FURMAN CENTER FOR REAL ESTATE & URBAN POLICY
NEW YORK UNIVERSITY SCHOOL OF LAW • WAGNER SCHOOL OF PUBLIC SERVICE
139 MacDougal Street, 2nd Floor, New York, NY 10012 · Tel: (212) 998 6713 · Fax: (212) 995 4341 · www.furmancenter.org

American Murder Mystery Revisited: Do Housing Voucher Households Cause Crime?

March 2012

Ingrid Gould Ellen
Michael C. Lens
Katherine O'Regan

NYU Wagner School and
Furman Center for Real Estate & Urban Policy

Abstract: Potential neighbors often express worries that Housing Choice Voucher holders heighten crime. Yet no research systematically examines the link between the presence of voucher holders in a neighborhood and crime. Our paper aims to do just this, using longitudinal, neighborhood-level crime and voucher utilization data in 10 large U.S. cities. We test whether the presence of additional voucher holders leads to elevated crime, controlling for neighborhood fixed effects, time-varying neighborhood characteristics, and trends in the broader sub-city area in which the neighborhood is located. In brief, crime tends to be higher in census tracts with more voucher households, but that positive relationship becomes insignificant after we control for unobserved differences across census tracts and falls further when we control for trends in the broader area. We find far more evidence for an alternative causal story; voucher use in a neighborhood tends to increase in tracts that have seen increases in crime, suggesting that voucher holders tend to move into neighborhoods where crime is elevated.

In the past few decades, the Housing Choice Voucher (HCV) program has expanded significantly.[1] In 1980, about 600,000 low income households used federal rental housing vouchers to help support their rent; by 2008, that number had swelled to 2.2 million. While many in the academic and policy communities embrace the growth in tenant-based assistance, community opposition to voucher use can be fierce (Galster et al., 2003; Mempin 2011). Local groups often express concern that voucher recipients will both reduce property values and heighten crime. Hanna Rosin gave voice to the latter worries in her widely-read article, "American Murder Mystery," published in the *Atlantic Magazine* in August 2008. A 2011 opinion piece in the Wall Street Journal titled "Raising Hell in Subsidized Housing" (Bovard, 2011) echoed these concerns. Despite the continued publicity, however, there is virtually no research that systematically examines the link between the presence of voucher holders in a neighborhood and crime. Our paper aims to do just this, using longitudinal, neighborhood-level crime and voucher utilization data in 10 large U.S. cities. We use census tracts to represent neighborhoods.

The heart of the paper is a set of regression models of census tract-level crime that test whether additional voucher holders lead to elevated crime, controlling for census tract fixed effects—which capture unobserved, pre-existing differences between neighborhoods that house large numbers of voucher households and those that do not – and trends in crime in the city or broad sub-city area in which the neighborhood is located. We also control for time-varying census tract characteristics such as the extent of other subsidized housing, and in some models

[1] The HCV program began as the Section 8 existing housing program or rental certificate program in 1974. As the rental certificate program grew in popularity, Congress authorized the rental voucher program as a demonstration in 1984 and later formally authorized it as a program 1987. The rental certificate program and the rental voucher program were formally combined in the Quality Housing and Work Responsibility Act of 1998. Through conversions of rental certificate program tenancies, the HCV program completely replaced the rental certificate program in 2001. (Background information on the HCV program was condensed from U.S. Department of Housing and Urban Development, Office of Public and Indian Housing (1981), *Housing Choice Voucher Program Guidebook*. Washington, DC: U.S. Department of Housing and Urban Development, pgs 1-2 through 1-5.)

we include measures of demographic composition. (We do not include time-varying demographic attributes in all models as the voucher holders themselves might be the source of some of these changes). Finally, we also test for the possibility that voucher holders tend to settle in higher crime areas.

In brief, we find little evidence that an increase in the number of voucher holders in a tract leads to more crime. While crime tends to be higher in census tracts with more voucher households, that positive relationship disappears after we control for unobserved characteristics of the census tract and crime trends in the broader sub-city area. We do find evidence to support an alternative story, however. That is, the number of voucher holders in a neighborhood tends to increase in tracts that have seen increases in crime, suggesting that voucher holders tend to move into neighborhoods where crime is elevated.

I. Background and Prior Literature

The Housing Choice Voucher (HCV) Program provides federally funded but locally administered housing subsidies that are mobile; they permit the recipient to select and change housing units, as long as those units meet certain minimum health and safety criteria.[2] Households are generally eligible only if their income is below 50 percent of area median income (AMI). In addition, local housing authorities (HAs) are required to provide 75 percent of vouchers to households whose incomes are at or below 30 percent of AMI.[3] In addition to these income criteria, HAs may also impose additional priorities to accommodate local preferences and housing conditions. Voucher recipients receive a subsidy, the value of which depends on the

[2] Landlords also need to agree to participate and enter into contractual relationship with HUD for payment.
[3] See HUD's Housing Choice Voucher fact sheet, accessed July 20,2011. http://portal.hud.gov/hudportal/HUD?src=/program_offices/public_indian_housing/programs/hcv/about/fact_sheet

income of the household, the rent actually charged by landlords, and the rental payment standard for housing established by the local HA.[4] A voucher household whose rent is equal to or less than the local payment standard will pay no more than 30 percent of its income for gross rent.[5] Any rent above the payment standard is paid by the voucher holder. As with other affordable housing programs, demand for vouchers greatly exceeds supply in most areas, with long waiting periods for those who successfully qualify and get on HA waiting lists.[6]

Local residents often oppose the entry of subsidized housing recipients (whether through construction of subsidized rental housing or the in movement of housing voucher holders), voicing concerns that the presence of subsidized tenants will reduce property values and increase crime. While the rationale behind these fears about crime is not always well-articulated, additional voucher holders could theoretically affect crime in a neighborhood, through five different pathways.

First, if the voucher households are new to the neighborhood, they may add to the ranks of poor (or near poor) households. Economists and sociologists in particular have developed and tested robust theories on the link between poverty and crime, dating back to Becker's (1968) portrayal of the criminal as rational economic actor and Merton's (1938) sociological-based "strain theory." Both theories predict that poor individuals (e.g., those who expect to derive more income from crime than from legal pursuits – accounting for the risk of incarceration and other criminal penalties) will be more likely to commit crimes, and crime will be higher in higher poverty neighborhoods.

[4] HA payment standards are generally between 90 and 110% of HUD determined local Fair Market Rents (FMRs), but there are exceptions.
[5] Gross rent includes utilities.
[6] HAs close wait lists when the number greatly exceeds supply in the near future.

Empirically, studies have found a connection between family income and the likelihood that members of that family will be involved in criminal activity (Bjerk, 2007; Hsieh and Pugh 1993). Many studies have also found a relationship between the poverty rate in a neighborhood and the crime rate (Hannon 2002; Krivo and Peterson 1996; Stults 2010). Hsieh and Pugh (1993) provide a useful meta-analysis, summarizing much of this work.[7] In terms of what drives the association between concentrated disadvantage and violence, Sampson, Raudenbush, and Earls (1997) results suggest the mediator is collective efficacy – the social cohesion of neighborhood residents and the willingness of residents to intervene on behalf of others. Others have found that cultural characteristics of neighborhoods, including respect for police authority, mediate the relationship between poverty and crime (Kirk and Papachristos, 2011).

