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

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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 rates of crime, controlling for neighborhood fixed effects and either time-varying neighborhood characteristics or 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 disappears after we control for existing trends. We find far more evidence for the reverse causal story; voucher use in a neighborhood increases in tracts with rising crime, suggesting that voucher holders tend to move into neighborhoods where crime rates are increasing.

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 rates of 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. In some models, we also control

[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.)

for time-varying census tract characteristics such as the extent of other subsidized housing and demographic composition. (We do not include these time-varying 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 a 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 where crime rates are increasing.

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

[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

and housing conditions. Voucher recipients receive a subsidy, the value of which depends on the 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, 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 lead to elevated rates of crime. While the rationale behind these fears 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. Both economists (Becker 1968) and sociologists (Agnew 1992; Merton 1938) have developed robust theories explaining why poor individuals are more likely to engage in criminal activity. Sociologists tend to draw on Merton's widely cited "strain theory," where poverty and inequality produce pressure or 'strain' to obtain recognized symbols of status and wealth, resulting in increased deviant behavior. Economists meanwhile frequently cite Becker (1968), who portrayed criminals as rational actors who weigh the costs and benefits of committing crimes. In this view, more impoverished individuals simply have more to gain

[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.

(and less to lose) from criminal activity. Both theories predict that poor individuals (e.g., those who expect to derive more income from crime than from legal pursuits) will be more likely to commit crimes, and crime will be higher in higher poverty neighborhoods.[7]

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.

Given these relationships between poverty and crime, we might expect crime rates to rise in a neighborhood when the number of low-income voucher holders increases. Specifically, we expect crime rates to rise (or rise more than they 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, voucher holders, at least in theory, 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 disadvantage in a neighborhood, it may not always do so in practice. Indeed, in lower

[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.

income areas, an increase in voucher holders could potentially reduce economic disadvantage, given the positive wealth effects of housing subsidies.

A second potential mechanism through which voucher holders might affect crime is that voucher holders may potentially increase the level of income diversity or income inequality in a neighborhood, particularly in 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 are broken down. Sampson, Raudenbush, and Earls (1997), in a widely-cited paper, report a strong association between residential instability and violent crime in Chicago. Although they also find that higher levels of collective efficacy in these neighborhoods appears to reduce the link between residential instability and concentrated disadvantage and crime. Researchers have also found that neighborhood attributes that are symptomatic of residential instability, such as white

[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.

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, there is virtually no empirical work that directly tests whether and how vouchers shape neighborhood crime rates. 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 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 data 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, 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). 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 do occupy neighborhoods

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

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] 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.

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 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. 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 previous and subsequent

[16] We have no data on vouchers for Philadelphia and Seattle data from 2002 through 2006 and incomplete data in 1996 for Chicago, Cleveland and Indianapolis, in 2000 for Chicago, and in 2006 and 2007 for Washington, DC..

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 10 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 seven years.

In addition to crime and voucher data, we have access to a limited number of control variables that are available annually 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, in some models we control for additional tract characteristics that may vary over time to capture changes in both the housing stock and population. Specifically, we include the number of public housing and Low Income Housing Tax Credit (LIHTC) units in a tract in a given year,

[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. We identified this data issue in 771 cases (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.

using the data provided by HUD and the estimated annual poverty rate, homeownership 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 50,000. 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 1000 persons for our sample and for the 202 U.S. cities with population greater than 50,000 for which crime data were available from the FBI Uniform Crime Report system in 2000.The average crime rate in our sample in 2000 (for nine cities) was about 68.8 crimes per 1,000 persons, with substantial variation across cities. As a comparison, the 2000 crime rate for the sample of U.S. cities with populations greater than 50,000 with crime data available was 60.9 per 1000 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,

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

rape, 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 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 lower rents and greater presence of poor households) may also directly shape crime rates. Similarly, reverse causality is a threat too. Crime itself may actually lead to lower rents and thereby make 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 experimenting with the timing of voucher presence and changes in crime. 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) and city-specific year dummies to control for crime trends in the city.

Our second model includes additional, time-varying tract controls, such as the number of public housing and LIHTC units, the poverty rate, the homeownership rate, and racial composition, to capture trends that may independently affect crime in the tract. Because several of the theoretical mechanisms through which voucher holders might affect crime occur via changes in a tract's demographics, we estimate a third model, without these time-varying characteristics but including 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. (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 the affect crime, while omitting tract-specific trends that may themselves be affected by the presence of voucher holders.

