concluding that there does not appear to be a relationship between vouchers and crime in US cities and suburbs at a robust level. This study has vast policy implications and significant implications for public discourse. This study implies that the argument that voucher programs bring crime to neighborhoods is unfounded, and as such, they should be met with less resistance.

As seen through this literature review, there exists no consensus amongst econometrics studies or politicians on the true impact of low-income housing on crime rates. Like Fagan and Davis, many studies have found that low-income housing increases crime rates, supporting the discourse by many that they do not want low-income housing in their neighborhood. Many others, like Woo, Freedman, and Owens, have found that low-income housing decreases crime rates. Implying that low-income housing policy may be an effective tool in helping reduce neighborhood crime. Finally, others like Lens have found that there is no correlation. Suggesting that " not in my backyard" arguments are unfounded. Conflicting theories in econometrics literature need to be addressed, and my paper plans to clarify the relationship between low-income housing policy and crime rates.

Data Discussion

This analysis utilizes three-panel datasets: two spanning the years 2007, 2010, 2013, and 2016, and one encompassing all years between 2005 and 2016. Within the panel data, the dependent variable of interest is crime incidence. Specifically, in panel A, the total number of violent and property crimes committed within a county per year is used. This data was collected from the Uniform Crime Reporting (UCR) Program, which provides reliable statistics for use in law enforcement. Within this data set, *violent crimes* are defined as murder, rape, robberies, and aggravated assaults. *Property crimes* include burglary, larceny, motor vehicle theft, and arson. The total number of violent and property crimes within a county is the sum of these various sub-crime categories.

In panel B, the dependent variable represents the count of crimes occurring within a census tract in Chicago. This data was collected from the Chicago Police Department's CLEAR (Citizen Law Enforcement Analysis and Reporting) system and reported by the City of Chicago Open Data. This data set contained the longitudes and latitudes of where crimes were committed, and using Census tract shape files from the Census, one could match each longitude and latitude to a particular tract. Within this data set, *violent crimes* are defined as assault, battery, sexual assault and offense, kidnapping, human trafficking, and homicide. *Property crimes* are arson, burglary, criminal damage, motor vehicle theft, and robbery. *Other crimes* are defined as concealed carry license violation, trespassing, deceptive practice, gambling, intimidation, interference with a public officer, narcotics, obscenity, offense involving children, narcotic violations, prostitution, disturbing the peace, ritualism, stalking, and weapons violation. The total number of violent, property, and other crimes committed within a census tract in a year is the sum of these sub-crime categories.

The primary independent variable of interest in panels A and B is QCT status. The United States Department of Housing and Urban Development (HUD) provides yearly data on whether or not a census tract is a Low-Income Housing Tax Credit Qualified Census Tract (QCT). To qualify for QCT status, a tract must have 50 percent of households with incomes below 60 percent of the Area Median Gross Income (AMGI) or a poverty rate of 25 percent or more. QCT status is represented by a dummy variable: zero if the tract does not qualify and one if the tract does qualify. This data is at the census tract level and thus aggregates at the county level for panel A. The aggregated variable is continuous and represents the number of census tracts within a county qualified for QCT status. The independent variable of interest in panel C is the number of newly allocated LIHTC units. The United States Department of Housing and Urban

Development (HUD) provides yearly data on where LIHTC units are built and how many are built. The number of new units allocated in each year is used within the panel.

Additionally, income is included in panels A and B. The income variable in panel A is per capita income in dollars, collected by the Bureau of Economic Analysis. Specifically, this data is from the Local Area Personal Income data set. It is calculated by taking the personal income of a given area and dividing it by the resident population of the area to determine the per capita income in dollars. For the census tract level data in Chicago (panel B), income is not directly controlled; instead, the poverty rate is controlled. This data is collected by the HUD and provided in the Qualified Census Tracts data set. Additionally, the population is included in all the panel data sets (A, B, and C) and was collected from the Census.

The data regarding these three variables (QCT status, income or poverty rate, population, and crime rates) is then merged to create three comprehensive panel data sets. After creating the county-level data set, several counties were removed due to missing observations. Specifically, counties in Puerto Rico, Alaska, and several other smaller counties were removed because they were missing either crime or QCT data. After creating the Census tract-level data sets, several observations were dropped as well, which can be explained by the changing boundaries of census tracts across years.

Intriguing suggestive evidence emerges when looking at patterns throughout the data. When considering macro trends, it is suggested that between 2005 and 2016, crime incidence was on a relatively stable decline across the US; it also appears that the number of QCTs was relatively stable over the period. When looking at the mean number of QCTs per county vs. the mean number of crimes committed at first glance, there appears to be a substantial positive correlation—suggesting higher crime rates in counties with more QCTs (*Figure 2*). Conversely, when looking at the total number of crimes committed within a tract by QCT status, it appears that the average crime rate is very similar amongst QCTs and non-QCTs and that there are more outliers in tracts that did not qualify for QCT status (*Figure 3*). Another intriguing trend is revealed when looking at the total number of crimes committed within a tract vs. the number of low-income housing units in that tract (*Figure 5*). This graph shows a negative correlation between crime incidence and the number of low-income housing units. This correlation suggests that low-income housing units are built in larger quantities in places with previously low crime levels. Such conflicting suggestive evidence makes this a fascinating question to try and investigate.