Given these relationships between poverty and crime, we might expect crime to rise in a neighborhood when the number of low-income voucher holders increases. Specifically, we expect crime to rise (or rise more than it would otherwise) if the voucher holders who move into a neighborhood have lower incomes than the households who would have otherwise moved in.[8] In many neighborhoods, at least in theory, voucher holders are likely to have lower incomes than other potential residents because the rent subsidy makes affordable units that would otherwise be out of their reach. However, new voucher holders will not always have lower incomes than existing and potential residents – and in fact, the subsidy provided by the voucher means that low income voucher holders are arguably less disadvantaged than unsubsidized households with the same income.[9] Thus, while an increase in voucher holders could increase the level of

[7] Concentrations of poverty beyond a certain tipping point may lead to even higher crime rates than expected given the level of poverty, due to a breakdown of social norms and reduced efficacy on the part of residents to organize against criminal elements (Galster 2005).
[8] In addition, if poverty only matters above a certain threshold, we might expect crime to increase if the number of voucher holders in a tract reaches a certain level of concentration.
[9] The market value of the voucher is not included in Census measures of income.

disadvantage in a neighborhood, it may not always do so in practice. Indeed, in lower income areas, an increase in voucher holders could potentially reduce economic disadvantage, given the positive wealth effects of housing subsidies. Moreover, local housing authorities are permitted to screen voucher applicants for criminal records and some do. Thus, if anything, voucher holders themselves are less likely to engage in criminal activity than other individuals with similar incomes who do not receive housing assistance (HUD 2001).

A second potential mechanism through which voucher holders might affect crime is via increases in a neighborhood's income diversity or income inequality, if they move into higher income neighborhoods. Several theories suggest that crime will grow with inequality, either because wealthier households and their property present targets to low-income households or because neighborhoods containing people of diverse backgrounds and limited shared experiences are likely to be characterized by greater social disorganization, which can reduce social control and lead to increases in crime (Shaw and McKay 1942).[10] Whatever the mechanism, many studies of neighborhood crime find that greater income inequality is correlated with higher levels of crime (Hipp 2007; Hsieh and Pugh 1993; Sampson and Wilson 1995). Hipp (2007), in a cross-sectional analysis of census tract-level crime data in 19 cities, argues that the significant association between poverty and crime might actually be picking up a more robust relationship between inequality and crime.

Third, a growth in the voucher population might simply increase turnover in a community, which may also lead to elevated crime as social networks and norms break down. Sampson, Raudenbush, and Earls (1997) report a strong association between residential instability and violent crime in Chicago. Researchers have also found that neighborhood

[10] In addition to being targets, higher income households may also make lower income households feel more inadequate and drive them to theft. As noted, Merton's strain theory suggests that greater inequality will pressure households to feel a need to attain recognized symbols of status and wealth.

attributes that are symptomatic of residential instability, such as white flight (Taub, Taylor, and Dunham 1984) and house sale volatility (Hipp, Tita, and Greenbaum 2009) are associated with higher crime. That said, the in-movement of voucher households can be a symptom as well as a cause of residential instability, as larger out-migration from a neighborhood opens up opportunities for voucher holders. Moreover, voucher holders are primarily protected from rent increases, and thus may be more residentially stable than other households. In this sense, additional voucher holders could lead to lower crime through enhanced community stability.

Rosin (2008) proposes a fourth mechanism, suggesting that the problem lies more specifically with housing voucher holders who have moved from demolished public housing developments, who take with them the gang and other criminal networks that they developed there. There is considerable evidence that crime rates are abnormally high in the distressed public housing developments that are typically targeted for demolition (Goering et al 2002; Hanratty, McLanahan, and Pettit 1998; Rubinowitz and Rosenbaum 2000). Thus it is possible that residents using vouchers to leave distressed public housing developments are more likely to commit crimes—or have friends who are more apt to commit crimes—than the individuals already living in the voucher holders' chosen destination neighborhoods. However, evidence from the Moving to Opportunity experiment suggests that youth who leave public housing may reduce their criminal behaviors when they leave (Kling, Ludwig, and Katz 2005).

Finally, while each of the above mechanisms presumes that the additional voucher holders are new arrivals to a neighborhood, a sizable number of voucher holders actually remain in their same unit when they first use their vouchers (Feins and Patterson 2005). Thus, some portion of the new vouchers in a neighborhood will generate no additional turnover, and bring additional economic resources to existing residents through the dollar value of their subsidy, which may dampen crime.

In sum, a number of theories suggest that a growth in the number of housing voucher households in a neighborhood could affect crime, perhaps particularly property crime, if we think that poverty leads to greater instances of crimes such as theft, burglary, and the like. These mechanisms, and much of the existing literature analyzing these mechanisms, suggest that additional voucher households in a neighborhood could plausibly increase crime. However, there are also reasons to believe that additional voucher holders could have little effect on crime or even reduce it.

As for empirical work on subsidized housing and crime, there are very few studies that directly test whether and how vouchers shape neighborhood crime. Several papers do explore how other types of subsidized housing affect crime. Most estimate the simple association between the presence of traditional public housing and neighborhood crime. As noted already, many studies find that crime rates are extremely high in and around distressed public housing developments (Goering et al 2002; Hanratty, Pettit, and McLanahan 1998; Rubinowitz and Rosenbaum 2000). But studies that examine how traditional public housing *affects* crime in the surrounding area find more mixed results (Farley 1982; McNulty and Holloway 2000; Roncek, Bell, and Francik 1981).

Perhaps more relevant to our analysis, a few studies actually evaluate how the creation of scattered-site, public housing shapes crime levels. For example, Goetz, Lam, and Heitlinger (1996) examine whether and how creating scattered-site public housing (either through new construction or conversion of existing units) in Minneapolis affects crime in the surrounding neighborhoods. The authors find that police calls from the neighborhoods actually decrease after the creation of the new subsidized housing. However, they also find some evidence that as the developments age, nearby crime increases over time. Galster et al. (2003) also study the impact of scattered-site public housing and find no evidence that the creation of either dispersed public housing or supportive housing alters crime rates in Denver.

Most recently, Freedman and Owens (2010) study whether the Low-Income Housing Tax Credit (LIHTC) activity within a county influences crime in that county. They exploit a discontinuity in the funding mechanism for these tax credits to develop a model that allows them to better estimate a causal relationship between the number of LIHTC developments in a county and crime. If anything, their findings suggest that LIHTC developments reduce crime. Given that they rely on county-level data, however, their results could be masking more localized effects.

Although these studies provide some useful context, their relevance is limited, as they focus on programs that involve supply-side housing production. As such, these programs may affect neighborhoods not only through bringing in subsidized residents but also by changing the physical landscape of the community. Perhaps more relevant to our work, in their assessment of public housing demolition and patterns of homicide concentration, Suresh and Vito (2009) also consider the concentration of Housing Choice Voucher holders. They find that homicides are clustered in neighborhoods that also house voucher recipients, however this work is purely cross-sectional and descriptive.

One unpublished paper specifically analyzes the effect of voucher locations on surrounding crime rates. Van Zandt and Mhatre (2009) analyze crime data within a quarter mile radius of apartment complexes containing 10 or more voucher households during any month between October 2003 and July 2006 in Dallas. Unfortunately, the police did not collect crime data in these areas if the number of voucher households dipped below 10, leading to gaps in coverage and limiting the number and type of neighborhoods examined. Moreover, a consent decree resulting from a desegregation case mandated that the Dallas Police Department collect these crime counts surrounding voucher concentrations, which may have led the police to focus crime control efforts on these areas. Still, the results are revealing. The authors find that clusters of voucher households are associated with higher rates of crime. However, they find no relationship between changes in crime and changes in the number of voucher households, suggesting that while voucher households tend to live in high-crime areas, they are not necessarily the cause of higher crime rates.