In addition to including neighborhood fixed effects in all models, we also lag voucher counts to wash out 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 the entry (or exit) 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 rates 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. 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 frequent enough occurrence that the data are better approximated by the normal distribution, and thus the corresponding models are best estimated using OLS.[21]

The baseline models are as follows:

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

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

[21] 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 results using the alternative dependent variable.

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

where $Crime_{ict}$ indicates the crime count in tract i, city c (or PUMA p), and year t, $Voucher_{it-1}$ 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_{it} include time-varying characteristics 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 these same specifications using voucher counts measured in year t, rather than year t-1. These models should not be interpreted causally, however. Rather, they are meant as a representation of 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 may lead to a contemporaneous association between vouchers and crime.

As an explicit test of such reverse causality, we estimate the same models as above, but with future voucher holders 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:

$$(2a)\ Crime_{ict} = b_0 + b_1 Voucher_{it+1} + \lambda_i + C_c * T_t + e_{ict}$$

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

$$(2c)\ Crime_{ipt} = b_0 + b_1 Voucher_{it+1} + \lambda_i + PUMA_p * T_t + e_{ipt}$$

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:

$$(3)\ Crime_{ipt} = b_0 + b_1 Voucher_{it-1} + b_2 Vouchersquared_{it-1} + \lambda_i + PUMA_p * T_t + e_{ipt}$$

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:

$$(4a)\ Crime_{ipt} = b_0 + b_1 Voucher_{it-1} + b_2 HighPoverty * Voucher_{it-1} + \lambda_i + PUMA_p * T_t + e_{ipt}$$

$$(4b)\ Crime_{ipt} = b_0 + b_1 Voucher_{it-1} + b_2 LowPoverty * Voucher_{it-1} + \lambda_i + PUMA_p * T_t + e_{ipt}$$

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 higher crime rates 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 population. The first and fourth columns show results of the basic regressions with city*year fixed effects, while the remaining columns show regressions that include additional controls for more local trends, either tract-level variables for observed, time varying factors in the neighborhood 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 number of Low-Income Housing Tax Credit (LIHTC) and public housing households 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.

In our first model (column 1), with city*year fixed effects and only population as a tract control, the coefficient on lagged voucher households is positive and statistically significant, suggesting that an additional voucher household in the tract is associated with an increase of approximately 0.12 crimes in a tract. When we add additional tract controls in the second column, the coefficient on voucher households falls and becomes insignificant. Once other changes in the neighborhood are accounted for, there is no significant relationship between the presence of voucher holders and a tract's crime rate.[22] In column three, we remove these tract variables and replace city*year fixed effects with PUMA*year fixed effects. The coefficient on voucher count remains insignificant. In other words, 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

[22] 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 results are nearly identical.

significant association between the presence of voucher holders in one year, and crime in the next.

The last three columns of the table show the results of the alternative specification with contemporaneous voucher counts, a model we don't interpret as causal for reasons discussed earlier. The coefficient on voucher households is positive and statistically significant in column four, showing a similar association between contemporaneous counts of crime and vouchers as we saw in the first column of the table. Again, after inserting controls for differences in demographic changes across tracts or for trends in the broader sub-city area, there is no relationship between vouchers and crime, even contemporaneously. This lack of relationship is not a by-product of our lagging crime.

These results suggest that the simple bivariate correlation between crime and vouchers may primarily be driven by other correlated factors. The only evidence of a relationship between crime and voucher counts comes from the models that do not control for other changes occurring in a tract or its surrounding area. The fact that the association is eliminated once such controls are included suggests reverse causation and omitted variables are its source.

To test for such reverse causality directly, Table 5 presents results from an intuitive causality test, in which we regress crime in year t on voucher counts in year t+1. Clearly, voucher holders who have not yet entered the tract cannot be causing crime in time t. In columns 4-6, we estimate regressions with both lagged and future voucher counts on the right hand side, to see if any association between lagged voucher counts and crime persists when future voucher counts are also included on the right-hand side of the equation.

