Memphis Murder Revisited: Do Housing Vouchers Cause Crime?

Assisted Housing Research Cadre Report

U.S. DEPARTMENT OF HOUSING AND URBAN DEVELOPMENT

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Memphis Murder Revisited:
Do Housing Vouchers Cause Crime?

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Authors:
Ingrid Gould Ellen
Michael C. Lens
Katherine O'Regan
Wagner School and Furman Center, New York University

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DISCLAIMER

Preface

Although the size of the Housing Choice Voucher (HCV) program has increased to over 2.2 million units by 2008, communities sometimes oppose vouchers because of concerns 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 Monthly* in August 2008. Despite the publicity, virtually no research systematically examines the link between the presence of voucher holders in a neighborhood and crime.

For this report, *Memphis Murder Revisited: Do Housing Vouchers Cause Crime?,* the researchers attempted to find evidence that an increase in the number of voucher holders in a tract leads to more crime. They found that crime in a year tends to be higher in census tracts with more voucher households that year, but that positive relationship disappears when they control for last year's crime rate or crime trends in the broader sub-city area. There is strong evidence for the reverse causal story, however. That is, the number of voucher holders in a neighborhood tends to increase in tracts with rising crime, suggesting that voucher holders are more likely to move into neighborhoods when crime rates are increasing.

Longitudinal, neighborhood-level crime and voucher utilization data in 10 large U.S. cities are the data that were used to examine whether additional voucher holders lead to elevated rates of crime. The unique set of annual, neighborhood-level crime data covers portions of the 1995 to 2008 period. The researchers used crime data for each of the 14 years for three cities (Austin, Chicago, and Indianapolis) and crime data for all but one year in Cleveland, Denver, and Seattle. For all other cities, they used crime data for between five and eight years.

The heart of the report is a set of regression models of census tract-level crime that test whether additional voucher holders lead to elevated rates of crime. The models control for the presence of other subsidized housing, demographic characteristics of census tracts that change over time, and census tract fixed effects, which capture unobserved, pre-existing differences between neighborhoods that house large numbers of voucher households and those that do not. Some models also control for neighborhood crime in the prior year and for broader crime trends. Finally, they also test for the possibility that voucher holders tend to settle in higher crime areas.

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Although the academic and policy communities have welcomed this shift, community opposition to vouchers can be fierce (Galster et al. 2003). 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 Monthly* in August 2008. Despite the 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.

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[*] 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.)

I. Introduction

The size of the Housing Choice Voucher (HCV) program has increased significantly over the course of its existence.[1] For instance, in 1980, the traditional public housing program was twice the size of the rental certificate program ("HCV predecessor"), but that shifted over time as the rental certificate program grew in popularity and as there was a shift in federal housing strategy from locally owned public housing to privately owned rental housing. By 2008, the voucher program was almost twice the size of the public housing program. There were 2.2 million vouchers nationwide in 2008, compared to 1.2 million public housing units.

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II. Theory and Prior Literature

Theoretical Mechanisms

Local residents often oppose the construction of subsidized rental housing or the arrival of new housing voucher holders, voicing concerns that the new residents will bring with them elevated rates of crime. While the fears are not always well-articulated, the entry of voucher holders could theoretically lead to crime in a neighborhood. There are at least four mechanisms through which the presence of voucher households might influence local crime rates. First, both economists (Becker 1968) and sociologists (Agnew 1992; Merton 1938) have developed robust theories explaining why economically distressed households might be more likely to engage in criminal activity. In Merton's widely cited genesis of "strain theory," poverty leads to antisocial and criminal behavior in societies where poverty, limited opportunity, and shared symbols of success produce pressure or "strain" to achieve economic mobility that can lead to deviant behavior. As for the economic perspective, Becker's criminal is a rational actor who weighs the costs and benefits of committing crimes. More impoverished individuals have much more to gain (and less to lose) from criminal activity. In each case, poor individuals will be more likely to commit crimes, and crime will be higher in higher poverty neighborhoods. Indeed, concentrated disadvantage can 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. (Or in a refinement of the theory, some have suggested that increases in poverty rates in a community will lead to crime once poverty rates reach a certain threshold level or tipping point (Galster 2005).) Empirically, many studies have found a connection between family income and the likelihood that members of that family will be involved in criminal activity and/or a relationship between the poverty rate in a neighborhood and the crime rate (Hsieh and Pugh 1993; Krivo and Peterson 1996; Hannon 2002; Bjerk, 2007; Stults 2010). Hsieh and Pugh (1993) provide a useful meta analysis, summarizing much of this work.

Given this relationship between poverty and crime, we would expect crime rates to rise in a neighborhood when voucher holders move in if the voucher holders have lower incomes than the previously existing residents. Alternatively, 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.

Second, voucher holders may increase not only poverty in a neighborhood but also income diversity or inequality. Many studies of neighborhood crime find that greater income inequality leads to more 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, finds that the significant association between crime and poverty might actually be picking up a more robust relationship between inequality and poverty. Some posit that this relationship exists because wealthier households and their property present targets to low-income households. Others have argued that neighborhoods containing people of diverse backgrounds and limited shared experiences and perspectives are typically characterized by greater social disorganization, which can reduce social control and lead to increases in crime (Shaw and McKay 1942).

Third, a growth in the voucher population might simply increase turnover in a community, which may also lead to elevated crime through social disorganization, as social networks and norms are broken down. Of course, the in-movement of voucher households can be both a cause and a symptom of residential instability, as larger out-migration from a neighborhood opens up opportunities for voucher holders.

Finally, Rosin (2008) proposes a fourth mechanism, suggesting that housing voucher holders who have moved from demolished public housing developments often take with them the gang and other criminal networks that they developed there. There is considerable evidence that crime rates are abnormally high in many public housing developments (Goering et al 2002; Hanratty, McLanahan, and Pettit 1998; Rubinowitz and Rosenbaum 2000). Thus it is possible that residents using vouchers to leave public housing are more likely to commit crimes—or have friends who are more criminogenic—than the individuals already living in the voucher holders' chosen destination neighborhoods.

In sum, there are a number of relevant theories that suggest that a growth in the number of housing voucher households in a neighborhood could lead to an increase in crime. These mechanisms, and much of the extant literature analyzing these effects, suggest that the hypothesis that additional voucher households in a neighborhood could increase crime is a plausible one. However, none of these studies have directly tested the affect that vouchers or other subsidized households have on neighborhood crime rates, which is the focus of this paper. In the next section, we will summarize the empirical evidence from the studies that have directly estimated these effects.

Empirical Evidence on Subsidized Housing and Crime

There are several papers exploring the effect that other types of subsidized housing have on crime. Most estimate the simple association between the presence of traditional public housing and neighborhood crime. As noted already, many studies find that distressed public housing developments have extremely high levels of crime (Goering et al 2002; Hanratty, Pettit, and McLanahan 1998; Rubinowitz and Rosenbaum 2000). But studies that examine the relationship between crime and public housing more generally find more mixed results. Roncek, Bell, and Francik (1981), for example, examine the areas surrounding 17 public housing developments in Cleveland, and find that nearby blocks have significantly higher crime rates than other blocks, and that the size of the housing project is related to the block-level crime rate. However, when they control for other socioeconomic variables, they find that proximity to public housing was only a minor factor contributing to a block’s crime rate.

Farley (1982) examines crime data in St. Louis from 1971 to 1977 in the areas that contain the city’s ten largest public housing developments. He finds that the crime rates on the blocks containing public housing are no different from what would be expected given the demographic composition of these areas.

McNulty and Holloway (2000) use crime data from 1990 to 1992 and 1990 census and public housing data to examine race, public housing, and crime in 435 Atlanta block groups. The authors find that neither proximity to public housing nor racial composition is correlated with crime rates, but areas with public housing and largely black populations have significantly higher violent crime rates, suggesting an interactive effect between race and public housing.

Perhaps more relevant to our analysis, a few studies actually study how the creation of new subsidized housing affects crime levels. For example, Goetz, Lam, and Heitlinger (1996) examine how converting and creating scattered-site public housing affects crime in the surrounding neighborhoods of Minneapolis. The authors find that police calls from the developments’ locations actually decrease after the creation of the new subsidized housing. However, they also find some evidence that as the developments age, nearby crime increases.

Galster et al. (2003) also study how scattered-site public housing affects crime using time-series data. The authors find no evidence that the creation of either dispersed public housing or supportive housing affects crime rates in Denver.