Van Zandt and Mhatre's results show that reverse causality may confound estimates of how voucher presence affects crime. As with many low-income households, voucher holders face a constrained set of choices when deciding where to live. They can only live in neighborhoods with affordable rental housing,[11] and they may only know about—or feel comfortable pursuing—a certain set of those neighborhoods, given their networks of social and family ties. In addition, they may be constrained by landlord resistance to accepting vouchers. Research on the Moving to Opportunity (MTO) demonstration program shows that landlord attitudes toward voucher holders play an important role in determining whether voucher households move to and stay in low poverty neighborhoods (Turner and Briggs 2008). Kennedy and Finkel (1994) find that most voucher holders rely on a narrow set of landlords, who serve the "Section 8 submarket." This submarket tends to be concentrated in areas with higher vacancies, as landlords have fewer alternatives in these markets (see Galvez 2010). Collectively, these constraints may lead voucher households to choose neighborhoods that are declining and are experiencing increases in crime. In related work, we find that voucher households occupy

[11] As noted, households bear the full costs of rents that exceed the local rental payment cap.

neighborhoods with higher than average crime rates, at least in cities (Lens, Ellen, and O'Regan 2011).[12] Thus, in our analysis of impacts, we will attempt to control for voucher holders' tendency to locate in high crime areas.

II. Data and Methods

We use a number of different data sources for our analyses, spanning numerous cities and years. First, we collected neighborhood-level crime data for 10 U.S. cities from one of three sources: directly from police department web sites or data requests to the department (Austin, New York, and Seattle), from researchers who obtained these data from police departments (Chicago and Portland[13]), and from the National Neighborhood Indicators Partnership (NNIP) — a consortium of local partners coordinated by the Urban Institute to produce, collect, and disseminate neighborhood-level data (Cleveland, Denver, Indianapolis, Philadelphia, and Washington, DC).[14] For all cities except Philadelphia, we include all property and violent crimes categorized as Part I crimes under the Federal Bureau of Investigation's Uniform Crime Reporting System.[15] In all cities except for Denver, neighborhoods are proxied by census tracts. (Denver crime data are aggregated to locally defined neighborhoods, which are typically two to three census tracts.)

[12] We find that voucher holders live in lower crime neighborhoods than their counterparts in place-based, subsidized housing, however.

[13] We are grateful to Garth Taylor for providing Chicago crime data and to Arthur O'Sullivan for providing crime data for Portland.

[14] We thank the following NNIP partners: Case Western Reserve University (Cleveland), The Piton Foundation (Denver), The Polis Center (Washington, DC), and The Reinvestment Fund (Philadelphia).

[15] Part I violent crimes include homicide, rape, aggravated assault,and robbery, while Part I property crimes include burglary, larceny, motor vehicle theft, and arson. Philadelphia did not provide data on sexual assaults or homicides, and New York City did not provide data on sexual assaults. Thus, those crimes are not included in overall totals for those cities.

Our second key source of data comes from the U.S. Department of Housing and Urban Development (HUD): household-level data on voucher holders and public housing tenants nationwide from 1996 to 2008, which we aggregate to the census tract-level, in order to link to our crime data. Voucher data are provided to HUD by local housing agencies, and should reflect the count of assisted households in a census tract as of the end of the specified year.

Working with administrative data brings challenges. The data on subsidized households are household-level files that come to HUD from local housing agencies, and as such, are subject to potential data quality inconsistencies across these different data collecting entities. Unfortunately, we are missing housing voucher data almost entirely for some cities in some years, most notably, Philadelphia and Seattle. Given the resulting short panels in these two cities (together with the short panel in New York City due to a small number of years of crime data), we re-estimate all the models in this paper on a smaller sample of seven cities for which we have more complete data. As the results are nearly identical, we present results from estimations on the full set of ten cities.[16]

Additionally, we also find some anomalies in particular census tracts for certain years. Some of these gaps are explained by HUD's inability to geocode all of the addresses for voucher holders collected by the housing authorities, particularly in the early years. HUD researchers estimate that the census tract ID is missing and irretrievable for about 15 to 20 percent of the cases in 1996 and 1997, but this rate gradually declines over the time period to about six percent by 2008. In our sample, we find that the reported count of voucher holders in about 2 percent of tract-years deviates sharply from the voucher counts in that tract's previous and subsequent

[16] We have incomplete data on vouchers for Philadelphia and Seattle data from 2002 through 2006, in 1996 for Chicago, Cleveland and Indianapolis, and in 2002 for Portland. We drop these city-years from the dataset.

years.[17] We assume that these counts are incorrect and smooth the data using a linear interpolation to derive what we hope to be more precise estimates of voucher counts for those tracts in those years.[18] While these data inconsistencies and coverage gaps will add measurement error, we have no reason to believe that they are systematically related to neighborhood crime. As a robustness test, we also estimate models that simply omit these data points, and the key results are unchanged.

As shown in Appendix A, our crime and voucher data for these cities cover portions of the 1996 to 2008 period. We have data for at least 11 years in five of our cities (Austin, Chicago, Cleveland, Denver and Indianapolis)[19] and for the remaining five cities, we have data for between four and eight years.

In addition to crime and voucher data, we have access to a limited number of control variables that are available at the census tract level, which help us to provide a more precise estimate of the relationship between voucher locations and crime. First, we control for annual population counts, using linear interpolation to estimate population between the 1990 and 2000 decennial census years and the American Community Survey 2005-2009 average estimates. Second, we control for additional tract characteristics that may vary over time to capture changes in both the housing stock and total population. Specifically, we include the number of public housing units in a tract in a given year, using annual data provided by HUD and the estimated

---

[17] Specifically, we consider a tract's voucher data to be invalid if the number of vouchers in year t was either at least 25 more than the number of vouchers in year t-1 (and at least 25 less in year t+1) or at least 25 less than the number of vouchers in year t-1 (and at least 25 more in year t+1). We use a threshold of 25 because the mean number of vouchers in the sample's tracts is 25. In other words, we assume that changes as large as an entire census tract's typical count, followed by an immediate equally large 'correction' must be attributable to uneven data collection rather than actual changes in program utilization. Using this method, we identified voucher counts in 771 tract-years as invalid (2.4% of the total).

[18] Following Powers (2005), we use the PROC EXPAND tool in SAS to modify the invalid data using a linear interpolation.

[19] In the case of Cleveland, we use 1997 and 1999 crime data to estimate the missing 1998 crime rates with a linear interpolation.

annual vacancy rate, and homeownership rate. In some models, we also include time-varying controls for neighborhood demographic characteristics, such as poverty rate and racial composition, using decennial census data and the American Community Survey (ACS). Because these demographic data are only available for 1990, 2000, and an average for 2005-2009, we linearly interpolate the decennial and ACS data, using the bookend years, as we do with population estimates.[20]

Descriptive Statistics

To provide some context for our sample, Table 1 displays population-weighted means in the year 2000 for the full sample of census tracts, along with the sample of all tracts in U.S. cities with population greater than 100,000. Our sample of cities in year 2000 had, on average, fewer vouchers and more public housing per tract than those in all large U.S. cities. Also, tracts in our sample have a greater share of poor, non-Hispanic black and renter households and a smaller proportion of non-Hispanic whites.

In terms of crime, Table 2 compares the average total, violent, and property crime rates per 1,000 persons for our sample and for the 222 U.S. cities with population greater than 100,000 for which crime data were available from the FBI Uniform Crime Report system in 2000. The average crime rate in our sample in 2000 was about 68.8 crimes per 1,000 persons, with substantial variation across cities.[21] As a comparison, the 2000 crime rate for the sample of U.S. cities with populations greater than 100,000 with crime data available was 60.9 per 1,000 persons, so consistent with the slightly higher poverty rates, the neighborhoods in our sample of cities have slightly higher crime rates on average. Property crimes (burglary, larceny, theft, and motor vehicle theft) are the most common types of crime, with violent crimes (murder, rape,

---

[20] For the purposes of interpolation, we assume that the five-year ACS average represents the middle year, or 2007.