Panel A: County-Level Crime Rates

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max
# of Tracts with QCT Status	12,549	3.723	18.07	0	563
# of Violent Crimes	12,549	155.0	786.1	0	35,692
# of Property Crimes	12,549	444.0	1,458	0	43,923
Per Capita Income (dollars)	12,328	36,198	10,599	14,363	199,635
County Population	12,549	99,722	318,862	69	1.012e+07
Violent Crime Rate	12,549	0.00115	0.00105	0	0.0132
Property Crime Rate	12,549	0.00362	0.00291	0	0.0478

Panel B: Chicago Census Tract Level Crime Rates

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max
# of Other Crimes	2,892	284.6	251.4	1	3,053
# of Property Crimes	2,879	180.9	148.5	1	2,080
# of Violent Crimes	2,845	98.41	94.21	1	715
Poverty Rate	2,892	0.201	0.153	0	1
QCT	2,892	0.411	0.492	0	1
Tract Population	2,892	3,464	1,812	135	19,015

Panel C: Chicago Census Tract Level Crime Rates and Number of New Allocated Units

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max
# of New Units	11,835	3.111	24.13	0	789
# of Other Crimes	11,835	271.0	279.4	0	3,609
# of Property Crimes	11,835	170.3	166.9	0	2,478
# of Violent Crimes	11,835	90.55	100.7	0	780
Tract Population	10,127	3,526	1,810	73	28,192
QCT Status	11,835	0.342	0.474	0	1

Figure 2: Total Number of Crimes Committed within a County and QCT Status

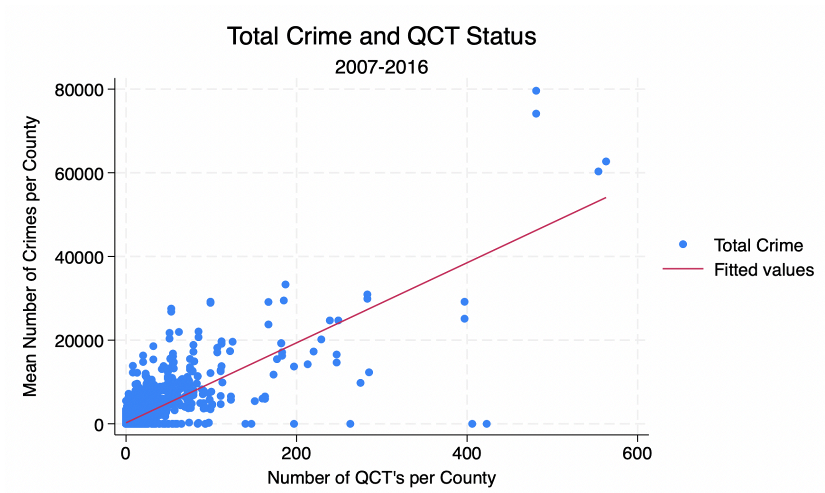


Figure 3: Total Number of Crimes Committed within a Tract and QCT Status

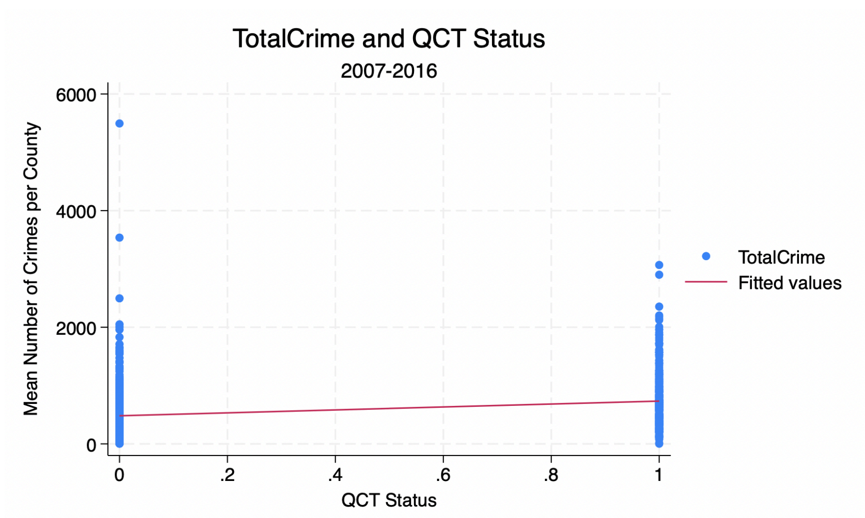


Figure 4: Total Crimes by Year in Chicago

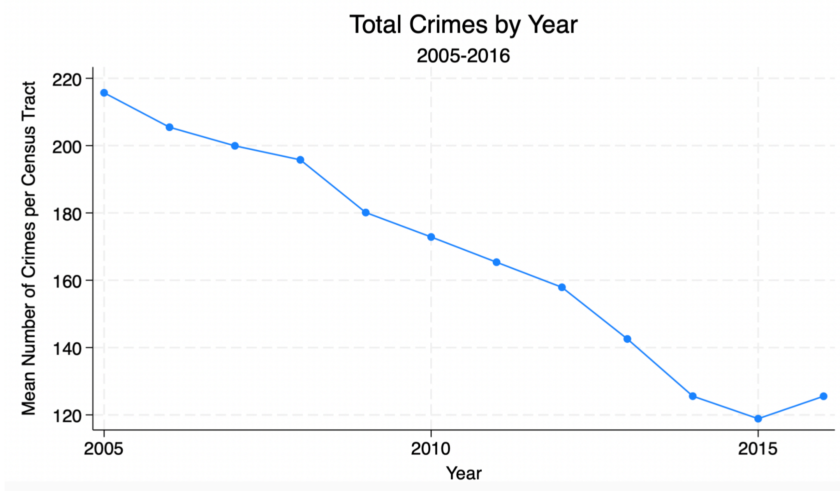


Figure 5: Total Number of Crimes in Census Tracts and the Number of Affordable Housing Units