Freedman and Owens (2010) look at the extent to which the Low-Income Housing Tax Credit (LIHTC) affects crime at the county level. 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. They find that

LIHTC developments lead to decreases in violent crime at the county level, but there are no effects on property crime.

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, suggesting that the police may have focused crime control efforts on these areas. The authors estimate a set of spatial regressions, controlling for spatial autocorrelation, and find that clusters of voucher households are associated with higher rates of crime. However, they find that changes in crime have no relationship to the change 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 suggest 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, 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). Collectively, these constraints may lead voucher households to choose neighborhoods that either have high crime rates or are experiencing increases in crime, due to broader trends of neighborhood decay. In related work, we find that voucher households occupy neighborhoods with higher than average crime rates (Lens, Ellen, and O'Regan 2011).[ii] Thus, in our analysis of impacts, we will attempt to control for the fact that voucher holders tend to locate in high crime areas.

Taken as a whole, the empirical evidence on the extent to which voucher households affect neighborhood crime is quite scarce. There is a larger body of literature that analyzes the effect of other subsidized households on crime, but much of that literature is dated and focuses on large public housing developments, providing limited insight about the extent to which voucher households might affect neighborhood crime.

III. Data and Methods

We use a number of different data sources for our analyses, spanning several cities and years. First, the U.S. Department of Housing and Urban Development (HUD) provided us with household-level data on voucher holders and public housing tenants nationwide from 1995 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 specified year. Our crime data consist of a unique set of annual, neighborhood-level crime data in 10 U.S. cities, which cover portions of the 1995 to 2008 period. Table 1 displays the years in which crime data are available in each city. We have crime data for each of the 14 years for three cities (Austin, Chicago, and Indianapolis) and crime data for all but one year in Cleveland, Denver, and Seattle. (In the case of Cleveland, we use 1997 and 1999 crime data to estimate the missing 1998 crime rates with a linear interpolation.) For all other cities, we have crime data for between five and eight years.

Unfortunately, we are also missing housing voucher data in some years in some cities. We have no data on vouchers for Philadelphia and Seattle data from 2002 through 2006, and incomplete data in Chicago, Cleveland, and Indianapolis for 1995 and 1996. Appendix Table A—1 displays the tract counts by city and year for the 10 cities in the sample.

Table 1: Crime data by city by year

	95	96	97	98	99	00	01	02	03	04	05	06	07	08
Austin	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Chicago	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Cleveland	X	X	X		X	X	X	X	X	X	X	X	X	X
Denver*	X	X	X	X	X	X	X	X	X	X	X	X	X	
Indianapolis***	X	X	X	X	X	X	X	X	X	X	X	X	X	X
New York										X	X	X	X	X
Philadelphia**				X	X	X	X	X	X	X	X	X		
Portland	X	X	X	X	X	X	X	X						
Seattle	X	X	X	X	X	X	X	X	X	X	X	X	X	
Washington, DC						X	X	X	X	X	X	X	X	

*Crime available at the neighborhood-level, where neighborhoods are typically aggregates of two or three census tracts.

**No homicide or rape data.

***Crime data missing for half of the tracts. The tracts included represent just under half of Indianapolis' population.

Crime data were collected 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[iii]), 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[iv]). 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.[v] 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.)

We merged tract-level counts of voucher and public housing households to the crime data and created a panel data set spanning the city-years for which we have tract-level crime data and housing assistance data. We have access to only a limited number of control variables that are available annually at the census tract level, but we have collected a number of variables that we think may help to provide a more precise estimate of the relationship between voucher locations and crime. First, we control for the number of public housing and Low Income Housing Tax Credit (LIHTC) units in a tract in a given year, using the data provided by HUD. Second, we control for tract-level demographic characteristics, including the poverty rate, homeownership rate, and racial composition using decennial census data and the American Community Survey (ACS). Because these data are only available for 1990, 2000, and 2005 to 2009 (average), we linearly interpolate the decennial and ACS data, using the bookend years.[vi] We also include census tract fixed effects to control for time-invariant differences across census tracts, and include separate year dummy variables for Census Public Use Microdata Areas (PUMAs) to control for crime trends in larger, sub-city areas. Note that PUMAs are substantially larger than census tracts. PUMAs house at least 100,000 people, while census tracts house about 4,000 people on average.

Working with administrative data brings some 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. Particularly in the early years of the data set, HUD was not able to geocode all of the addresses collected by the housing authorities. HUD researchers estimate that the tract ID is missing and irretrievable for about 15 to 20 percent of the cases from about 1995 to 1997, but this gradually improves over the time period to about six percent of the cases by 2008. We have no way to account for fluctuations in voucher counts that are attributable to missing data in a given year. We drop the city-years where obvious undercounts occurred, as discussed above. While these coverage gaps will add measurement error, we do not expect that they are related in any way to neighborhood crime.

Descriptive Statistics

Table 2 displays population-weighted means for the full sample of census tract-years. The average crime rate was about 74 crimes per 1000 persons, with substantial variation. As a comparison, the 2000 crime rate for all core cities of metropolitan areas was 75.8 per 1000 persons, so the neighborhoods in these cities appear fairly comparable to others with respect to their crime rates. Property crimes (burglary, larceny, theft, and motor vehicle theft) are the most common types of crime, with violent crimes (murder, rape, robbery, and aggravated assault) occurring much less frequently. On average, the tracts house 47.5 voucher holders, 43.8 public housing tenants, and 31.3 LIHTC households. As for demographics, the tracts in the sample were about 45 percent white, 29 percent non-Hispanic black, and 22 percent Hispanic. Their average poverty rate was 19 percent. These proportions are quite comparable to those found in all large cities in the U.S. as of the 2000 census.

Table 2: Average tract characteristics

Sample: All cities, all years

Variable	N	Mean	Std Dev	Minimum	Maximum
Total crime, count	30656	258	222	0	4,819
Violent crime, count	30460	48	48	0	462
Property crime, count	30460	160	170	0	4,372
Total crime, rate per 1000 persons	30656	85	121	0	5,133
Violent crime, rate per 1000 persons	30460	16	19	0	428
Property crime, rate per 1000 persons	30460	55	93	0	3,592
Voucher units	30732	37	68	0	1,331
Public housing units	30732	35	164	0	3,264
LIHTC units	30732	27	96	0	1,837
Population	30732	3,768	2,543	200	29,573
Percent Hispanic	30732	0.200	0.236	0	1
Percent non-Hispanic black	30732	0.342	0.376	0	1
Percent non-Hispanic white	30732	0.430	0.331	0	1
Poverty rate	30732	0.209	0.147	0	0.970
Homeownership rate	30732	0.439	0.240	0	1

Methods

The maps in Appendix B seem to reveal a correlation (albeit imperfect) between the presence of voucher holders and crime. But our aim is to test whether the presence of voucher holders precipitates crime. Identifying a causal relationship between voucher use and crime is tricky at best. We begin by modeling the number of crimes in a tract during a specified year as a function of the number of households with vouchers in that tract in the prior year, while controlling for other time-varying census tract characteristics, such as population, the number of public housing and LIHTC units, poverty, homeownership, and racial composition. Lagging voucher counts can help to wash out some of the potential reverse causal relationship (that is, that voucher holders tend to live in higher crime areas). Measuring crime and voucher holders in the same period exacerbates problems of interpreting causation. In addition, if we measure voucher counts and crime counts contemporaneously, some of the crimes we count may actually occur prior to the entry (or exit) of voucher holders because our dependent variable measures all crime in a tract over the year, including crimes committed quite early in the year, while our count of vouchers captures the number of vouchers in a tract at the end of a year. Lagging the voucher counts also allows some time for crime rates to change after changes in voucher locations—and thus may yield a more accurate effect. We also include census tract fixed effects in all models, to control for unobserved baseline differences across neighborhoods. (We essentially include a dummy variable for each census tract in the sample.)

To control for broader trends in crime, we include year dummies. In models 1b and 1c, we also include tract crime in the prior year, to control for recent trends in crime rate in the neighborhood and account for the possibility that voucher holders may tend to move into tracts where crime is rising. Finally, in model 1c, we replace the year dummy variables with PUMA*year fixed effects, to control more precisely for crime trends in the PUMA in which the census tract is located. (Note that we also estimated pooled, ten-city models with city*year fixed effects to control for crime trends in the particular city in which the census tract is located.)