[21] This average excludes New York City as we do not have crime data for New York City in 2000.

robbery, and aggravated assault) occurring much less frequently. In both our sample and nationwide, crime rates declined between 2000 and 2007. Table 3 displays the distribution of voucher households and all households in each city by household income relative to the federal poverty line. Given the income requirements of the voucher program, it is not surprising that voucher households have lower incomes than the general population, but there are two additional observations worth noting. First, the overwhelming majority of voucher holders have incomes at or below the poverty line. Second, while the local income standard will vary due to its reliance on area median income (or AMI), there is a somewhat surprising uniformity across cities in the poverty status of voucher recipients. For voucher households, the proportions of households with incomes below the poverty line range only from 69 to 77 percent. Thus, there may be more similarity in the populations served by this program across our ten cities than the local eligibility standards might suggest.

Methods

Identifying a causal relationship between voucher use and crime is challenging. Many of the neighborhood characteristics that are associated with the presence of voucher households (such as higher vacancy rates and a greater presence of poor households) may also directly shape crime rates. So a neighborhood that experiences a growth in poverty or vacancy rates might both attract more voucher holders and experience an increase in crime. Similarly, reverse causality is a threat too. Crime itself may actually lead to higher vacancy rates and lower rents, thereby making housing units more attainable for voucher households and renting to voucher holders more attractive to landlords. We adopt several strategies to address these endogeneity threats in our modeling, such as including census tract fixed effects, controlling for time-varying trends in neighborhood conditions, and lagging voucher counts..

Our core model regresses the number of crimes in a tract during a specified year on the number of households with vouchers in that tract in the prior year as well as census tract fixed effects (to control for unobserved baseline differences across neighborhoods), time-varying housing characteristics of the tract (public housing counts, the homeownership rate, and the vacancy rate), and city-specific year dummies to control for crime trends in the city. It is worth emphasizing that by including tract fixed effects in these models, which control for all non-varying differences across tracts, our models test whether *deviations* from average voucher use within a given tract are associated with *deviations* in crime within that tract.

Our second model includes time-varying demographic controls in the tract – the poverty rate, median household income, and racial composition, to capture additional trends that may independently affect crime in the tract. However, as noted previously, voucher holders may increase neighborhood crime by changing the population in the neighborhood. To avoid over controlling for these channels, our third model drops time-varying demographic controls and adds PUMA*year dummies to control for trends in the sub-city area surrounding the tract. PUMAs, or Census Public Use Microdata Areas, are sub-city areas that typically include about 25 census tracts, so there is little risk that changes in a particular tract will drive changes in these much larger areas. (PUMAs house at least 100,000 people, while census tracts house about 4,000 people on average.) This provides some ability to control for broader area trends that affect crime.

In addition to including neighborhood fixed effects in all models, we also lag voucher counts to reduce some of the potential reverse causality (that is, that voucher holders tend to live in higher crime areas). Moreover, if we were to measure voucher counts and crime counts contemporaneously, some of the crimes included in the annual count may have actually occurred prior to any change in the number of voucher holders because our dependent variable measures all crimes in a tract over the year, including crimes committed quite early in the year, while our count of vouchers captures the number of voucher holders in a tract at the end of a year. Lagging the voucher counts also allows some time for crime to change after changes in voucher locations—and thus may yield more accurate results.

We choose to use crime counts (which is how the data are reported by the various police departments), rather than rates, to provide greater flexibility and minimize issues with measurement error in intercensal population estimates. It is preferable to absorb this error in a control variable rather than in the dependent variable.[22] As noted, all models include census tract fixed effects, population, and either city*year fixed effects or PUMA*year fixed effects as controls. Hence, we are essentially examining deviations from a tract's average crime, controlling for changes in total population and crime trends in the broader area.

It is important to note that while many researchers use alternative specification strategies (such as Poisson and the Negative Binomial) when crime counts are the dependent variable (Bottcher and Ezell 2005; Gardner, Mulvey, and Shaw 1995; Hipp and Yates 2009; Osgood 2000) we do not believe that this is a necessary step in this case. For our annual data, the distribution of the crime count variable better approximates a normal distribution than either Poisson or Negative Binomial. In these census tracts, over the course of a year, crime is a

[22] Our annual population estimates are interpolated from the decennial census, and so measured with error. While some researchers estimate crime models using rates, with population in the denominator, this is problematic when the population is measured with error. Including population in the denominator of both crime and voucher measures would create a correlation – driven purely by error in measurement error in population – between crime rates and voucher rates (see Ellen and O'Regan, 2010). Hence, we control for population separately from crime and voucher counts, to minimize bias on the key coefficients of interest.

frequent enough occurrence that the data are better approximated by the normal distribution, and thus the corresponding models are best estimated using OLS.[23]

The baseline models are as follows:

$$(1a)\ Crime_{ict} = b_0 + b_1 Voucher_{ict-1} + b_2 X_{ict} + \lambda_i + C_c * T_t + e_{ict}$$

$$(1b)\ Crime_{ict} = b_0 + b_1 Voucher_{ict-1} + b_2 Z_{ict} + \lambda_i + C_c * T_t + e_{ict}$$

$$(1c)\ Crime_{ipt} = b_0 + b_1 Voucher_{ipt-1} + b_2 X_{ipt} + \lambda_i + P_p * T_t + e_{ipt}$$

where $Crime_{ict}$ indicates the crime count in tract i, city c (or PUMA p), and year t, $Voucher_{ict-1}$ represents the number of voucher holders in tract i in year t-1, λ_i is a census tract fixed effect, C_c*T_t is a vector of city-specific year fixed effects (and $PUMA_p*T_t$ is a vector of PUMA*year fixed effects), X_{ict} include time-varying housing characteristics of the tract in year t, Z_{ict} signifies the time-varying housing and demographic controls of the tract in year t, and e_{ict} is the error term. A significant coefficient on the number of vouchers in year t-1 provides evidence of an association between neighborhood crime and voucher holders.

We also estimate – but do not report – these same specifications using voucher counts measured in year t, rather than year t-1. These models represent the 'observational' association discussed in Rosin (2008), which has resulted in speculation that vouchers cause crime. Indeed, as discussed, there is as much or more reason to believe that reverse causality (or the presence of

[23] However, we did estimate all of the models reported in this paper with the natural logarithm of crimes as the dependent variable as a robustness check. The advantage of using this variable is we can relax the normal distribution assumption. On the other hand, in some tract-years (less than 0.1% of the observations), there were no crimes, and ln(0) is undefined. Thus, we were forced to choose an arbitrary number (0.01 in this case) instead of 0. We did not find any meaningful differences in this set of results using the alternative dependent variable.

trends that lead neighborhoods to experience increases both increases in crime and voucher use) may lead to a contemporaneous association between vouchers and crime.