We see in Table 5 (columns 1-3) that the future voucher counts are much more strongly associated with crime in time t than are lagged voucher counts (columns 1-3, Table 4). When future voucher counts are included in the model (columns 4-6), there is only a weak association between lagged voucher counts and crime in our most basic model, column 4, and none in the more fully controlled models.[23] Yet the coefficient on counts of future voucher holders is consistently positive and significant. This suggests that it is not the presence of voucher holders *per se* in a tract that leads to an increase in crime rates, but rather that voucher holders tend to enter neighborhoods whose crime rates are high or increasing.[24]

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. Column 1 of Table 6 presents results of such a quadratic model with lagged voucher counts, for our third model, with PUMA*year controls, but no tract demographic variables (other than population) to avoid over-controlling. Neither the coefficient on the linear nor that on the quadratic term is significant. We find the same results for our model with time-varying tract controls.

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. To do so, we estimate models that include voucher counts, and an interaction between voucher counts and

[23] We get similar results for models using contemporaneous voucher holders; indeed, the coefficient on contemporaneous counts of voucher holders becomes insignificant in models that also include future voucher holder counts on the right-hand side.
[24] In models (not shown) where we included future and lagged voucher counts on the right-hand side, the coefficients on lagged voucher counts are no different than in the base models.

whether the tract is ranked in the top (or bottom) quartile of poverty rates in 1990. Results for our models with PUMA*year fixed effects (again, no controls other than population) are shown in the last two columns of Table 6. The coefficient on lagged voucher counts is also insignificant in models with limited tract controls and PUMA*year effects, as are all interactions.[25]

For all of the models reported in Tables 4 through 6, we specified 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). The goal here was to determine whether we would be able to detect differential effects on property and violent crimes. In those models (not displayed), we found no relationship at all between voucher counts and violent crime. For property crime, the results did not differ appreciably from those with total crimes, not surprising given the fact that the vast majority of total crimes are property crimes.

IV. Conclusion

Through our many models, we find little evidence to support the hypothesis offered by Hanna Rosin and James Bovard that voucher holders invite or create crime. There is no evidence that more voucher holders in a tract predict more crime one year later. Our findings suggest a positive relationship only when other trends in the area 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

[25] 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.

evidence of a causal relationship running from vouchers to crime. However, when we examine the relationship between current crime and voucher counts in the future, we find much stronger evidence of a reverse causal relationship. More crime predicts more voucher holders in the future.

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 like 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 rates are rising 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 > 50,000

	All Tracts in Sample Cities (N=5,063)		All Tracts in U.S. Cities > 50,000 (N=25,893)	
Variable	**Weighted Mean**	**Totals**	**Weighted Mean**	**Totals**
Voucher Units	25.2	91,226	36.6	787,368
Public Housing units	35.8	130,967	27.1	717,548
LIHTC Units	26.7	105,292	23.0	508,319
Population	5088.6	17,872,468	5220.7	106,466,565
Poverty Rate	19.2%	N/A	15.7%	N/A
Percent Non-Hispanic White	43.5%	N/A	53.2%	N/A
Percent Non-Hispanic Black	27.9%	N/A	18.8%	N/A
Percent Hispanic	20.5%	N/A	20.4%	N/A
Homeownership Rate	42.8%	N/A	54.4%	N/A

Table 2: Average Total, Violent, and Property Crimes per 1000 Persons, 2000 and 2007

Sample: 10 Sample Cities, and 202 U.S. Cities with Population > 50,000 (where crime data available)

	Our 10 Cities			**202 U.S. Cities with Population > 50,000**		
	Crimes per 1000	Violent Crimes per 1000	Property Crimes per 1000	Crimes per 1000	Violent Crimes per 1000	Property Crimes per 1000
2000	68.8	13.5	55.3	60.9	9.3	51.6