The baseline models are as follows:

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

$$(1b)\ Crime_{it} = b_0 + b_1 Voucher_{it-1} + b_2 X_{it} + b_3 Crime_{it-1} + \lambda_i + T_t + e_{it}$$

$$(1c)\ Crime_{it} = b_0 + b_1 Voucher_{it-1} + b_2 X_{it} + b_3 Crime_{it-1} + \lambda_i + PUMA_i * T_t + e_{it}$$

where $Crime_{it}$ indicates the crime rate or count in tract i in year t, $Voucher_{it}$ represents the number of voucher holders in tract i in year t, X_{it} represents other time-varying characteristics of tract i in year t. λ_i is a census tract fixed effect, T_t is a vector of year fixed effects (and $PUMA_i$*T_t is a vector of PUMA*year fixed effects), and e_{it} 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.

As a robustness test, we also estimate these same specifications using voucher counts measured in year t, rather than year t-1. As noted, the results of these models should be interpreted cautiously, given that changes in crime are likely to precede changes in voucher counts. These models can be expressed as:

$$(2a)\ Crime_{it} = b_0 + b_1 Voucher_{it} + b_2 X_{it} + \lambda_i + T_t + e_{it}$$

$$(2b)\ Crime_{it} = b_0 + b_1 Voucher_{it} + b_2 X_{it} + b_3 Crime_{it-1} + \lambda_i + T_t + e_{it}$$

$$(2c)\ Crime_{it} = b_0 + b_1 Voucher_{it} + b_2 X_{it} + b_3 Crime_{it-1} + \lambda_i + PUMA_i * T_t + e_{it}$$

It is still possible that the tracts where voucher holders tend to (are able to) locate are those that are declining and experiencing a number of changes that contribute to upward trends in crime. Unfortunately, we are not able to observe and control for all the factors that might contribute to both the location of voucher holders and the crime experienced in the neighborhood. However, a good predictor of such changes would be the presence of voucher holders in the future. Thus, we also estimate a model in which we include future voucher holders on the right hand side, as well as lagged voucher holders, to test if any correlation between past voucher holders and crime exists after controlling for future count of voucher holders. In the presence of the future voucher holder variable, a positive coefficient on voucher holders would suggest that voucher holders do lead to increased crime, after controlling for the unobserved changes in tracts that tend to attract voucher holders. Specifically, we estimate the following models:

$$(3a)\ Crime_{it} = b_0 + b_1 Voucher_{it-1} + b_2 X_{it} + b_3 Crime_{it-1} + b_4 Voucher_{it+1} + \lambda_i + PUMA_i * Tt + e_{it}$$

$$(3b)\ Crime_{it} = b_0 + b_1 Voucher_{it-1} + b_2 X_{it} + b_3 Voucher_{it+1} + \lambda_i + PUMA_i * T_t + e_{it}$$

(Note that we also estimate these same regressions with contemporaneous (and future) voucher counts on the right hand side as models 3c and 3d.)

To more formally test for a reverse causal relationship (crime attracts voucher holders), we run the following regression to isolate the role of reverse causality as a potential source of bias:

$$(4)\ Voucher_{it} = b_0 + b_1 Voucher_{it-1} + b_2 X_{it} + b_3 Crime_{it-1} + \lambda_i + PUMA_i * T_t + e_{it}$$

If the coefficient on *past* crime, b_3, is significant, this suggests that voucher holders tend to move into high-crime neighborhoods, so that any associations we see between voucher holders and crime may be due to this link rather than proving that voucher holders tend to increase neighborhood crime.

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, specifically by including the number of vouchers plus the number of vouchers squared on the right hand side. This regression can be expressed as follows:

$$(5)\ Crime_{it} = b_0 + b_1 Voucher_{it-1} + b_2 Vouchersquared_{it-1} + b_3 X_{it} + \lambda_i + PUMA_i * T_t + e_{it}$$

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

$$(6a)\ Crime_{it} = b_0 + b_1 Voucher_{it-1} + b_2 HighCrime * Voucher_{it-1} + b_3 X_{it} + \lambda_i + PUMAi * T_t + e_{it}$$

$$(6b)\ Crime_{it} = b_0 + b_1 Voucher_{it-1} + b_2 LowCrime * Voucher_{it-1} + b_3 X_{it} + \lambda_i + PUMA_i * T_t + e_{it}$$

IV. Results

The simple bivariate correlation between total crime counts and voucher household counts for the full sample is 0.27. So there does seem to be a positive relationship between the presence of voucher holders and crime—tracts with higher crime rates also house more voucher holders on average. But of course our interest is not on a simple association, but rather whether the presence of voucher holders actually increases crime.

Table 3 displays results from our first two sets of models of crime counts, estimated on the pooled sample. As mentioned in the previous section, we control for a variety of tract characteristics, including population, racial composition, poverty, and the number of Low-Income Housing Tax Credit (LIHTC) and public housing households. All models include tract fixed effects, and either year or PUMA*year effects for time varying factors.

To ensure that the entry of all voucher holders precedes the crimes that occur, model 1 regresses crime rates in year t on voucher counts in year t-1. Versions (a) and (b) include census tract and year fixed effects. In Model (1a), the coefficient on voucher households is positive and significant, suggesting that an additional voucher household in the tract is associated with an increase of approximately 0.1 crimes in a tract. To control for pre-existing trends in tract-level crime, model 1b includes lagged crime on the right hand side. Once this control is added, the coefficient on voucher counts declines greatly and is no longer significant. We have also run models 1a and 1b with city*year effects, to control for city-specific time variant factors, such as city crime rates. The coefficient on vouchers declines by about one half, and is not significantly different from zero in either model. Model 1c includes PUMA*year effects, to control for trends in the broader area of the city. The coefficient on voucher again declines precipitously, and remains insignificant.[vii]

Model 2 provides the alternative specification of voucher counts, those contemporaneous with crime rates. As noted, this specification has some drawbacks and should be interpreted cautiously. In 2a and 2b, the coefficient on current voucher counts is now statistically significant, at a magnitude similar to the coefficient shown in 1a. When city*year effects are included, the coefficient on vouchers declines by a third, but remains significant. However, once we control for trends in a tract's broader environment (by including PUMA*year fixed effects), the coefficient on the number of voucher households declines by more than half and loses significance. This suggests that the association between contemporaneous voucher counts and crime may be driven by changes in the broader area (which are correlated with changes in tract characteristics, including demographics and voucher counts.)

In terms of other control variables, the signs are generally consistent with expectations and previous work. In versions (a) and (b) of both models, crime rates are higher in tracts with more public housing units, higher poverty, greater share minorities (both black and Hispanic)[viii] and higher vacancy rates; crime rates are lower in tracts with more owner occupied housing. Once PUMA*year effects are added, these coefficients are no longer significant. The only tract level variables that remain significant are population and, in version (c), lagged crime.

Table 3: Baseline regression results

Dependent variable: Number of crimes in tract in year

Sample: All cities

Variable	1a.	1b.	1c.	2a.	2b.	2c.
Voucher Counts t-1	0.112**	0.045	0.005			
	(0.051)	(0.029)	(0.028)			
Voucher Counts t				0.136**	0.118***	0.052
				(0.057)	(0.036)	(0.035)
Log population	47.8**	24.6**	31.8***	47.2**	23.8**	31.2***
	(18.7)	(10.1)	(8.7)	(19.5)	(10.1)	(8.8)
Tract Crime Counts t-1		0.480***	0.432***		0.478***	0.431***
		(0.0450)	(0.057)		(0.049)	(0.057)
Public Housing Counts	0.053***	0.026***	0.012	0.050***	0.025***	0.012
	(0.014)	(0.008)	(0.009)	(0.012)	(0.009)	(0.009)
LIHTC Counts	-0.009	0.010	-0.009	-0.022	0.007	-0.010
	(0.028)	(0.014)	(0.014)	(0.031)	(0.014)	(0.014)
Percent Poverty	59.5***	33.4***	1.6	63.0***	33.4***	1.7
	(20.5)	(10.8)	(10.8)	(21.1)	(10.8)	(10.8)
Percent Black	64.6**	26.9*	24.9	74.0***	26.6*	23.4
	(26.8)	(15.5)	(15.2)	(25.5)	(15.5)	(15.2)
Percent Hispanic	100.2***	52.4***	28.5	86.4**	50.3***	27.0
	(35.9)	(19.4)	(20.0)	(34.0)	(19.3)	(19.8)
Percent Owner-Occ	-97.1**	-42.7**	-6.2	-111.1***	-38.9*	-4.6
	(37.9)	(20.4)	(19.7)	(37.0)	(20.6)	(19.8)
Percent Vacant Units	75.3**	34.2*	-7.4	75.5**	34.8*	-7.2
	(32.3)	(18.5)	(18.2)	(32.9)	(18.6)	(18.3)
Constant	-93.4	-46.0	-123.9*	-23.9	-42.7	-119.9*
	(155.3)	(78.0)	(65.5)	(160.8)	(77.6)	(65.8)
Tract FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	No	Yes	Yes	No
Puma*Year FEs	No	No	Yes	No	No	Yes
Observations	26,419	26,419	26,413	30,656	26,419	26,413
Number of Tracts	4,235	4,235	4,234	4,237	4,235	4,234
Adjusted R-squared	0.085	0.299	0.394	0.121	0.300	0.395

Robust standard errors in parentheses.