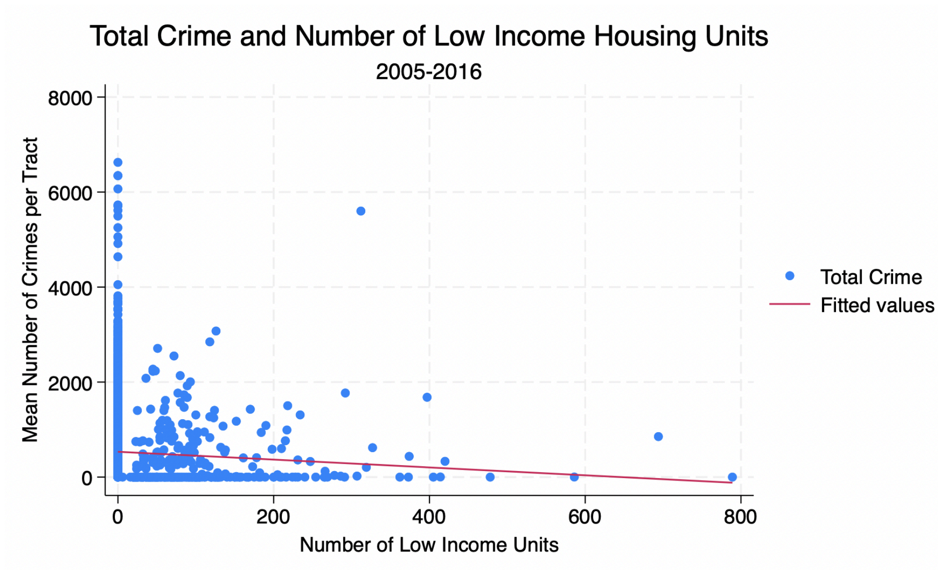
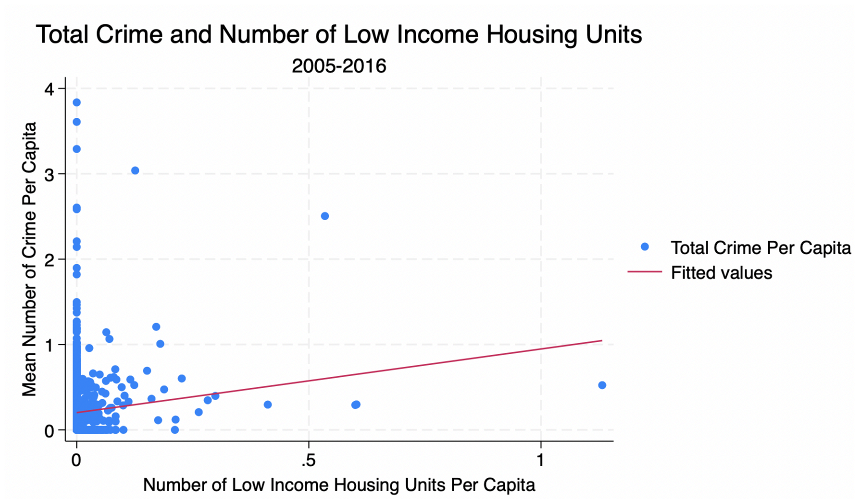


Figure 6: Total Number of Crimes Per Capita vs. Crime Rate Per Capita



Methodology

The empirical strategy utilizes a multivariate regression with county and time-fixed effects. The equation for the regression is as follows:

$$Crime_{it} = QCT_{it} + \beta\ X_{it} + \alpha_i + \ \mu_t + \epsilon_{it} \quad \textbf{\textit{(1)}}$$

Within this equation, (i) is the county-state, (t) is the year, and (x) is per capita income.

The analysis commenced with a straightforward linear regression of crime rates on the number of qualified census tracts within a county. Subsequently, a multivariate regression was conducted, incorporating income as a crucial crime determinant. This multivariate regression played a pivotal role in controlling for endogeneity within the model, as evidenced by the significant decrease in the coefficients associated with violent and property crime. However, despite this control, residual bias persisted due to the omission of time and unit fixed effects. Given the inherent systematic differences across years and counties, this omission could potentially bias the previous coefficient estimates.

Recognizing the potential biases introduced by not controlling for time and unit-fixed effects, we employed a new model that included county and time-fixed effects. A fixed-effect model is appropriate for running a regression on this data as it was a panel data set. The time-fixed effect, for instance, controls for time-specific factors that affect all of the units simultaneously, such as a broad macroeconomic trend like the great depression in 2008. The county-fixed effect, on the other hand, controls for county-specific factors constant over time, like population density. This comprehensive approach, including fixed effects, not only accounted for unobserved factors that vary across time and counties but also helped address potential endogeneity issues introduced by omitted variables.

Based on running this regression and comparing it to the multivariate linear model, not controlling for county and year-fixed effects introduces considerable omitted variable bias into the model. When not controlling for fixed effects, the multivariate and simple regressions imply that QCT status impacts crime positively and statistically significantly. In contrast, the fixed effects model suggests its impact is negative and statistically significant. Changes in the coefficient associated with QCT indicate that unobservable variables within the error term resulted in systematically too high coefficient estimates in the multivariate regression; however, by controlling for county and year fixed effects, we eliminate the positive omitted variable bias.

Despite the fixed effects model addressing some endogeneity concerns, an argument remains that a linear regression model is not the proper specification. The argument could be made that the relationship between the number of census tracts that qualify for QCT status and crime rates is nonlinear. For instance, if 50 census tracts within a county qualify, a one-unit increase would matter less than for a county going from 0 to 1 QCTs. A regression incorporating the squared term of QCT status is run to test this nonlinear hypothesis. In running this regression, it is found that the squared term is non-statistically significant. Based on this finding, the null hypothesis that QCT status and crime rates have a nonlinear relationship is rejected.