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	0.120**	0.0871	0.0784			
	(0.0606)	(0.0769)	(0.0630)			
Voucher Counts t				0.135**	0.0854	0.0955
				(0.0680)	(0.0880)	(0.0699)
Log population	45.50***	21.04	51.16***	44.54***	18.74	48.69***
	(12.55)	(14.73)	(11.45)	(14.02)	(17.43)	(12.61)
Public Housing Counts		0.0540***			0.0469***	
		(0.0146)			(0.0139)	
LIHTC Counts		-0.0515			-0.0682	
		(0.0382)			(0.0450)	
Percent Poverty		29.78			38.76*	
		(19.82)			(21.15)	
Percent Black		72.06***			75.15***	
		(23.50)			(22.85)	
Percent Hispanic		58.54*			48.63	
		(30.41)			(30.29)	
Percent Owner-Occ		-92.18***			-114.9***	
		(22.59)			(23.74)	
Percent Vacant Units		25.39			20.26	
		(30.38)			(32.93)	
Median HH Income		2.89e-05			0.000143	
		(0.000185)			(0.000199)	
Constant	-141.9	27.50	-202.8**	-159.8	50.61	-179.6*
	(100.4)	(115.1)	(91.10)	(111.4)	(136.1)	(100.4)
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	28,322	24,819	28,317	32,560	28,975	32,554
Number of Tracts	4,235	4,149	4,234	4,237	4,155	4,236
Adjusted R-squared	0.174	0.172	0.242	0.193	0.196	0.265

Robust standard errors in parentheses.

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

Table 5: Testing Causality

Dependent variable: Number of crimes in tract in year

Sample: All cities

Variable	**1**	**2**	**3**	**4**	**5**	**6**
Voucher Counts t+1	0.275**	0.186	0.252**	0.284***	0.208*	0.273***
	(0.114)	(0.131)	(0.118)	(0.0972)	(0.125)	(0.105)
Voucher Counts t-1				0.106*	0.0755	0.0800
				(0.0634)	(0.0786)	(0.0702)
Log Population	42.95**	15.16	47.06***	42.44***	16.99	48.39***
	(16.84)	(20.06)	(15.22)	(15.17)	(17.32)	(14.02)
Public Housing Counts		0.0471***			0.0535***	
		(0.0149)			(0.0159)	
LIHTC Counts		-0.0825*			-0.0744	
		(0.0500)			(0.0504)	
Percent Poverty		43.70*			32.83	
		(23.30)			(22.17)	
Percent Black		84.42***			81.81***	
		(23.91)			(25.49)	
Percent Hispanic		65.60**			79.63**	
		(32.03)			(32.86)	
Percent Owner-Occ		-121.9***			-90.93***	
		(26.05)			(25.37)	
Percent Vacant Units		17.48			21.54	
		(36.20)			(34.06)	
Median HH Income		0.000198			7.54e-05	
		(0.000225)			(0.000221)	
Constant	-146.2	98.67	-163.5	-116.8	43.48	-181.9*
	(133.4)	(156.8)	(120.6)	(120.3)	(133.7)	(110.3)
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	28,397	27,356	28,393	24161	23202	24158
Number of Tracts	4,235	4,153	4,234	4233	4147	4232
Adjusted R-squared	0.195	0.194	0.265	0.180	0.172	0.246

Robust standard errors in parentheses.

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

Table 6: Testing for Non-Linearities

Dependent variable: Number of crimes in tract in year

Sample: All cities

Variable	**1**	**2**	**3**
Voucher Counts t-1	0.147	0.00471	0.0529
	(0.119)	(0.0401)	(0.0637)
Voucher Counts t-1, Squared	-0.000103		
	(9.32e-05)		
Vouchers t-1*Highest Poverty Quartile		0.0982	
		(0.138)	
Vouchers t-1*Lowest Poverty Quartile			-0.2810
			(0.213)
Log Population	50.54***	42.42***	43.01***
	(11.53)	(10.87)	(10.91)
Constant	-195.4**	-130.7	-135.9
	(91.60)	(86.48)	(86.77)
Tract FEs	YES	YES	YES
City*Year FEs	NO	NO	NO
Puma*Year FEs	YES	YES	YES
Observations	28,317	27,481	27,481
Number of Tracts	4,234	4,158	4,158
Adjusted R-squared	0.242	0.229	0.229

Robust standard errors in parentheses.

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

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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	1,469
Chicago	0	0	821	821	821	821	0	820	819	819	817	814	811	0	8,184
Cleveland	0	0	210	210	210	210	210	210	210	209	208	205	204	204	2,500
Denver	0	76	76	76	76	76	76	76	76	76	76	76	76	0	912
Indianapolis	0	0	0	98	98	98	98	98	98	98	98	98	98	98	1,078
New York	0	0	0	0	0	0	0	0	0	2,120	2,118	2,117	2,117	2,117	10,589
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	0	0	1,080
Total	261	451	1,482	1,790	1,935	2,116	1,295	1,497	1,496	3,615	3,610	3,423	3,535	2,532	29,038