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

These results suggest that the simple bivariate correlation between crime and vouchers may primarily be driven by other correlated factors, at the tract level (such as pre-existing trends in neighborhood crime rates, model versions (b)) and crime trends in the broader area (versions (c)). The only evidence of a relationship between crime and voucher counts comes from the contemporaneous models, a specification that raises questions of the direction of causation.

To address these questions more directly, Table 4 presents results from an intuitive causality test, in which we regress crime in year t on voucher counts either in year t-1 (models 3a-3b) or in year t (models 3c-3d). In addition, we add the number of vouchers that will be in the tract in year t+1. Clearly, voucher holders who have not yet entered the tract cannot be causing crime in time t. They may, however, be a good indicator of other (unobservable) trends that affect both vouchers and crime in time t.

We see in Table 4 that once voucher counts in the future are included in the model, the coefficients on current or lagged voucher counts are never significant. This strongly suggests that it is not the presence of voucher holders *per se* in a tract that leads to an increase in crime rates, and that voucher holders may enter neighborhoods whose crime rates may be high or increasing.

We test for evidence of such reverse causality by modeling the number of voucher counts in a tract in time t as a function of voucher counts in t-1, other tract characteristics, and crime in t-1. Table 5 shows these results for the pooled model. When we lag crime on the right-hand side to predict shifts in voucher counts, we find a strong positive and significant relationship, even after controlling for existing voucher counts in t-1 and other demographic characteristics. This suggests that voucher counts tend to increase in neighborhoods that had high crime rates even before the voucher counts increased.

As noted previously, it is possible that the relationship between vouchers and crime is non-linear. Such misspecification could lead to our “non findings.” We test for such non-linearities by adding a squared voucher count term to our models, permitting an additional voucher in a tract to have a different effect depending on whether the initial voucher counts are low or high. Table 6 presents results of such a quadratic model with with lagged voucher counts. Voucher counts are not significant.. The weak relationship between crime and voucher counts in models with full controls in Table 3 does not appear to arise from use of a linear specification.

Finally, we also consider whether the relationship between vouchers and crime varies by context. Specifically, whether the coefficient on voucher counts depends on whether the tract has a high or low crime rate. We estimate models that include voucher counts, and an interaction between voucher counts and whether the tract is ranked in the top (or bottom) quartile of crime rates over the time period. Lagged voucher counts are insignificant in all models, as are all interactions.

Table 4: Testing causality #1

Dependent variable: Number of crimes in tract in year

Sample: All cities

Variable	3a.	3b.	3c.	3d.
Voucher Counts t-1	0.001	0.019		
	(0.038)	(0.046)		
Voucher Counts t			0.034	0.026
			(0.040)	(0.037)
Voucher Counts t+1	0.152***	0.253**	0.141***	0.198***
	(0.058)	(0.106)	(0.052)	(0.050)
Log Population	34.1***	55.0***	33.7***	56.6***
	(11.7)	(17.8)	(11.8)	(9.8)
Tract Crime Counts t-1	0.403***		0.403***	
	(0.059)		(0.059)	
Public Housing Counts	0.028**	0.049***	0.028**	0.046***
	(0.011)	(0.019)	(0.011)	(0.011)
LIHTC Counts	-0.035	-0.065	-0.036	-0.069**
	(0.026)	(0.045)	(0.027)	(0.033)
Percent Poverty	-5.4	0.1	-5.4	7.7
	(13.6)	(22.9)	(13.6)	(13.4)
Percent Black	33.0*	40.8	31.9	34.1*
	(19.3)	(31.2)	(19.4)	(18.3)
Percent Hispanic	39.7*	55.8	38.7	23.4
	(24.0)	(39.2)	(24.0)	(21.2)
Percent Owner-Occ	-4.4	-9.5	-3.8	-28.6
	(27.6)	(45.1)	(27.7)	(22.0)
Percent Vacant Units	10.6	24.2	10.4	23.2
	(23.3)	(36.1)	(23.4)	(19.8)
Constant	-145.8	-215.3	-144.8	-205.6**
	(91.4)	(150.5)	(92.0)	(81.7)
Tract FEs	Yes	Yes	Yes	Yes
Year FEs	No	No	No	No
Puma*Year FEs	Yes	Yes	Yes	Yes
Observations	30,656	26,419	26,419	26,132
Number of Tracts	4,237	4,235	4,235	4,234
Adjusted R-squared	0.121	0.300	0.085	0.264

Robust standard errors in parentheses.

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

Table 5: Testing causality #2

Dependent variable: Number of voucher units in tract in year

Sample: All cities

Variable	4.
Tract Crime Counts t-1	0.017***
	(0.006)
Voucher Counts t-1	0.344***
	(0.064)
Log Population	9.5**
	(3.7)
Public Housing Counts	0.0004
	(0.0028)
LIHTC Counts	0.022***
	(0.007)
Percent Poverty	-2.4
	(4.4)
Percent Black	25.1***
	(7.3)
Percent Hispanic	24.9**
	(9.7)
Percent Owner-Occ	-24.8***
	(6.4)
Percent Vacant Units	-6.5
	(6.1)
Constant	-56.6*
	(31.0)
Tract FEs	Yes
Year FEs	No
Puma*Year FEs	Yes
Observations	26,132
Number of Tracts	4,234
Adjusted R-squared	0.383

Robust standard errors in parentheses

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

Table 6: Regression results: testing for non-linearities

Dependent variable: Number of crimes in tract in year

Sample: All cities

Variable	5.	6a.	6b.
Voucher Counts t-1	0.0789	0.0362	0.0470
	(0.0573)	(0.0312)	(0.0464)
Lag Voucher Counts Squared	-5.64e-05		
	(5.08e-05)		
Lag Vouchers*Tract Crime Top Quartile		0.0224	
		(0.109)	
Lag Vouchers*Tract Crime Bottom Quartile			-0.0239
			(0.0754)
Log Population	53.07***	53.43***	53.50***
	(13.77)	(7.469)	(7.429)
Public Housing Counts	0.0277*	0.0276***	0.0276***
	(0.0142)	(0.00944)	(0.00943)
LIHTC Counts	-0.0322	-0.0316*	-0.0316*
	(0.0274)	(0.0183)	(0.0183)
Percent Poverty	4.908	5.004	4.879
	(18.53)	(10.94)	(10.95)
Percent Black	35.74	36.57**	36.75**
	(26.03)	(15.48)	(15.40)
Percent Hispanic	46.08	46.88**	47.04**
	(35.02)	(19.28)	(19.30)
Percent Owner-Occ	-13.85	-14.63	-14.39
	(33.87)	(17.42)	(17.53)
Percent Vacant Units	2.479	2.539	2.554
	(30.31)	(16.36)	(16.38)
Constant	-186.2	-189.6***	-195.2***
Tract FEs	Yes	Yes	Yes
Year FEs	No	No	No
Puma*Year FEs	Yes	Yes	Yes
Observations	26413	26,413	26,413
Number of Tracts	4234	4,234	4,234
Adjusted R-squared	0.250	0.271	0.271

Robust standard errors in parentheses.

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

The relationship between crime and vouchers might also vary by the broader context of the city. While these pooled results provide our strongest evidence for generalizable findings, they may mask significant heterogeneity across cities. To test for such differences, we have estimated models independently for the three cities with the most complete tract-level crime and voucher data for our time period—Austin, Chicago, and Cleveland. Appendix Tables A2 to A4 show the results for these three cities. In the interest of brevity, we show only results that include PUMA*year fixed effects. (Full results are available from authors upon request.) These three cities reveal that the voucher-crime relationship might well vary by context, in complicated ways.