While the fixed effects model addressed some endogeneity concerns, a significant limitation remained. County-level education levels, a crucial determinant of crime rates, remain not explicitly controlled for. While effective in controlling for county-specific factors that are constant over time, the county-fixed effect may not capture the yearly changes in education levels. If this is the case, then the county fixed effect would not pick up this factor, and as such, education could bias our results. Education is undoubtedly correlated with crime rates and is likely associated with QCT status. Less educated people are more likely to live in lower-income areas, which are more likely to qualify for QCT status. Explicitly controlling for education levels is especially important as within the literature regarding crime determinants, it is found that the two most prominent crime determinants are education and income. So, while income is explicitly controlled for, not controlling for education could bias the coefficients.

Another limitation of the model is that it looks at county-level crime rates. To address this concern, I looked specifically at data from the Chicago census tracts. I used a similar model to that above. The equation for the regression is as follows:

$$Crime_{it} = QCT_{it} + \beta\ X_{it} + \alpha_i + \mu_t + \epsilon_{it} \quad \textbf{\textit{(2)}}$$

Within this equation, (i) represents the census tract, (t) denotes the year, and (x) signifies the poverty rate.

A census tract-level approach will allow for a more believable and thorough analysis because it will help eliminate the bias introduced by crime displacement within counties. Ruling out the possibility that changes in QCT status move crime from one neighborhood to the next helps better identify the causal mechanisms through which affordable housing impacts the perpetuation of crime. This analysis was critical to investigate as these findings are more relevant to public discourse and policy reform.

A notable constraint of the model mentioned above lies in its potential difficulty asserting external validity. It is plausible to infer that systematic variances exist among different US cities. Consequently, the influence of low-income housing on crime rates in one city may significantly differ from that in another. To address this concern, one could juxtapose the findings in Chicago with those of other cities across the United States. Doing so would allow one to make more definitive assertions regarding its implications and validity. Incorporating data from multiple cities could enhance this examination and validate its external applicability. This endeavor, while exceeding the current scope of the analysis, holds great promise for future studies.

I additionally ran a similar analysis to investigate how the number of allocated low-income housing units within a census tract impacts crime incidence. The equation is as follows:

$$\Delta Crime_{it} = NewAllocatedLIHTCUnits_{it} + \beta_{Pop} Population_{it} + \alpha_i + \mu_t + \epsilon_{it} \quad \textbf{\textit{(3)}}$$

Within this equation, (i) is the census tract, (t) is the year. In this model, I calculate the changes in crime incidence between years and the number of newly allocated units in each census tract. This model is essential to investigate because it is plausible that just because a census tract qualified for the LIHTC does not mean a developer took advantage of the policy and built additional low-income housing units. By looking at the number of low-income units, we can control for this possibility and see how the concentration of low-income housing impacts crime rates. Controlling the number of units allocated provides greater insight and is more relevant to public discourse, especially when considering how and where additional units should be allocated within cities.

However, a concern with this analysis is that the building of new LIHTC units may be correlated with other factors that also impact crime; because of this, I want to find a way to look at exogenous variation due to policy. A way to only look at this variation is to utilize QCT status as an instrument variable for the number of affordable housing units within a county. The empirical strategy for this follows the following equations (two-stage least squares):

$$\Delta Crime_{it} = \beta_{2SLS}\widehat{NewLIHTCUnits}_{it} + \beta_{Pop}Population_{it} + \alpha_i + \mu_t + \epsilon_{it} \quad \textbf{(4)}$$

$$\widehat{NewLIHTCUnits}_{it} = \hat{\gamma}_{QCT}QCT_{it} + \hat{\gamma}_{Pop}Population_{it} + \phi_i + \lambda_t \quad \textbf{(5)}$$

An instrumental variable strategy is appropriate due to concerns regarding reverse causality between the number of affordable housing units and crime rates. Previous research suggests that crime rates may impact the affordability of housing units, so using an instrumental variable should address this concern. QCT status is a valid instrumental variable in this case as it has a causal impact on the number of newly allocated LIHTC units (as shown through approximating the first reduced form equation), impacts crime rates only through increasing or decreasing the number of affordable housing units, and there is no confounding for the effect of Z on Y. This strategy zeroes in on the central goal of the paper: to comprehensively examine not only the effects of the policy itself but also the tangible impacts of constructing LIHTC units on crime rates.

Results and Discussion

Beginning with the county-level analysis, the multivariate OLS regression (*Figure 7*) found that an additional census tract qualifying for QCT status is associated with a 3.82 percent increase in violent crimes committed within a county and a 3.71 percent increase in property crimes. All of these coefficients are statistically significant at a 1% significance level. These results suggest that low-income housing policies within counties are associated with increased crime. However, these results are only informative when controlling for the population and demographics of the counties; a fixed effects model can be used to control for such variables.

In the fixed effects model (*Figure 8*), it is discovered that an additional census tract qualifying for QCT status is associated with a -7.15e-06 point decrease in violent crime rates and a -2.23e-05 point decrease in property crime rates, holding all else constant. While these coefficients are statistically significant at a 5% and 1% level, their small magnitude makes it challenging to interpret the findings. The specifications in columns 1 and 2 of *Figure 8* are more informative. These specifications find that an additional census tract qualifying for QCT status is associated with a .248 percent decrease in violent crime and a .342 percent decrease in property crime, holding all else constant. These coefficients are statistically significant at a 10% and 1% level. Considering the mean number of property crimes committed within a county in a given year is 443.8, and the mean number of violent crimes is 154.9, a .248% and a .342% reduction is quantitatively meaningful. These results suggest that low-income housing policy in counties reduces property crime and violent crime.