To test for the potential for such reverse causality – and to provide an additional control for unobserved time-varying characteristics of the tracts under study – we estimate the same models as above, but with future voucher holders included on the right hand side to test whether increases in crime are followed by increases in voucher use, rather than the reverse. Specifically, we estimate the following regressions:

$$(2a)\ Crime_{ict} = b_0 + b_1 Voucher_{ict-1} + b_2 Voucher_{ict+1} + b_3 X_{ict} + \lambda_{\mathrm{i}} + \mathrm{C_c} * T_t + e_{ict}$$

$$(2b)\ Crime_{ict} = b_0 + b_1 Voucher_{ict-1} + b_2 Voucher_{ict+1} + b_3 Z_{ict} + \lambda_{\mathrm{i}} + C_c * T_t + e_{ict}$$

$$(2c)\ Crime_{ipt} = b_0 + b_1 Voucher_{ipt-1} + b_2 Voucher_{ipt+1} + b_3 X_{ipt} + \lambda_{\mathrm{i}} + \mathrm{P}_p * T_t + e_{ipt}$$

Finally, we also experiment with several alternative specifications of the relationship between voucher counts and crime. First, we test whether the marginal impact of an additional voucher holder varies with the baseline number of vouchers through a non-linear specification, by including the number of vouchers plus the number of vouchers squared on the right hand side. This regression can be expressed as follows (3a repeated as 3b-3c as above):

$$(3a)\ Crime_{ict} = b_0 + b_1 Voucher_{ict-1} + b_2 Voucher\char`^2_{ict-1} + b_3 X_{ict} + \lambda_{\mathrm{i}} + \mathrm{C_c} * T_t + e_{ict}$$

Second, we also test whether the marginal impact of an additional voucher holder in a neighborhood varies with the poverty level of a tract. Here, the idea is that an additional voucher holder may affect tracts that are generally high poverty differently than tracts that are typically low poverty. We test for this by allowing the association between voucher holders and crime to vary depending on whether a tract's 1990 poverty rate was in the top or bottom quartile of all neighborhoods. These models can be expressed as follows:

$$(4a)\ Crime_{ict} = b_0 + b_1 Voucher_{ict-1} + b_2 HighPoverty * Voucher_{ict-1} + b_3 X_{ict} + \lambda_i + C_c * T_t + e_{ict}$$

$$(5a)\ Crime_{ict} = b_0 + b_1 Voucher_{ict-1} + b_2 LowPoverty * Voucher_{ict-1} + b_3 X_{ict} + \lambda_i + C_c * T_t + e_{ict}$$

III. Results

The simple bivariate correlation between total crime counts and voucher household counts within each of our cities averages 0.30 suggesting a positive relationship between the presence of voucher holders and crime—tracts with more crime also house more voucher holders on average. But our interest is not in a simple association, but rather whether the presence of voucher holders actually increases crime.

Table 4 displays results from our first two sets of models of crime counts, estimated on the pooled sample. As noted, all models include tract fixed effects and controls for population, public housing counts, and the homeownership and vacancy rates. The first and fourth columns show results of the basic regressions with these housing controls and city*year fixed effects, while the remaining columns show regressions that include additional controls for more local trends, either tract-level demographic variables or PUMA*year effects to control for unobserved, time varying factors in the sub-city area that includes the census tract. Specifically, the second and fifth columns include racial composition, poverty, and the median household income in the tract. As noted, these models may 'over-control' to some degree, in that they control for some of the mechanisms through which vouchers could influence crime. The third and sixth columns omit these demographic controls but include the PUMA*year effects.

To ensure that our count of voucher holders reflects the number present before any crimes counted in our annual measures occur, the first three columns show regressions of crime counts in year t on voucher counts in year t-1. To provide a test for reverse causality, and an additional control for unobservable trends, we show results of regressions that include voucher counts in year t+1 (columns four through six).

In our first model (column 1), with tract fixed effects, city*year fixed effects and only population and housing characteristics as time-varying tract controls, the coefficient on lagged voucher households is positive but statistically insignificant (even at the 10 percent level). When we add tract demographic controls in the second column, the coefficient on voucher households falls very slightly, remaining insignificant.[24] In column three, we remove these demographic tract variables and replace city*year fixed effects with PUMA*year fixed effects. The coefficient on voucher counts declines by about half and is now far from significant. Indeed, the coefficient is now smaller than its standard error. We have replicated these models on various samples, adding and removing city*years based on different criteria for data quality. In all versions of our sample, once we control for crime trends in the broader area (which could not be caused by voucher holders in the specific tract), there is no significant association between deviations in the number of voucher holders in a tract in one year, and deviations in tract crime in the next.[25]

The last three columns of the table show results for models that include voucher counts in year t+1. Clearly, voucher holders who have not yet entered the tract cannot be causing crime in time t. We see here that there is a strong positive and statistically significant relationship between

---

[24] We also estimated these same models on the smaller set of 7 cities for which we have the most complete annual crime and voucher data, omitting New York City, Philadelphia, and Seattle. The key results are the same.

[25] We have also estimated these same models with contemporaneous voucher counts, a model we don't interpret as causal for reasons discussed earlier. Even with contemporaneous measures of crime, after including controls for trends in the broader sub-city area, there is no relationship between vouchers and crime. This lack of relationship in Table 4 is not a by-product of our lagging crime.

future vouchers and current crime, suggesting that voucher use tends to increase in neighborhoods where crime has been elevated. Furthermore, we see that the magnitude of the coefficients on lagged vouchers in columns 1 and 2 decline further when the voucher count in year t+1 is added. To some extent, these future vouchers may be picking up unobserved trends in the tract that might invite crime. Note that we ran these models with vouchers in year t as the independent variable, with virtually the same results. The coefficient on contemporaneous crime is never significant in models that also include future voucher counts, though the coefficient on the number of future voucher holders is highly significant in all models[26].

These results suggest that the simple bivariate correlation between crime and vouchers may primarily be driven by other correlated factors. The positive association becomes insignificant once we control for census tract fixed effects, even in models with few controls for other changes occurring in a tract or its surrounding area. Moreover, the magnitude of the estimated coefficients declines by half once such controls are included. Thus, it appears that crime tends to be elevated in tracts with more voucher holders because of unobserved characteristics of the tracts that house voucher holders and broader trends in the areas surrounding them.

As noted previously, it is possible that the relationship between vouchers and crime is non-linear. Such misspecification could prevent us from finding a positive relationship. We test for such non-linearities by adding a squared voucher count term to our models, permitting an additional voucher recipient in a tract to have a different effect depending on whether the initial voucher counts are low or high. Table 5 presents results of such quadratic models with lagged voucher counts, in the same order seen in Table 4.

---

[26] We re-estimated all regressions with standard errors clustered at the level of the PUMA and the coefficients on future voucher use retained significance.

The coefficient on voucher counts is larger and weakly significant (at the 10 percent level) in columns 1 and 2, and the coefficient on vouchers squared is weakly statistically significant in model 1 and close to significant at the 10 percent level in model 2. This suggests the nonlinear specification may better capture the form of any association between the presence of voucher holders and crime. Nonetheless, the point estimates are small though, suggesting that adding another voucher holder to a tract with 25 voucher holders (the average number of vouchers in our sample) is associated with 0.1 additional crimes. Assuming an average number of annual crimes in the tract, this amounts to an increase in crime of one tenth of one percent. Moreover, once area trends are controlled for (column 3), the coefficient on the linear lagged voucher counts declines by half and becomes statistically insignificant. In columns 4-6, we include future voucher counts as an alternative control on trends, and the coefficient on lagged counts is diminished and insignificant in all models.

Finally, we also consider whether the relationship between vouchers and crime varies by context, and specifically by the baseline level of poverty in a neighborhood, as many of the theories outlined above suggest that additional voucher holders might have a larger effect in lower poverty areas. To do so, we estimate models that include both voucher counts and an interaction between voucher counts and a dummy variable indicating whether the tract is ranked in the top (or bottom) quartile of poverty rates in 1990.[27] Results for our models with PUMA*year fixed effects and housing controls for the bottom poverty quartile interactions are shown in Table 6. In columns 1-3, results very closely mirror those from our baseline models. The coefficient on lagged vouchers is again positive but statistically insignificant in our first two models, and the coefficient on the interaction term is insignificant as well, providing no evidence

[27] Notably, there is a large difference between average poverty rates within the high and low poverty quartiles. The average poverty rate is 44 percent among tracts in the top quartile compared to four percent for those in the bottom quartile.

of differential effects. Again, the coefficient on voucher counts drops considerably after PUMA*year interactions are added. Columns 4 through 6 provide results for non-linear versions of these models, with essentially the same findings. The only coefficient on voucher counts that is significant is vouchers squared interacted with low poverty, and that coefficient is negative.[28] Note that we also estimated models interacting voucher counts with a dummy variable indicating whether or a not a tract was in the highest poverty quartile, and we found no statistically significant coefficients.[29]

We also test separately for differential impacts on property and violent crimes. Specifically, for all of the models reported in Tables 4 through 6, we estimate the same models with the natural logarithm of violent crimes and the natural logarithm of property crimes as dependent variables (using the natural logarithm since data in these disaggregated crime categories are more likely to be skewed toward zero). In those models (not displayed), we find no relationship at all between voucher counts and violent crime. For property crime, the results are highly similar to the results for total crime, which is not surprising given that a very large majority of crimes are property crimes.