The only evidence of a positive relationship between crime and vouchers comes from Cleveland (A—2). In both model (1c) (with lagged voucher counts) and in model 2c (contemporaneous voucher counts), we obtain positive and significant coefficients on voucher counts. When we test for alternative causal channels by including future voucher counts (model 3), contemporaneous voucher counts becomes insignificant but lagged voucher counts remain significant. Testing for a reverse causal relationship between a tract's crime rate in t-1 and voucher counts in t (model 4), we find no evidence of reverse causation. Controlling for other tract characteristics, crime rates at t-1 are not associated with voucher counts in t. For Cleveland, the empirical evidence is fairly consistent with a positive causal relationship running from vouchers to crime.

Austin and Chicago each provide a different picture. In models 1c and 2c, the only significant coefficient on voucher counts is negative (on contemporaneous counts, in Austin). Neither city provides evidence of other causal pathways, with all relevant coefficients insignificant in models 3 and 4. Interestingly, there is some continued evidence of a negative relationship between voucher counts and crime in the non-linear specifications (model 5). In regressions for both cities, either the coefficient on lagged voucher counts or the coefficient on lagged voucher counts squared is negative and significant.

These individual city results might suggest that local factors (for example, housing markets or crime levels) may matter in shaping the relationship between voucher location and neighborhood crime. Therefore, our pooled results cannot eliminate the possibility that the posited positive relationship might exist in some cities or under some conditions. However, we cannot rule out the opposite relationship existing either. The results do suggest that a positive relationship is not the norm, at least in a causal way.

V. Conclusion

Through our many models, we find little evidence to support Hanna Rosin's claim in her *Atlantic Monthly* article that voucher holders invite or create crime. The findings from the pooled models suggest a relationship only when voucher counts are measured contemporaneously, and even then not in all models. Given the timing of voucher counts and crime, it is problematic to interpret the causal relationship as running from vouchers to crime. Indeed, when we examine other avenues of causation (using future voucher counts), the contemporaneous relationship is eliminated. Most significantly, there is no evidence that more voucher holders in a tract predict more crime one year later. That said, testing the causal relationship between vouchers and crime is challenging, and our examination of individual cities suggests some variation. We see evidence of a positive relationship between vouchers and crime in Cleveland, but no evidence of a positive association in Austin or Chicago (and indeed some very weak evidence of a negative correlation). Given the empirical challenges of distinguishing causation, we think readers should take these results as preliminary. Future work employing models at a smaller level of geography may allow for more precise identification.

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Endnotes

[i] 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.)

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

[iii] Garth Taylor in Chicago and Arthur O'Sullivan in Portland.

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

[v] Philadelphia was not able to share data on sexual assaults or homicides, and those crimes are thus not included in overall totals or the individual categories.

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

[vii] We get the same results in model (1c) if lagged crime is omitted.

[viii] While the relative magnitude of coefficients on black and Hispanic reverses between version (a) and (b), differences in these coefficients are not statistically significant in any model.

Appendixes

Appendix A: Data

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

City	**1995**	**1996**	**1997**	**1998**	**1999**	**2000**	**2001**	**2002**	**2003**	**2004**	**2005**	**2006**	**2007**	**2008**	**Total**
Austin	0	114	114	112	112	113	113	113	113	113	113	113	113	113	1,356
Chicago	0	0	821	821	821	821	821	820	819	819	817	814	811	0	8,191
Cleveland	0	0	210	210	210	210	210	210	210	209	208	205	204	204	2,295
Denver	0	76	76	76	76	76	76	76	76	76	76	76	76	0	1,474
Indianapolis	0	0	0	98	98	98	98	98	98	98	98	98	98	98	980
New York	0	0	0	0	0	0	0	0	0	2,120	2,118	2,117	2,117	2,117	8,472
Philadelphia	0	0	0	357	357	357	357	0	0	0	0	0	0	0	1,428
Portland	145	145	145	0	145	145	145	0	0	0	0	0	0	0	870
Seattle	116	116	116	116	116	116	116	0	0	0	0	0	116	0	928
Washington	0	0	0	0	0	180	180	180	180	180	180	0	180	0	1,260
Total	261	509	1,540	1,848	1,993	2,174	2,174	1,555	1,554	3,673	3,668	0	3,773	2,532	27,254

Table A—2: Regression results

Dependent variable: Number of crimes in tract in year

Sample: Austin

VARIABLES	1c.	2c.	3a.	3b.	4: DV: #Vouchers	5.	6a.	6b.
Voucher counts t-1		-0.300	-0.038	-0.060	0.832***	0.397	-0.315	-0.271
		(0.274)	(0.378)	(0.400)	(0.042)	(0.427)	(0.205)	(0.331)
Voucher counts	-0.439**							
	(0.207)							
Voucher counts t+1			-0.507	-0.548				
			(0.314)	(0.342)				
Lag voucher counts squared						-0.004***		
						(0.001)		
Lag voucher*mean tract rate top quartile							-0.217	
							(0.757)	
Lag voucher*mean tract rate bottom quartile								-0.198
								(0.332)
Log population	100.5***	97.0***	95.7***	107.3***	5.6**	111.8***	113.0***	115.3***
	(30.7)	(30.2)	(30.1)	(34.2)	(2.8)	(19.5)	(19.5)	(19.8)
Lag tract crime counts	0.160***	0.160***	0.119**		-0.001			
	(0.053)	(0.054)	(0.050)		(0.002)			
Public housing counts	-0.610**	-0.612**	-0.648**	-0.638**	0.007	-0.631***	-0.611***	-0.604***
	(0.245)	(0.248)	(0.283)	(0.269)	(0.011)	(0.231)	(0.232)	(0.230)
LIHTC counts	0.030	-0.005	0.150	0.167	0.074***	-0.014	-0.008	-0.004
	(0.082)	(0.081)	(0.142)	(0.151)	(0.013)	(0.063)	(0.065)	(0.062)
Percent poverty	-28.5	-22.2	-16.6	-8.8	-11.0**	-12.1	-15.5	-15.7
	(83.5)	(81.3)	(88.6)	(99.7)	(5.2)	(50.9)	(51.1)	(51.4)

Percent black	161.6	160.9	256.5**	307.5**	3.8	205.1**	218.0**	234.6**
	(107.3)	(110.3)	(116.8)	(133.8)	(19.8)	(85.4)	(86.1)	(95.6)
Percent Hispanic	252.9**	250.6**	282.8**	322.5**	-3.2	280.2***	310.7***	307.5***
	(105.1)	(104.6)	(112.7)	(127.2)	(11.4)	(77.9)	(77.1)	(77.3)
Percent owner-occ housing	-94.0	-69.9	-91.9	-106.9	-29.4**	-114.7	-82.6	-82.7
	(123.7)	(130.4)	(150.3)	(167.3)	(14.6)	(101.8)	(96.3)	(105.3)
Percent vacant units	133.9	130.0	324.3	374.8	7.7	163.6	183.8	177.6
	(221.0)	(221.6)	(228.3)	(245.0)	(16.8)	(150.2)	(152.1)	(150.7)
Constant	-628.5**	-613.6**	-618.1**	-697.7**	-25.7	-701.1***	-709.0***	-730.6***
	(274.2)	(269.6)	(272.3)	(314.9)	(28.0)	(177.4)	(178.0)	(183.2)
Tract FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	No	No	No	No	No	No	No	No
Puma*year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,355	1,355	1,239	1,239	1,354	1,355	1,355	1,355
Number of tracts	114	114	113	113	114	114	114	114
Adjusted R-squared	0.084	0.083	0.069	0.057	0.810	0.062	0.059	0.059

Robust standard errors in parentheses.