Another intriguing finding of the regression in *Figure 8* is the complex relationship between per capita income and crime rates. In this model, a 1% increase in per capita income is associated with a 0.388% increase in violent crimes and a 0.14% decrease in property crimes, holding all else constant. Only the coefficient associated with violent crime rates is statistically significant at a 1% level. These findings underscore the need for further investigation into the underlying mechanisms behind the relationship between crime and economic status. The logged population also has a statistically significant relationship with crime—a 1% increase in population is associated with a 0.703% increase in violent crimes and a 0.774% increase in property crimes. These findings align with expectations, as counties with larger populations would naturally have more crime.

Significant differences are found when comparing the fixed effects estimates (*Figure 8*) to the OLS estimates (*Figure 7*). This comparison indicates that not controlling for county and year introduces significant omitted variable bias into the model. Using the fixed effects model helps eliminate endogeneity caused by constant unobservables within a county and year-specific across all counties. Furthermore, significant differences exist between the fixed effects model and OLS's fitness. The R-squared value in the fixed effects model increases significantly to 0.945 and 0.962 from 0.178 and 0.133 in the multivariate regression. These values signify that the fixed effects model represents 94.5% of the variance in violent crime rates and 96.2% in property crime rates. Higher R-squared values indicate that the fixed effects model explains much more of the variance in crime. The increased R-squared value makes sense, as controlling for additional variables that impact crime would innately lead to the model explaining more of its variance. A different fixed-effects regression is run to check the hypothesis that QCT and crime rates have a nonlinear relationship. Within this model (*Figure 9*), the coefficients associated with the squared terms are not statistically significant, allowing us to reject the null hypothesis that QCT and crime rates have a nonlinear relationship.

Moving into the case study analysis of Chicago, the multivariate regression (*Figure 10*) found that a census tract qualifying for QCT status was associated with a 58.3% increase in violent crimes committed compared to a census tract that did not qualify. It was also found that QCT status was associated with an 18.6% increase in property crimes and a 27% increase in other crimes compared to those without QCT status. These results are all statistically significant at a 1% significance level. These results suggest that low-income housing policies within census tracts are associated with increased crime. However, these results are only that informative when controlling for systematic differences across tracts and between years. A fixed effects model can be used to control for such variables.

In the fixed effects model (*Figure 11*), it is found that a census tract qualifying for QCT status was associated with a 2.14% increase in violent crimes committed compared to a census tract that did not qualify. It was also found that QCT status was associated with a 2.33% decrease in property crimes and a 0.828% decrease in other crimes compared to those without QCT status. All of these estimates are not statistically significant at any reasonable level. These findings suggest that the presence of a low-income housing policy does not impact the incidence of crime in the surrounding areas—such results conflict with the county-level findings, which indicated that county-level crime rates decreased.

Similar to the county-level analysis, significant differences exist between the fixed effects and multivariate OLS estimates. This comparison indicates that not controlling for census tract and year introduces significant omitted variable bias into the model. Using the fixed effects model helps eliminate endogeneity caused by constant unobservables within a census tract and year-specific across all tracts. Furthermore, significant differences exist between the fixed effects model's and OLS's fitness. The R-squared value in the fixed effects model increases significantly to 0.967, 0.969, and 0.978 from 0.321, 0.119, and 0.158. These values mean that the fixed effects model represents 96.7% of the variance in violent crime rates, 96.9% in property crime rates, and 97.8% in other crimes. These higher R-squared values indicate that the fixed effects model represents much more of the variance in crime.

Moving into the analysis of how the number of newly allocated LIHTC units impacts crime, the fixed effects model (*Figure 1*3) found that 1,000 newly allocated units within a census tract are associated with a 15.3% decrease in other crimes committed, holding all else constant. It was also found that 1,000 newly allocated units within a census tract are associated with a 10.3% decrease in violent crime and a 14.1% decrease in property crimes, holding all else constant. While these values are quantitatively significant, these estimates are not statistically significant at any reasonable level. These findings again suggest that a low-income housing policy does not impact the incidence of crime in the surrounding areas. Once again, this conflicts with the county-level findings, which suggested that county-level crime rates decreased but does support the findings of how QCT status impacted crime in Chicago.

A different fixed-effects regression is run to check the hypothesis that the number of new low-income housing units and crime rates have a nonlinear relationship. It is reasonable to assume there may be a threshold after which the number of low-income housing units will begin to increase crime incidence. Within this model, we find that the coefficients associated with the squared terms are not statistically significant, allowing us to reject the null hypothesis that the number of low-income housing units and crime rates have a nonlinear relationship.

Building new LIHTC units may correlate with other crime-related factors. As such, using an instrumental variable and 2sls can correct for this possibility. However, in running the reduced form equation, it is found that when restricting the sample to Chicago only, QCT has a negative but statistically significant relationship with the number of new LIHTC units allocated within a census tract. One reason may be that QTC status is negatively correlated with the unobserved determinants of LIHTC building in the equation. For example, time-varying factors like increasing poverty may push tracts into QCT status but also decrease the attractiveness of developing new infrastructure in that area. Given the reduced form results, the second-stage results are not informative.