IV. Conclusion

Through our many models, we find little evidence to support the hypothesis offered by Hanna Rosin and others that voucher holders invite or create crime. Our findings suggest a weak and small positive relationship in a few selected models, but only when other trends in the area

[28] Also, a tract would have to have 36 voucher holders for this negative effect to dominate, which would be quite rare for a low poverty tract.
[29] We also stratify sample by 2000 poverty rate and get essentially the same results. In no set of neighborhoods do we find that lagged vouchers are positively linked to crime.

or tract are not taken into account. When such trends in the surrounding area are controlled for, there is no association between more voucher holders and crime, even when measured contemporaneously. Thus, we find no credible evidence of a causal relationship running from vouchers to crime. However, when we examine the relationship between current crime and future voucher counts, we find a much stronger connection; more crime today is associated with more voucher holders in the future. This association could be driven either by broader trends that both contribute to neighborhood crime and make neighborhoods more accessible to voucher holders or by reverse causality (higher crime rates themselves make neighborhoods more open to voucher holders, via higher vacancy rates and greater interest on the part of landlords, for example). Regardless of the precise channel, these results are important for how one interprets any association between vouchers and crime in other research and for policy.

There is surely room for additional research (such as testing relationships in suburban communities as well as urban neighborhoods, testing for impacts on less serious, public order crimes like vandalism, and estimating models at an even smaller level of geography such as blocks), but these results should provide some comfort to communities concerned about the entry of voucher holders. However, our finding that voucher holders tend to move into neighborhoods where crime has been elevated should be troubling to policymakers. The housing choice voucher program is designed to enhance tenant choice, allowing them to choose among a wide variety of homes and neighborhoods. The fact that voucher use in a neighborhood tends to grow after crime increases suggests that these choices may be constrained. Policymakers should take a close look at the administration of the voucher program and work hard to address any barriers to tenant choices.

Table 1: Average Tract Characteristics

Sample: Sample Cities, Year 2000 and All Tracts in U.S. Cities with 2000 Population > 100,000

Weighted by census tract population

	All Tracts in Sample Cities (N=4,813)				All Tracts in U.S. Cities > 100,000 (N=19,252 tracts, 250 cities)			
Variable	**Mean**	**Min**	**Max**	**Totals**	**Mean**	**Min**	**Max**	**Totals**
Voucher Units	25.2	0	356	91,226	38.4	0	690	596,783
Public Housing units	35.9	0	6,497	130,967	31.5	0	3,852	606,448
LIHTC Units	26.7	0	1,516	105,292	24.9	0	1,516	405,728
Population	5088.6	207	24,523	17,872,468	5,162.0	0	34,055	76,867,230
Poverty Rate	19.2%	0	93%	N/A	17.1%	0	1	N/A
Percent Non-Hispanic White	43.5%	0	100%	N/A	48.0%	0	1	N/A
Percent Non-Hispanic Black	27.9%	0	100%	N/A	21.8%	0	1	N/A
Percent Hispanic	20.5%	0	99%	N/A	22.3%	0	1	N/A
Homeownership Rate	42.8%	0	100%	N/A	51.5%	0	1	N/A

Table 2: Average Total, Violent, and Property Crimes per 1000 Persons, 2000

Sample: Sample Cities[30] and 222 U.S. Cities with Population > 100,000 (with crime data available)

Weighted by Census Tract Population

	Our Sample Cities (N=2,116)				U.S. Cities with Population > 100,000 (N=19,252)			
Variable	**Mean***	**Std. Dev**	**Min**	**Max**	**Mean***	**Std. Dev**	**Min**	**Max**
Total Crimes per 1000	68.8	3895.2	0	1835.6	60.9	13000.8	14.3	148.2348
Violent Crimes per 1000	13.5	848.2	0	209.2	9.5	2740.5	1.15	27.43058
Property Crimes per 1000	55.3	3420.4	0	1710.6	55.4	10574.9	13	127.3397

[30] Excludes New York City, as we do not have crime data for New York City in 2000.

Table 3: Voucher Households and All Residents of Sample Cities by Relation to the Poverty Line, by City, 2000

	VOUCHERS			ALL HOUSEHOLDS		
	Below Poverty	100 to 150% Poverty	150% Poverty and Above	Below Poverty	100 to 150% Poverty	150% Poverty and Above
AUSTIN	71.5%	19.1%	9.4%	14.4%	8.9%	76.7%
CHICAGO	71.1%	18.1%	10.8%	19.6%	10.4%	70.0%
CLEVELAND	73.6%	18.1%	8.3%	26.3%	13.2%	60.6%
DENVER	70.1%	21.6%	8.3%	14.3%	9.8%	75.9%
INDIANAPOLIS	70.3%	21.5%	8.2%	11.9%	8.2%	79.9%
NEW YORK	76.9%	14.8%	8.3%	21.2%	9.8%	68.9%
PHILADELPHIA	75.2%	16.6%	8.2%	22.9%	10.7%	66.3%
PITTSBURGH	72.8%	20.2%	7.1%	20.4%	10.4%	69.2%
PORTLAND	72.7%	19.1%	8.3%	13.1%	8.5%	78.4%
SEATTLE	69.2%	19.4%	11.4%	11.8%	6.6%	81.6%
WASHINGTON	70.5%	16.9%	12.7%	20.2%	8.4%	71.4%
TOTAL	74.5%	16.6%	8.8%	19.9%	9.9%	70.2%

Table 4: Baseline Regression Results

Dependent variable: Number of crimes in tract in year

Sample: All cities

Variable	**1**	**2**	**3**	**4**	**5**	**6**
Voucher Counts t-1	0.0581	0.0544	0.0299	0.0422	0.0414	0.0237
	(0.0374)	(0.0372)	(0.0357)	(0.0403)	(0.0402)	(0.0413)
Voucher Counts t+1				0.167***	0.157**	0.160**
				(0.0633)	(0.0632)	(0.0645)
Log Population	46.83***	40.54***	48.67***	47.64***	40.79**	48.55***
	(13.99)	(15.45)	(12.45)	(16.73)	(18.31)	(15.02)
Public Housing	0.0430***	0.0359***	0.0278*	0.0630***	0.0541***	0.0382*
	(0.0131)	(0.0132)	(0.0144)	(0.0169)	(0.0179)	(0.0203)
Percent Owner-Occ	-90.90***	-67.73**	-28.96	-81.01**	-56.98	-27.91
	(30.75)	(30.16)	(31.11)	(37.43)	(35.33)	(37.42)
Percent Vacant	64.79**	48.55	18.87	84.83**	68.56*	34.52
	(29.81)	(32.07)	(30.84)	(32.94)	(35.37)	(34.29)
Percent Hispanic		69.45**			81.06**	
		(30.49)			(33.02)	
Percent NH Black		87.49***			84.78***	
		(26.23)			(28.70)	
Median HH Income		0.00006			0.000111	
		(0.0002)			(0.0003)	
Percent Poverty		11.20			14.06	
		(21.64)			(24.72)	
Constant	-125.3	-130.6	-173.5	-164.9	-173.1	-181.0
	(119.0)	(128.4)	(105.6)	(143.6)	(153.4)	(128.9)
Tract FEs	Yes	Yes	Yes	Yes	Yes	Yes
City*Year FEs	Yes	Yes	No	Yes	Yes	No
Puma*Year FEs	No	No	Yes	No	No	Yes
Observations	25,702	25,570	25,698	21,541	21,433	21,539
Number of Tracts	4,235	4,225	4,234	4,233	4,223	4,232
Adjusted R-squared	0.184	0.186	0.245	0.188	0.190	0.244

Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Testing for Nonlinear Effects – Quadratic Models

Dependent variable: Number of crimes in tract in year

Sample: All cities

Variable	**1**	**2**	**3**	**4**	**5**	**6**
Voucher Counts t-1	0.113*	0.105*	0.0510	0.0220	0.0205	-0.0202
	(0.0587)	(0.0588)	(0.0582)	(0.0647)	(0.0649)	(0.0655)
Vouchers Squared t-1	-0.00008*	-0.00007	-0.00003	0.00005	0.00005	0.00008
	(0.00005)	(0.00005)	(0.00005)	(0.0001)	(0.0001)	(0.0001)
Voucher Counts t+1				0.132	0.109	0.134
				(0.120)	(0.120)	(0.124)
Vouchers Squared				0.0001	0.0001	0.00009
				(0.0003)	(0.0003)	(0.0003)
Log Population	46.38***	39.92***	48.46***	47.92***	41.27**	49.05***
	(13.96)	(15.42)	(12.45)	(16.71)	(18.25)	(14.93)
Public Housing	0.0431***	0.0361**	0.0279*	0.0629**	0.0538**	0.0380*
	(0.0132)	(0.0132)	(0.0144)	(0.0169)	(0.0179)	(0.0203)
Percent Owner-Occ	-88.84***	-66.85**	-28.52	-82.18**	-57.66	-28.42
	(30.83)	(30.19)	(31.16)	(38.14)	(35.79)	(37.77)
Percent Vacant	64.21**	48.11	18.75	85.46***	69.18*	35.03
	(29.77)	(32.02)	(30.83)	(32.90)	(35.32)	(34.27)
Percent Hispanic		69.14**			81.18**	
		(30.47)			(33.02)	
Percent NH Black		86.47***			86.46***	
		(26.20)			(28.08)	
Median HH Income		7.66e-05			0.000102	
		(0.000229			(0.000269	
Percent Poverty		11.51			14.20	
		(21.62)			(24.75)	
Constant	-121.6	-126.8	-168.9	-162.2	-169.1	-183.6
	(118.7)	(128.1)	(105.6)	(143.9)	(153.1)	(128.7)
Tract Fes	Yes	Yes	Yes	Yes	Yes	Yes
City*Year FEs	Yes	Yes	No	Yes	Yes	No
Puma*Year FEs	No	No	Yes	No	No	Yes
Observations	25,702	25,570	25,698	21,541	21,433	21,539
Number of Tracts	4,235	4,225	4,234	4,233	4,223	4,232
Adjusted R-squared	0.184	0.186	0.245	0.188	0.190	0.244

Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Testing for Differential Effects – Poverty Models

Dependent variable: Number of crimes in tract in year

Sample: All cities

Variable	**1**	**2**	**3**	**4**	**5**	**6**
Voucher Counts t-1	0.0564	0.0555	0.0320	0.0589	0.0507	0.00788
	(0.0394)	(0.0393)	(0.0382)	(0.0604)	(0.0598)	(0.0601)
Vouchers*Low Pov t-1	0.0221	-0.0142	-0.0267	0.0695	-0.0140	-0.00278
	(0.132)	(0.134)	(0.136)	(0.182)	(0.184)	(0.183)
Voucher Squared t-1				-0.00006	-0.00003	-0.00002
				(0.00005)	(0.00005)	0.00005)
Vouch Sq *Low Pov t-1				-0.0019	-0.0016	-0.00153*
				(0.0007)	(0.0007)	(0.0008)
Log Population	46.64***	40.67***	48.88***	33.99***	26.37*	37.66***
	(14.04)	(15.53)	(12.53)	(12.68)	(14.50)	(11.54)
Public Housing	0.0430***	0.0359***	0.0278*	0.0463***	0.0386***	0.0307**
	(0.0132)	(0.0132)	(0.0144)	(0.0133)	(0.0132)	(0.0145)
Percent Owner-Occ	-90.82***	-67.73**	-29.03	-113.5***	-90.96***	-50.93**
	(30.76)	(30.16)	(31.14)	(22.28)	(24.45)	(23.81)
Percent Vacant	64.84**	48.53	18.82	55.82*	34.32	11.58
	(29.82)	(32.08)	(30.86)	(29.43)	(31.03)	(30.84)
Percent Hispanic		69.66**			65.65**	
		(30.88)			(31.30)	
Percent NH Black		87.64***			99.69***	
		(26.26)			(25.52)	
Median HH Income		6.10e-05			5.95e-06	
		(0.000229)			(0.000207)	
Percent Poverty		11.18			5.577	
		(21.67)			(20.28)	
Constant	-123.2	-132.5	-174.3	-12.24	-9.161	-67.22
	(119.3)	(129.0)	(106.1)	(103.8)	(115.1)	(94.41)
Tract Fes	Yes	Yes	Yes	Yes	Yes	Yes
City*Year FEs	Yes	Yes	No	Yes	Yes	No
Puma*Year FEs	No	No	Yes	No	No	Yes
Observations	25,702	25,570	25,698	24,866	24,734	24,862
Number of Tracts	4,235	4,225	4,234	4,159	4,149	4,158
Adjusted R-squared	0.184	0.186	0.245	0.167	0.170	0.230

Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

References

Agnew, Robert. 1992. "Foundation for a general strain theory of crime and delinquency." *Criminology*, 30(1): 47-87.

Becker, Gary. 1968. "Crime and Punishment: An Economic Approach". *The Journal of Political Economy*. 76.

Bjerk, David. 2007. "Measuring the relationship between youth criminal participation and household economic resources." *Journal of Quantitative Criminology*, 23(1): 23-39.

Bovard, James. 2011. "Raising Hell in Subsidized Housing" *The Wall Street Journal*. August 17, 2011. http://online.wsj.com/article/SB10001424053111903520204576480542593887906.html

Bottcher, Jean, and Michael E. Ezell. 2005. "Examining the Effectiveness of Boot Camps: A Randomized Experiment with a Long-Term Follow Up." *Journal of Research in Crime and Delinquency* (42): 309-332.

Ellen, Ingrid Gould, and Katherine M. O'Regan. 2010. "Crime and Urban Flight Revisited: The Effect of the 1990s Drop in Crime on Cities." *Journal of Urban Economics*, 68(3): 247-259.

Farley, John E. 1982. "Has Public Housing Gotten a Bum Rap?: The Incidence of Crime in St. Louis Public Housing Developments." *Environment and Behavior* 14(4) (July): 443-77.

Freedman, Matthew and Emily Owens. 2011. "Low-Income Housing Development and Crime." *Journal of Urban Economics* (In Press).

Galster, George. C., Peter A. Tatian, Anna M. Santiago, Kathryn L.S. Pettit, and Robin E. Smith. 2003. *Why Not in My Backyard?: Neighborhood Impacts of Deconcentrating Assisted Housing*. New Brunswick, New Jersey: Center for Urban Policy Research.