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

Table A—3: Regression results

Dependent variable: Number of crimes in tract in year

Sample: Chicago

VARIABLES	1c.	2c.	3a.	3b.	4:DV: #Vouchers	5a.	6a.	6b.
Voucher counts t-1	0.003		-0.014	-0.101	0.405***	-0.104	-0.148***	-0.070
	(0.053)		(0.059)	(0.087)	(0.051)	(0.071)	(0.051)	(0.051)
Voucher counts		0.024						
		(0.045)						
Voucher counts t+1			0.047	0.011				
			(0.042)	(0.069)				
Lag voucher counts squared						0.0001		
						(0.0001)		
Lag voucher*mean tract rate top quartile							0.190**	
							(0.092)	
Lag voucher*mean tract rate bottom quartile								-0.050
								(0.125)
Log population	7.3	7.4	5.2	15.6	-1.8	21.4**	20.8**	21.3**
	(5.8)	(5.8)	(9.0)	(23.9)	(1.9)	(9.7)	(9.7)	(9.7)
Lag tract crime counts	0.606***	0.606***	0.596***		-0.0003			
	(0.024)	(0.024)	(0.027)		(0.0048)			
Public housing counts	0.027***	0.027***	0.029***	0.072***	0.001	0.071***	0.071***	0.071***
	(0.010)	(0.010)	(0.010)	(0.028)	(0.003)	(0.018)	(0.018)	(0.018)
LIHTC counts	-0.002	-0.004	0.020	0.030	0.051***	-0.013	-0.015	-0.014
	(0.023)	(0.023)	(0.025)	(0.050)	(0.018)	(0.036)	(0.036)	(0.036)
Percent poverty	-5.3	-5.4	-8.3	-18.9	1.1	-17.3	-16.7	-17.2
	(9.8)	(9.7)	(12.4)	(30.7)	(4.4)	(13.0)	(13.0)	(13.0)
Percent black	-2.4	-3.0	3.0	-31.0	16.6**	-26.7	-26.6	-27.0
	(19.2)	(19.2)	(25.7)	(63.7)	(6.7)	(28.5)	(28.5)	(28.3)
Percent Hispanic	-23.1	-23.3	-19.8	-77.2*	7.4	-77.8***	-77.4***	-77.7***

	(15.1)	(15.1)	(18.3)	(42.3)	(4.8)	(19.8)	(19.7)	(19.9)
Percent owner-occ housing	-9.5	-9.2	-16.9	-47.6	-10.9**	-33.6**	-34.7**	-33.3**
	(12.0)	(12.0)	(15.3)	(36.9)	(5.0)	(15.8)	(15.8)	(15.9)
Percent vacant units	-8.1	-8.2	-13.3	-16.9	3.2	-8.4	-9.1	-8.3
	(15.9)	(15.8)	(21.1)	(49.7)	(6.4)	(21.1)	(21.1)	(21.1)
Constant	43.4	35.6	49.6	169.6	29.2*	114.5	123.8	125.4
	(45.3)	(45.2)	(69.9)	(192.7)	(15.3)	(79.0)	(78.9)	(79.0)
Tract FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	No	No	No	No	No	No	No	No
Puma*year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,184	8,184	7,363	7,363	8,184	8,184	8,184	8,184
Number of tracts	821	821	821	821	821	821	821	821
Adjusted R-squared	0.632	0.632	0.609	0.353	0.425	0.362	0.363	0.362

Robust standard errors in parentheses.

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

Table A—4: Regression Results

Dependent Variable: Number of Crimes in Tract in Year

Sample: Cleveland

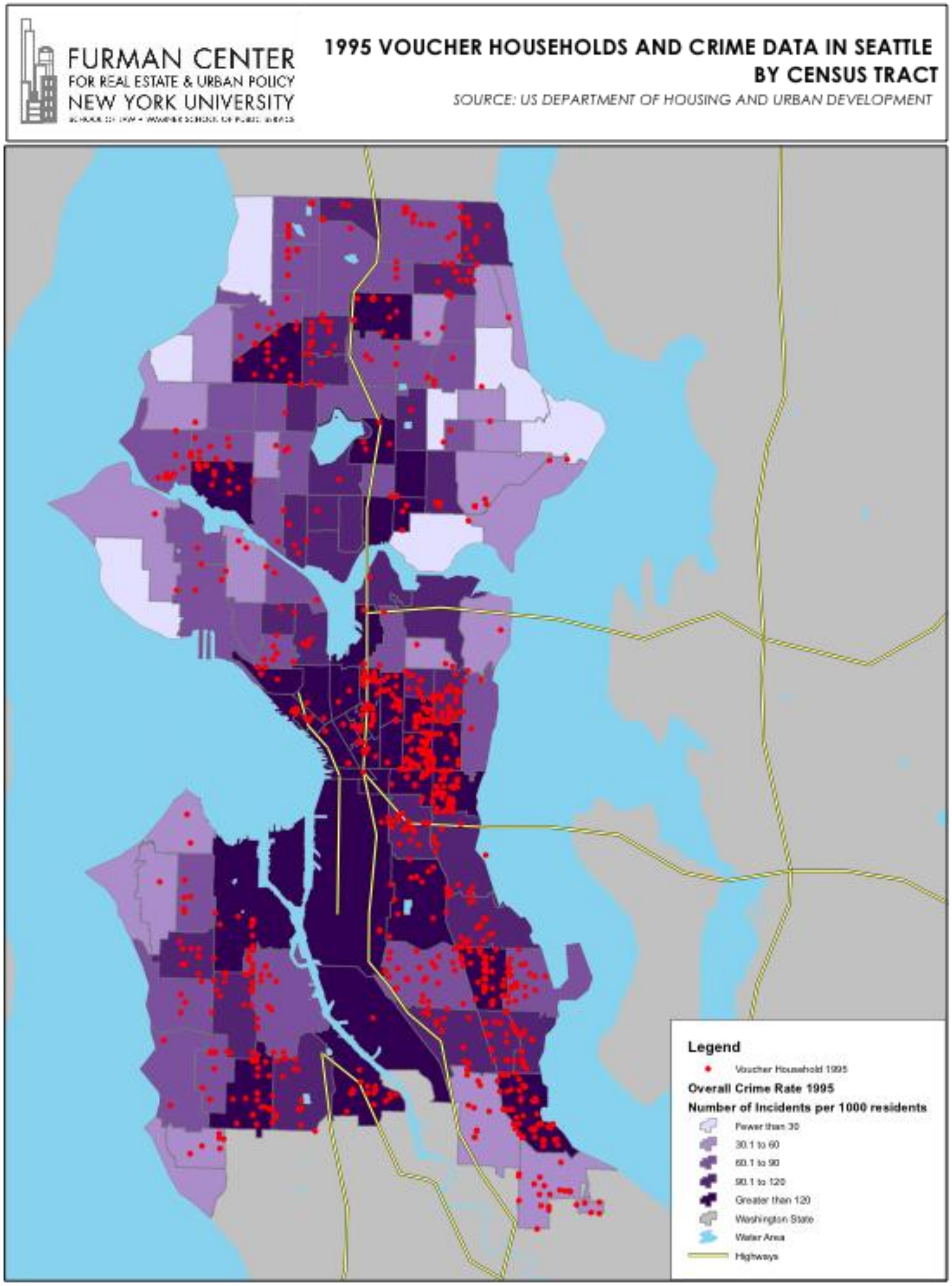
	1c.	2c.	3a.	3b.	4:DV: #Vouchers	5a.	6a.	6b.
Voucher counts t-1	0.113*		0.135*	0.200**	0.695***	0.342***	0.235***	0.191***
	(0.058)		(0.070)	(0.089)	(0.024)	(0.117)	(0.058)	(0.060)
Voucher counts		0.143**						
		(0.068)						
Voucher counts t+1			0.138*	0.186**				
			(0.073)	(0.084)				
Lag voucher counts squared						-0.001		
						(0.001)		
Lag voucher*mean tract rate top quartile							-0.388**	
							(0.154)	
Lag voucher*mean tract rate bottom quartile								0.841***
								(0.179)
Log population	2.8	2.2	4.0	8.2	5.4***	7.8	7.0	6.9
	(6.0)	(6.0)	(8.0)	(12.4)	(1.8)	(6.1)	(6.1)	(6.1)
Lag tract crime counts	0.395***	0.394***	0.389***		0.010			
	(0.041)	(0.042)	(0.040)		(0.007)			
Public housing counts	0.042***	0.041***	0.039***	0.044***	0.003*	0.050***	0.050***	0.050***
	(0.009)	(0.009)	(0.011)	(0.012)	(0.002)	(0.012)	(0.012)	(0.012)
LIHTC counts	-0.009	-0.012	-0.029***	-0.037**	0.025	-0.005	-0.012	-0.009
	(0.012)	(0.012)	(0.009)	(0.015)	(0.019)	(0.020)	(0.019)	(0.019)
Percent poverty	-3.9	-4.5	-7.2	-6.7	5.7	-6.2	-8.0	-5.3
	(15.1)	(15.1)	(17.2)	(26.5)	(3.8)	(13.8)	(14.0)	(13.8)
Percent black	39.9**	37.7*	37.4	65.5*	21.0***	64.9***	73.7***	67.8***
	(18.9)	(19.4)	(23.8)	(36.8)	(5.4)	(18.4)	(18.1)	(17.5)
Percent Hispanic	30.5	27.8	31.8	44.3	20.1***	45.9*	42.7*	34.6