After completing this primary analysis, additional specifications and models are used to address several concerns and check for robustness. One concern with using the fixed-effects model is justifying that there is enough variation in the QTC status. This concern arises because these fixed-effect models control for individual-specific effects and time-invariant unobservable heterogeneity. If there is not enough variation in the QTC status, the model is essentially picking up all the relevant variations, rendering the estimated effects unreliable and potentially biased. However, the provided statistics offer reassurance that there is enough variation in the QTC status across different panels. For panel A, it is found that 53.83% of the counties experienced changes in the number of qualified census tracts over the period. This value suggests that almost half the counties in the United States have experienced changes in their QTC, indicating substantial variation across years. Additionally, the standard deviation for the number of QTC in a county is 18.07, suggesting that there is also considerable variation across counties. For panel B, 17.82% of the tracts experienced a change in QTC status over the period, and for panel C, 31.13% experienced at least one change in QTC status over the period. These figures indicate that enough tracts have experienced changes in their QTC, highlighting high enough levels of variation to justify using the fixed-effects model.

Another concern with these models is that the natural logarithm of the number of crimes committed is used. While taking the natural log helps address skewness and heteroskedasticity, it excludes observations with zero counts because the natural log of zero is undefined. This exclusion can lead to concerns about bias or loss of information, mainly if a large portion of the data consists of zeros. A robustness check utilizing the transformation *ln(crime+1)* instead of *ln(crime)*can be used to mitigate this concern. Adding 1 to the count before taking the log ensures that observations with zero crime are retained in the analysis. In running the same regressions utilizing this new transformation, consistent results are found. Consistent results in the robustness check suggest that the dropping of census tracts and counties with a zero value for crime incidence was not that prevalent.

An additional robustness check is utilized, which includes lagged effects. Incorporating lagged effects into the analysis aims to capture the delayed impact of low-income housing on crime rates. It is reasonable to assume that the building of new units or that qualifying for QCT status may not lead to immediate changes in crime incidence but could manifest in subsequent periods. Therefore, by including lagged impacts in the regressions, the analysis accounts for the potential time delay between the causal factors and their effects on crime rates. In these regressions, the coefficients are consistent with those previously found, reinforcing the validity of the chosen specification and providing confidence in the results' robustness.

Figure 7: Multivariate linear regression (County data from panel A)

VARIABLES	(1) Logged Violent Crime	(2) Logged Property Crime	(3) Violent Crime Rate	(4) Property Crime Rate
# of QCT's	0.0382***	0.0371***	9.53e-06***	1.63e-05***
	(0.000853)	(0.000961)	(5.15e-07)	(1.36e-06)
Logged Per Capita Income (dollars)	0.774***	0.658***	-0.000430***	-0.000885***
	(0.0612)	(0.0681)	(3.71e-05)	(9.78e-05)
Constant	-4.827***	-2.497***	0.00561***	0.0128***
	(0.640)	(0.712)	(0.000388)	(0.00102)
Observations	10,969	11,186	12,328	12,328
R-squared	0.178	0.133	0.034	0.016

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 8: Multivariate Fixed Effects Regression (County data from panel A)

VARIABLES	(1) Logged Violent Crime	(2) Logged Property Crime	(3) Violent Crime Rate	(4) Property Crime Rate
# of QCT's	-0.00248*	-0.00342***	-7.15e-06**	-2.23e-05***
	(0.00127)	(0.00106)	(2.87e-06)	(5.87e-06)
Logged Per Capita Income (dollars)	0.388***	-0.140	0.000304	0.000619
	(0.104)	(0.102)	(0.000200)	(0.000403)
Logged Population	0.703***	0.774***		
	(0.209)	(0.174)		
Constant	-7.782***	-1.960	-0.00185	-0.00260
	(2.639)	(2.286)	(0.00207)	(0.00416)
Observations	10,908	11,141	12,320	12,320
R-squared	0.945	0.962	0.758	0.831
FE	x	x	x	x

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 9: Non-linear fixed effects regression (County data from panel A)

VARIABLES	(1) Logged Violent Crime	(2) Logged Property Crime	(3) Violent Crime Rate	(4) Property Crime Rate
# of QCT's	-0.00363**	-0.00419***	-8.66e-06**	-3.00e-05***
	(0.00184)	(0.00157)	(3.64e-06)	(7.60e-06)
# of QCT's Squared	4.21e-06	2.83e-06	5.81e-09	2.98e-08*
	(3.74e-06)	(2.37e-06)	(7.88e-09)	(1.58e-08)
Logged Per Capita Income (dollars)	0.386***	-0.141	0.000301	0.000604
	(0.104)	(0.102)	(0.000201)	(0.000404)
Logged Population	0.711***	0.780***		
	(0.210)	(0.175)		
Constant	-7.847***	-2.002	-0.00182	-0.00242
	(2.644)	(2.289)	(0.00207)	(0.00417)
Observations	10,908	11,141	12,320	12,320
R-squared	0.945	0.962	0.758	0.831
FE	x	x	x	x

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 10: Multivariate Linear Regression (Chicago data from panel B)

VARIABLES	(1) Logged Violent Crime	(2) Logged Property Crime	(3) Logged Other Crime	(4) Violent Crime Rate	(5) Property Crime Rate	(6) Other Crime Rate
QCT	0.583***	0.186***	0.270***	0.0165***	0.00792***	0.0224***
	(0.0572)	(0.0602)	(0.0635)	(0.00160)	(0.00241)	(0.00426)
Poverty Rate	2.846***	1.895***	2.397***	0.0995***	0.104***	0.224***
	(0.186)	(0.196)	(0.207)	(0.00515)	(0.00773)	(0.0137)
Logged Population	0.556***	0.437***	0.429***			
	(0.0341)	(0.0359)	(0.0379)			
Constant	-1.185***	0.872***	1.194***	0.00790***	0.0367***	0.0434***
	(0.282)	(0.297)	(0.313)	(0.000850)	(0.00127)	(0.00223)
Observations	2,845	2,879	2,892	2,845	2,879	2,892
R-squared	0.321	0.119	0.158	0.403	0.181	0.267