Galster, George C. 2005. "Consequences From the Redistribution of Urban Poverty During the 1990s: A Cautionary Tale." *Economic Development Quarterly* 19(2): 119-25. doi: 10 .1177/0891242404273981.

Galvez, Martha. 2010. "What Do We Know about Housing Choice Voucher Program Location Outcomes?" What Works Collaborative Working Paper. Washington, DC: The Urban Institute. http://www.urban.org/url.cfm?ID=412218

Gardner, William, Edward P. Mulvey, and Esther C. Shaw. 1995. "Regression Analyses of Counts and Rates: Poisson, Overdispersed Poisson, and Negative Binomial Models." *Psychological Bulletin* 118 (3), 392-404.

Goering, John, Judith D. Feins, and Todd M. Richardson. 2002. "A Cross-Site Analysis of Initial Moving to Opportunity Demonstration Results." *Journal of Housing Research* 13(1): 1.

Goetz, Edward G., Hin Kin Lam, and Anne Heitlinger. 1996. *There Goes the Neighborhood : The Impact of Subsidized Multi-Family Housing on Urban Neighborhoods*. Minneapolis, Minnesota: University of Minnesota Center for Urban and Regional Affairs.

Hannon, Lance. 2002. "Criminal Opportunity Theory and the Relationship between Poverty and Property Crime." *Sociological Spectrum* 22: 363-81.

Hanratty, Maria, Sara McLanahan, and Becky Pettit. 1998. "The Impact of the Los Angeles Moving to Opportunity Program on Residential Mobility, Neighborhood Characteristics, and Early Child and Parent Outcomes." Working Paper 98-18, Princeton University Center for Research on Child Wellbeing, Princeton, NJ.

Hipp, John R. (2007). "Income Inequality, Race, and Place: Does the Distribution of Race and Class within Neighborhoods Affect Crime Rates?" *Criminology* 45(3): 665-97.

Hipp, John R., George E. Tita, and Robert T. Greenbaum. 2009. "Drive-Bys and Trade-Ups: Examining the Directionality of the Crime and Residential Instability Relationship." *Social Forces* 87(4): 1777-1812.

Hipp, John R., and Daniel K. Yates. 2009. "Do Returning Parolees Affect Neighborhood Crime? A Case Study of Sacramento." *Criminology* 47 (3): 619-656.

Hsieh, Ching-Chi, and M. D. Pugh. 1993. "Poverty, Income Inequality, and Violent Crime: A Meta-Analysis of Recent Aggregate Data Studies." *Criminal Justice Review* 18(2): 182-202. doi:10.1177/073401689301800203.

Kennedy, Stephen D., and Meryl Finkel. 1994. "Section 8 Rental Voucher and Rental Certificate Utilization Study: Final Report." Cambridge, MA: Abt Associates.

Kirk, David, and Andrew V. Papachristos. "Cultural Mechanisms and the Persistence of Neighborhood Violence." *American Journal of Sociology* Vol. 116 (4): 1190-1233.

Kling, Jeffrey R., Jens Ludwig, and Lawrence F. Katz. 2005. "Neighborhood Effects on Crime for Female and Male Youth: Evidence from a Randomized Housing Voucher Experiment." *The Quarterly Journal of Economics* 120(1): 87-130.

Krivo, Lauren and Ruth D. Peterson. 1996. "Extremely Disadvantaged Neighborhoods and Urban Crime." *Social Forces* 75(2): 619-48.

Levitt, Steven. 1999. "The Changing Relationship between Income and Crime Victimization." *FRBNY Economic Policy Review* 5(3): 87-98.

Lens, Michael C., Ingrid Gould Ellen, and Katherine O'Regan. 2011. "Do Vouchers Help Low-Income Households Live in Safer Neighborhoods? Evidence on the Housing Choice Voucher Program." *Cityscape* Vol. 13(3): 135-159.

Mempin, Jennifer. 2011. “Subsidies and Suspicion.” *The New York Times* August 10, 2011. http://www.nytimes.com/2011/08/11/us/11housing.html?_r=2&pagewanted=all

McNulty, Thomas L., and Steven R. Holloway. 2000. “Race, Crime, and Public Housing in Atlanta: Testing a Conditional Effect Hypothesis.” *Social Forces* 79(2): 707-29.

Osgood, D. Wayne. 2000. “Poisson-Based Regression Analysis of Aggregate Crime Rates.” *Journal of Quantitative Criminology*,16 (1): 21-43.

Powers, Keiko I. 2005. “Effects of PROC EXPAND Data *Interpolation on Time Series Modeling When the Data are Volatile or Complex*. Cary, NC: SAS Institute.

Roncek, Dennis W., Ralph Bell, and Jeffrey M. A. Francik. 1981. “Housing Projects and Crime: Testing a Proximity Hypothesis.” *Social Problems* 29(2): 151-66.

Rosin, Hanna. 2008. "American Murder Mystery." *The Atlantic* July/August. Retrieved from http://www.theatlantic.com/magazine/archive/2008/07/american-murder-mystery/6872/1.

Rubinowitz, Leonard S., and James E. Rosenbaum. 2000. *Crossing the Class and Color Lines: From Public Housing to White Suburbia*. Chicago, Illinois: University of Chicago Press.

Sampson, Robert J., Stephen W. Raudenbush, and Felton Earls. 1997. “Neighborhoods and Violent Crime: A Multilevel Study of Collective Efficacy.” *Science* 277: 918-924.

Sampson, Robert J. and William Julius Wilson. 1995. “Toward a Theory of Race, Crime, and Urban Inequality.” In *Crime and Inequality*, edited by John Hagan and Ruth Peterson. Stanford, California: Stanford University Press.

Suresh, Geetha and Gennaro Vito. 2009. “Homicide Patterns and Public Housing: The Case of Louisville, KY (1989-2007)” *Homicide Studies* 13(4): 411-433.

Stults, Brian J. 2010. “Determinants of Chicago Neighborhood Homicide Trajectories: 1965-1995.” *Homicide Studies* 14(3): 244-267.

Shaw, Clifford R. & McKay, Henry D. 1942. *Juvenile Delinquency in Urban Areas*. Chicago, Illinois: University of Chicago Press.

Turner, Margery and Xavier de Souza Briggs. 2008. “Assisted Housing Mobility and the Success of Low-Income Minority Families: Lessons for Policy, Practice, and Future Research.” *Urban Institute Metropolitan Housing and Communities Brief* 5.

U.S. Department of Housing and Urban Development. 2001. *Housing Choice Voucher Program Guidebook.* Washington, D.C.: U.S Department of Housing and Urban Development.

Van Zandt, Shannon, and Pratik Mhatre. 2009. “The Effect of Housing Choice Voucher Households on Neighborhood Crime: Longitudinal Evidence from Dallas.” Working Paper 09-01, Sustainable Housing Research Unit (SHRU), College of Architecture, Texas A&M University.

Appendix A: Data

Table A—1: Number of tracts with voucher and crime data by city and year

City	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	Total
Austin	114	114	112	112	113	113	113	113	113	113	113	113	113	1,469
Chicago		821	821	821	821	821	820	819	819	817	814	811		9,005
Cleveland		210	210	210	210	210	210	210	209	208	205	204	204	2,500
Denver	76	76	76	76	76	76	76	76	76	76	76	76	76	988
Indianapolis		98	98	98	98	98	98	98	98	98	98	98	98	1,176
New York									2,120	2,118	2,117	2,117	2,117	10,589
Philadelphia			357	357	357	357								1,428
Portland		145	145	145	145	145								725
Seattle	116	116	116	116	116	116								696
Washington					180	180	180	180	180	180	180	180		1,440
Total	306	1580	1935	1935	2116	2116	1497	1496	3615	3610	3603	3599	2608	30016