	(21.3)	(21.2)	(24.1)	(34.3)	(6.4)	(24.3)	(23.8)	(24.3)
Percent owner-occ housing	-10.0	-10.3	-1.0	-18.2	1.2	-26.3	-21.5	-26.8
	(19.9)	(19.8)	(21.7)	(31.9)	(5.5)	(16.8)	(17.0)	(16.8)
Percent vacant units	29.9	31.2	36.5	68.3	-6.1	60.6**	66.0***	65.2***
	(28.6)	(28.5)	(31.0)	(48.5)	(6.2)	(25.6)	(25.0)	(25.0)
Constant	41.7	47.2	26.6	37.0	-47.7***	50.7	52.1	50.3
	(47.4)	(48.0)	(62.3)	(97.0)	(15.0)	(48.8)	(47.8)	(48.6)
Tract FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	No	No	No	No	No	No	No	No
Puma*year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,290	2,290	2,080	2,080	2,290	2,290	2,290	2,290
Number of tracts	210	210	210	210	210	210	210	210
Adjusted R-squared	0.219	0.220	0.215	0.076	0.733	0.074	0.078	0.078

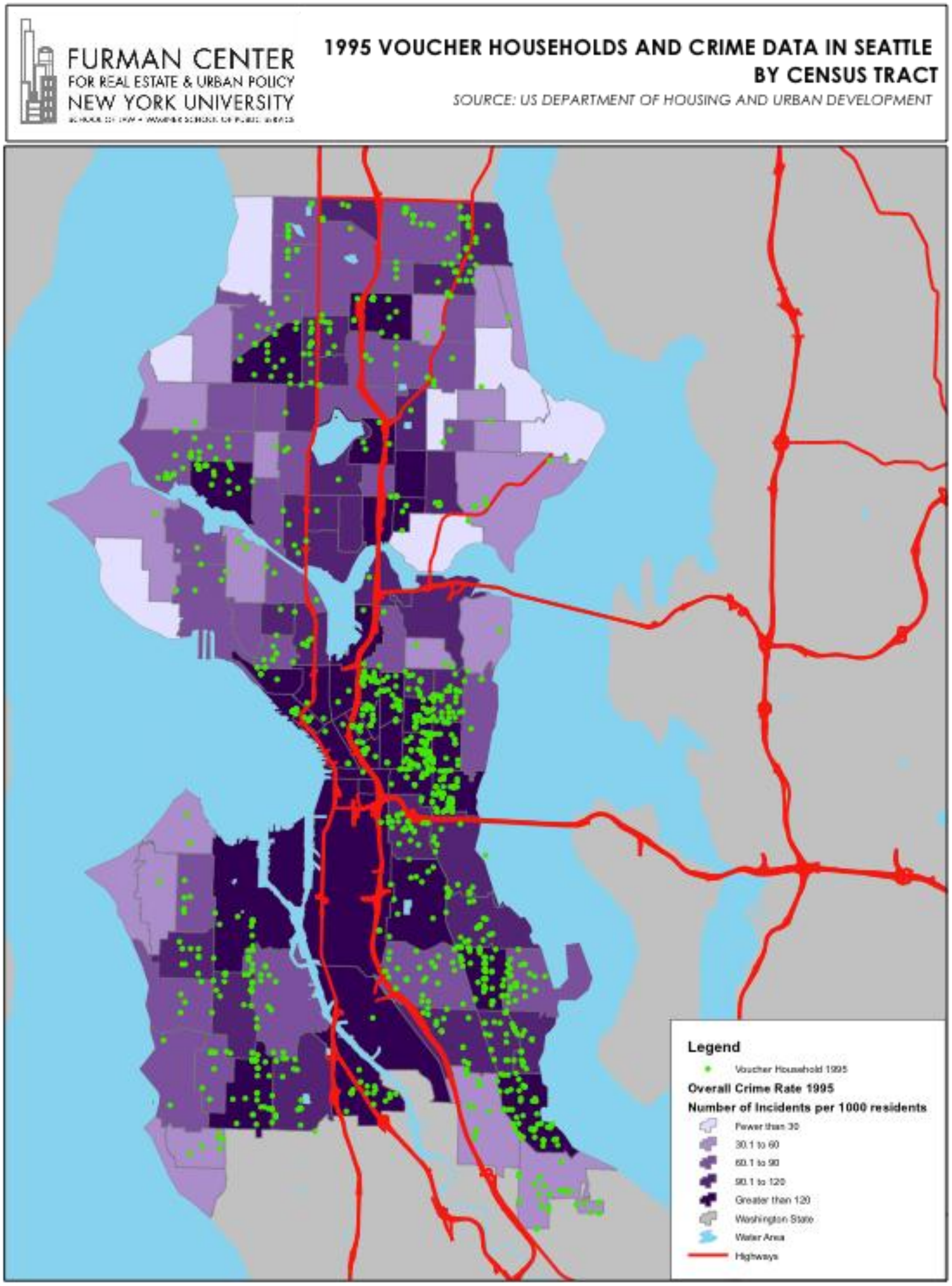
Robust standard errors in parentheses.

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

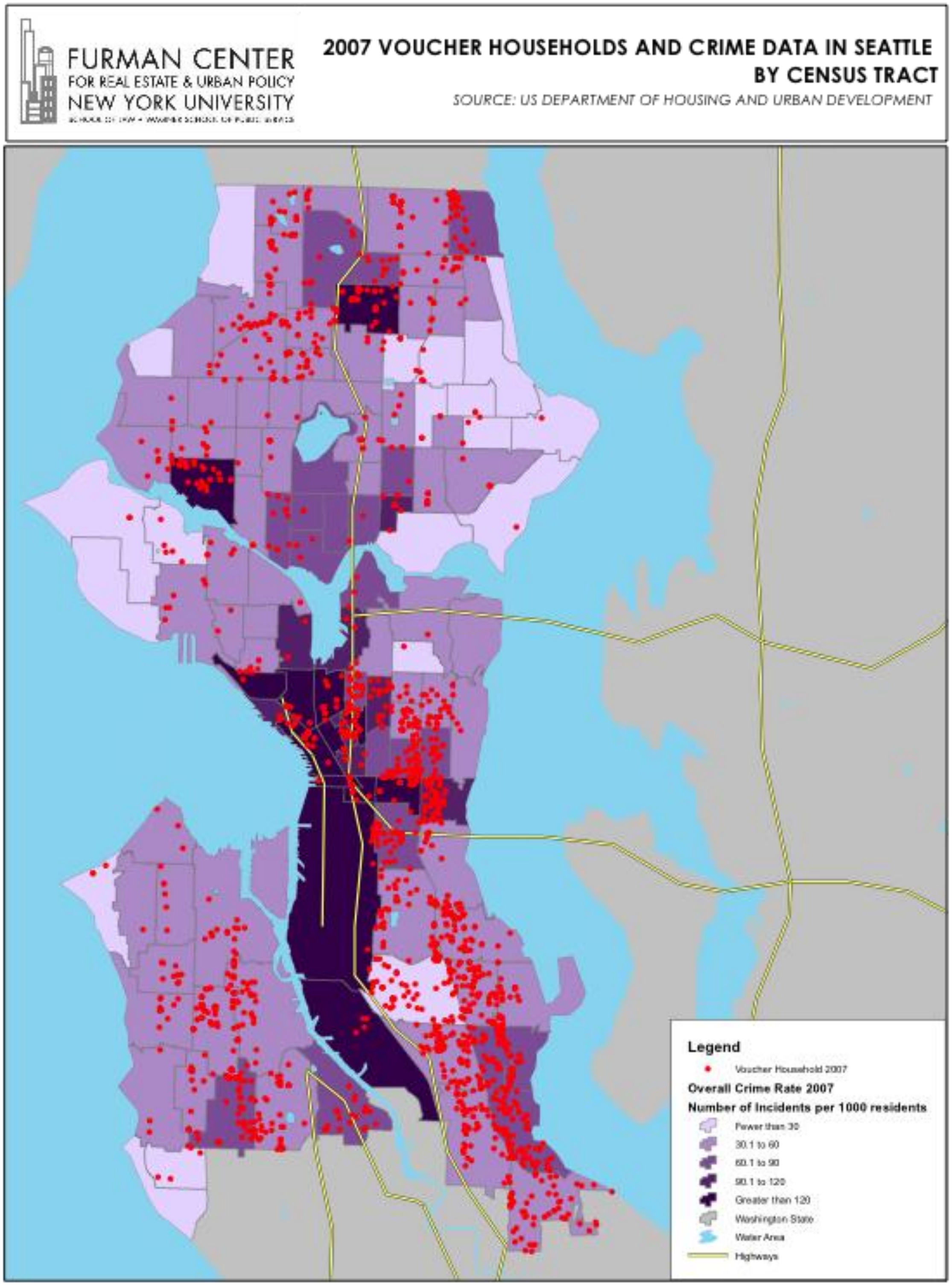
Map B—1: 1995 Voucher households and crime data in Seattle