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 11: Fixed Effects Regression (Chicago data from panel B)

VARIABLES	(1) Logged Violent Crime	(2) Logged Property Crime	(3) Logged Other Crime	(4) Violent Crime Rate	(5) Property Crime Rate	(6) Other Crime Rate
QCT	0.0214	-0.0233	-0.00828	0.00108	-0.00183	-0.000962
	(0.0225)	(0.0203)	(0.0176)	(0.00100)	(0.00177)	(0.00275)
Logged Population	0.412***	0.288***	0.308***			
	(0.0764)	(0.0635)	(0.0650)			
Poverty rate	0.0161	-0.0179	-0.0278	0.00586	0.0105	0.0153
	(0.0833)	(0.0693)	(0.0675)	(0.00559)	(0.00742)	(0.0148)
Constant	1.066*	2.794***	3.059***	0.0433***	0.0750***	0.125***
	(0.613)	(0.510)	(0.525)	(0.000987)	(0.00138)	(0.00258)
Observations	2,836	2,872	2,886	2,836	2,872	2,886
R-squared	0.967	0.969	0.978	0.932	0.915	0.897
FE	x	x	x	x	x	x

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 12: Multivariate OLS Regression (Chicago data from panel C)

VARIABLES	(1) Δ Logged Other Crime	(2) Δ Logged Violent Crime	(3) Δ Logged Property Crime	(4) Δ Other Crime Rate	(5) Δ Violent Crime Rate	(6) Δ Property Crime Rate
# of New Units (thousands)	-0.127	-0.121	-0.0256	-0.169***	-0.0391***	-0.0305***
	(0.147)	(0.181)	(0.162)	(0.0276)	(0.00690)	(0.0114)
Logged Tract Population	0.00857**	0.00876	-0.00145			
	(0.00435)	(0.00539)	(0.00481)			
Constant	-0.118***	-0.110**	-0.0322	-0.00537***	-0.00143***	-0.00250***
	(0.0350)	(0.0433)	(0.0386)	(0.000447)	(0.000112)	(0.000185)
Observations	9,037	8,863	8,993	8,812	8,812	8,812
R-squared	0.001	0.000	0.000	0.004	0.004	0.001

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 13: Fixed Effects Regression (Chicago data from panel C)

VARIABLES	(1) Δ Logged Other Crime	(2) Δ Logged Violent Crime	(3) Δ Logged Property Crime	(4) Δ Other Crime Rate	(5) Δ Violent Crime Rate	(6) Δ Property Crime Rate
# of New Units (thousands)	-0.153	-0.103	-0.141	-0.140	-0.0302	-0.0268
	(0.145)	(0.128)	(0.144)	(0.129)	(0.0188)	(0.0205)
Logged Tract Population	0.112***	0.0772**	0.0487***			
	(0.0299)	(0.0388)	(0.0175)			
Constant	-0.906***	-0.649**	-0.394***	0.00310	-0.000105	0.00488***
	(0.241)	(0.312)	(0.140)	(0.00197)	(0.000589)	(0.000692)
Observations	9,031	8,859	8,990	8,805	8,805	8,805
R-squared	0.069	0.052	0.089	0.110	0.105	0.125
FE	x	x	x	x	x	x

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 13b: Fixed Effects Regression with no population control (Chicago data from panel C)

VARIABLES	(1) Δ Logged Other Crime	(2) Δ Logged Violent Crime	(3) Δ Logged Property Crime	(4) Δ Other Crime Rate	(5) Δ Violent Crime Rate	(6) Δ Property Crime Rate
# of New Units over Population	-0.388	-0.532*	-0.257	-0.367	-0.106	-0.0747*
	(0.260)	(0.322)	(0.232)	(0.367)	(0.0665)	(0.0403)
Constant	-0.00727	-0.0296***	-0.00308	0.00348	2.78e-05	0.00496***
	(0.00716)	(0.00861)	(0.00706)	(0.00220)	(0.000597)	(0.000696)
Observations	9,031	8,859	8,990	8,805	8,805	8,805
R-squared	0.067	0.052	0.088	0.117	0.115	0.126
FE	x	x	x	x	x	x

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 14: First Stage OLS Regression (Chicago data from panel C)

VARIABLES	(1) # of New Units (thousands)	(2) # of New Units Over Population
QCT	-0.00255***	-0.00271
	(0.000946)	(0.00165)
Logged Population	-0.00925*	
	(0.00542)	
Constant	0.0784*	0.00387**
	(0.0438)	(0.00177)
Observations	9,766	9,766
R-squared	0.287	0.172
FE	x	x

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Conclusions

This study, which builds upon existing literature, delves into the impact of low-income housing policy, specifically Qualified Census Tract (QCT) designation, on crime rates. The investigation is conducted in three phases: firstly, QCT status is analyzed at the county level, utilizing FBI crime data from 2007, 2010, 2013, and 2016. Secondly, QCTs within Chicago are examined alongside Chicago Uniform Crime Data for the same years. Thirdly, using the number of newly allocated LIHTC units within census tracts, alongside changes in the number of crimes committed within a census tract from 2005 to 2016. When employing a fixed effects model at the county level, the research reveals a negative and statistically significant association between QCT status and crime, both property and violent. These findings imply that low-income housing policies might effectively rejuvenate distressed neighborhoods and mitigate neighborhood crime.