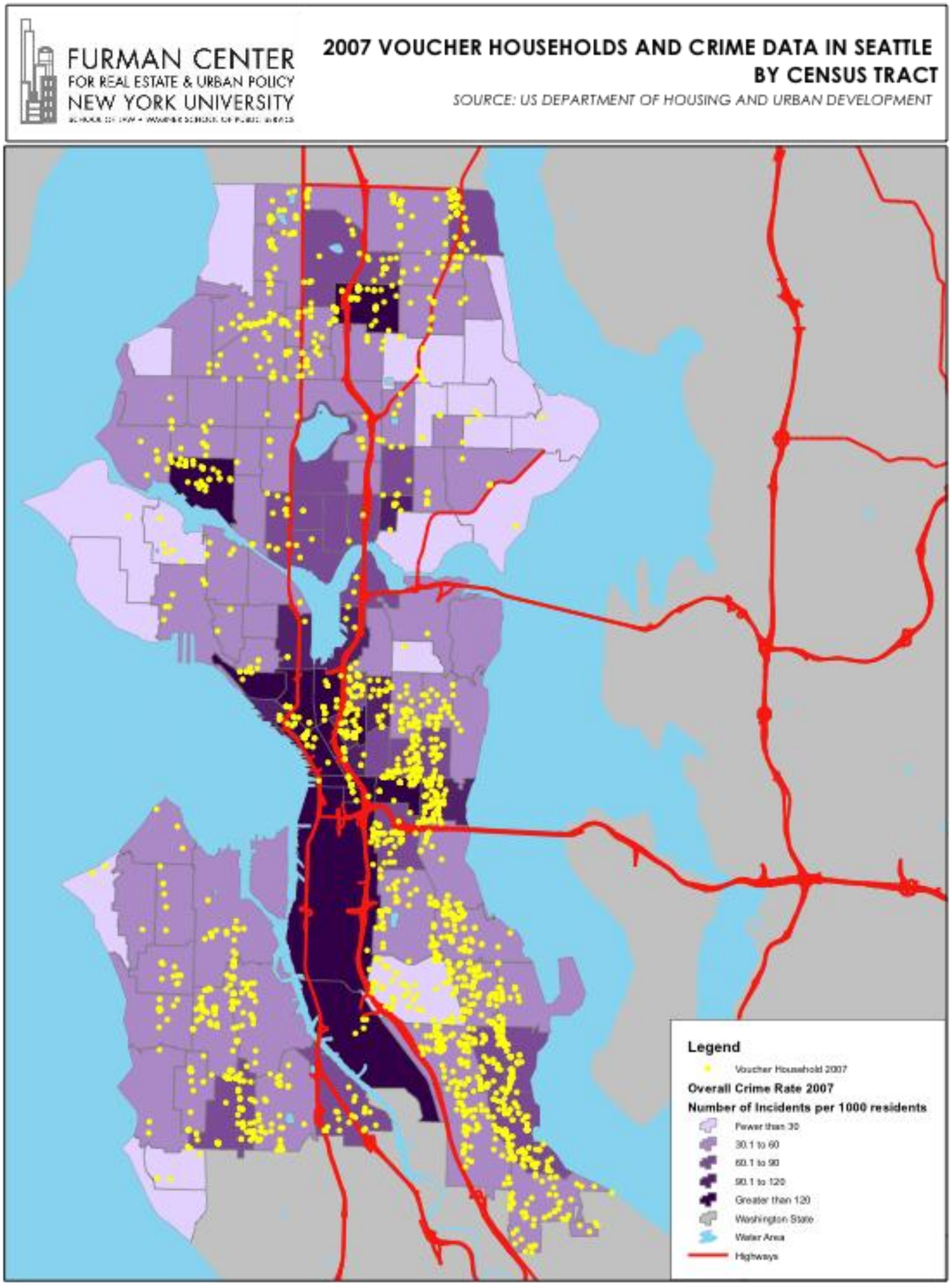
Map B—2: 1995 Voucher households and crime data in Seattle



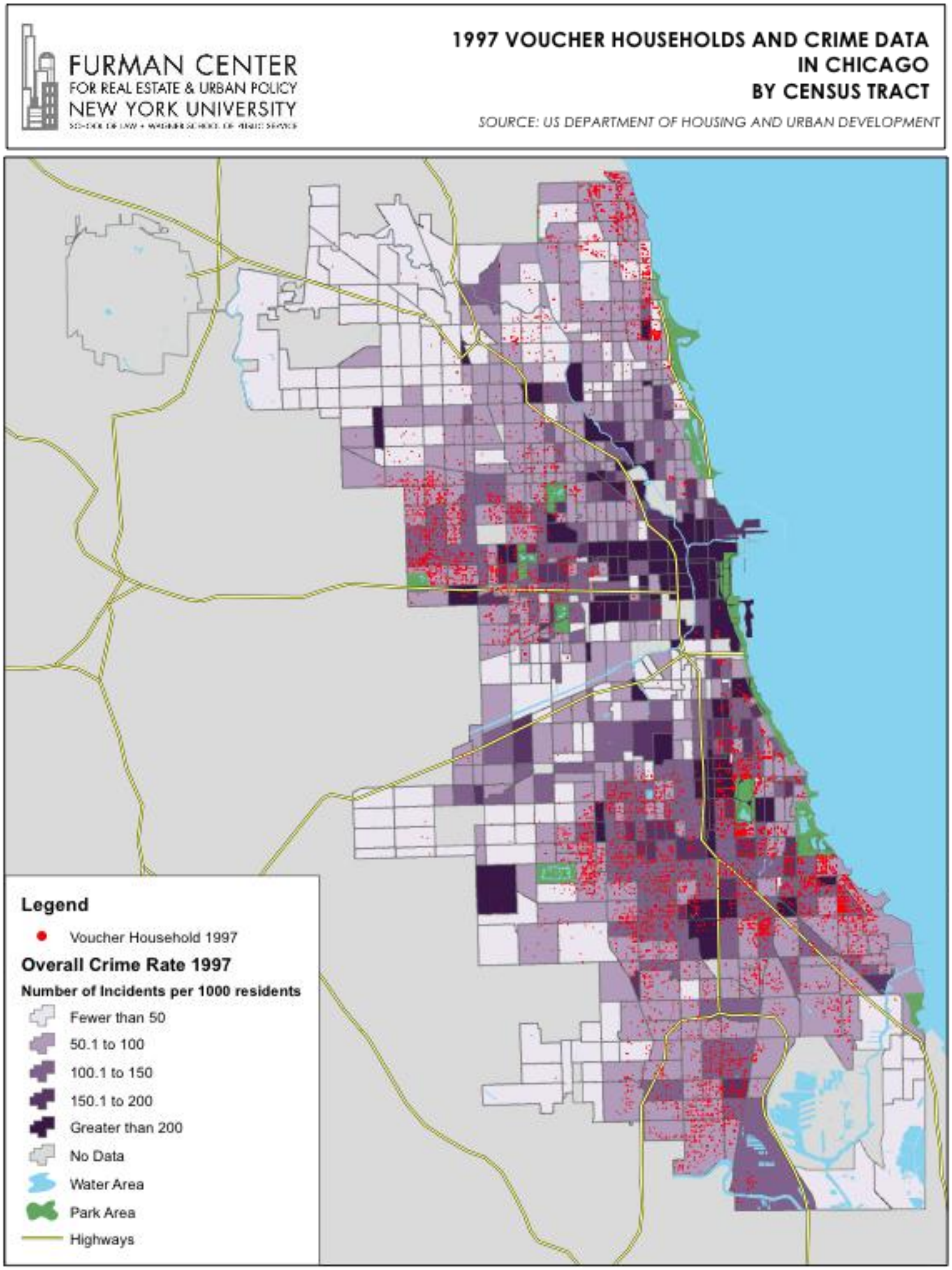
Map B—3: 2007 Voucher households and crime data in Seattle



Map B—4: 2007 Voucher households and crime data in Seattle



Map B—5: 1997 Voucher households and crime data in Chicago



Map B—5: 2007 Voucher households and crime data in Chicago