Conversely, when using the same fixed effects model but looking at census tract data from Chicago, the study finds no statistically significant relationship between QCT status and violent crime, property crime, and other crimes. These findings suggest that while low-income policy does not reduce crime within neighborhoods, it does not increase either. Similarly, when using the same fixed effects model but looking at newly allocated LIHTC units and changes in crime incidence, the study finds no statistically significant relationship. Again, this suggests that while low-income policy does not lower crime within neighborhoods, it does not increase either. These noteworthy findings contribute to the ongoing discourse on low-income housing and its impact on crime rates.

A constraint of the three main models is that they do not account for education levels at the county level. Although the county-fixed effect and the census tract-fixed effect address factors specific to each county and census tract that remain constant over time, education levels may fluctuate significantly annually. In such instances, the fixed effects fail to capture these variations, potentially biasing our results due to the correlation between education and crime rates and its likely association with QCT status. Given that less educated individuals are more inclined to reside in lower-income areas and more prone to qualify for QCT status, explicit control over education levels becomes crucial. Controlling for this variable is particularly pertinent considering the literature on crime determinants, where education and income emerge as primary determinants. Consequently, while income is explicitly factored into the analysis, overlooking education could bias the coefficients. Another constraint is the lack of external validity of the census tract fixed effects analysis. Claims regarding external validity can only be made if additional cities are analyzed and their results are compared.

Despite the limitations, this analysis presents various policy implications. The findings suggest that at the county level, there is a negative correlation between low-income housing and property crime, implying that low-income housing policy can effectively rejuvenate distressed neighborhoods and mitigate neighborhood crime. This finding challenges the assertions of opponents who argue that low-income housing exacerbates crime rates. These findings could help sway public discourse and allow future policies to be more easily passed. These findings additionally reveal the potential role of low-income housing policies in crime reduction. This potential highlights the need for lawmakers, city planners, and law enforcement agencies to coordinate their efforts and consider low-income housing as a tool for crime reduction.

Furthermore, while the analysis did not find a significant relationship between QCT status and crime rates at the census tract level, this alone holds importance to public discourse. It further debunks arguments utilized by people who advocate against low-income housing developments, citing their association with increased crime. Like the county-level findings, this insight can provide policymakers with the justification for continued and potentially increased development of low-income housing, ultimately benefiting those in need.

This research opens up several avenues for further exploration. The most pressing expansion would be determining the Chicago findings' external validity. This is especially pertinent as we have seen a different (and more sensible) first stage for the larger sample that included all counties in the United States. I would begin by running a similar analysis for other cities that provide crime data at the census tract level, as the HUD provides LIHTC and QCT data at the census tract level across the United States. The biggest obstacle to this will be finding enough cities that provide detailed crime data at the census tract level, as most do not.

Another potential improvement would be considering an event study or regression discontinuity approach. For an event study, one would need to identify counties and census tracts that were 'switchers.' For an RD design, one must identify tracts just above or below the cutoff criteria. While these methods may have the potential to provide greater insight and allow for causal estimates to be drawn, there may not be enough observations that fulfill these requirements for a practical analysis to be conducted. Notwithstanding, these suggestions provide suggestions for further research that would give great insight into the causal mechanisms of the relationship between crime and low-income housing. The potential for further research in this area is vast and promising, and I hope these suggestions and findings will inspire and guide future studies.

References

1. Becker, G. S. (1968). Crime and Punishment: An Economic Approach. Journal of Political Economy, 76(2), 169–217.
2. Jeffrey Fagan and Garth Davies, Street Stops, and Broken Windows: Terry, Race, and Disorder in New York City, 28 Fordham Urb. L.J. 457 (2000).
3. John MacDonald, Community Design and Crime: The Impact of Housing and the Built Environment, Crime and Justice 2015 44: 333-383 (2015).
4. Lens, Michael C. "The Impact of Housing Vouchers on Crime in US Cities and Suburbs." http://pschousing.org/files/Lens%20-%20Impact%20of%20Housing%20Vouchers%20on%20Crime%20in%20US%20Cities%20%26%20Suburbs.pdf. (2013).
5. Matthew Freedman, Emily G. Owens, Low-income housing development and crime, Journal of Urban Economics, Volume 70, Issues 2–3 (2011).
6. "Qualified Census Tracts and Difficult Development Areas | HUD USER." HUD User, https://www.huduser.gov/portal/datasets/qct.html. Accessed 21 September 2023.
7. Sciandra M, Sanbonmatsu L, Duncan GJ, Gennetian LA, Katz LF, Kessler RC, Kling JR, Ludwig J. Long-term effects of the Moving to Opportunity residential mobility experiment on crime and delinquency. J Exp Criminol. 2013 Dec;9(4):10.1007/s11292-013-9189-9. doi: 10.1007/s11292-013-9189-9. PMID: 24348277; PMCID: PMC3858819.
8. Zenou, Yves. "The Spatial Aspects of Crime." *Journal of the European Economic Association* 1, no. 2/3 (2003): 459–67. http://www.jstor.org/stable/40005195.
9. DeLone, G. J. (2008). Public housing and the fear of crime. *Journal of Criminal Justice*, *36*(2), 115–125. https://doi.org/10.1016/j.jcrimjus.2008.02.003

10. Ayoung Woo, Kenneth Joh, Beyond anecdotal evidence: Do subsidized housing developments increase neighborhood crime? Applied Geography, Volume 64, 2015, Pages 87-96, ISSN 0143-6228, https://doi.org/10.1016/j.apgeog.2015.09.004.
11. Lens, M. C. (2013). Subsidized Housing and Crime: Theory, Mechanisms, and Evidence. *Journal of Planning Literature*, *28*(4), 352-363. https://doi.org/10.1177/0885412213500992