Crime and Housing Prices

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Abstract

One of the most studied effects of crime is the impact that neighborhood crime has on housing values. By our count 18 studies have estimated this impact. A major drawback of these studies is that, although crime is undoubtedly endogenous in property value models, either because of simultaneity, omitted variables or measurement error, the majority of studies (12 studies) treat crime measures as exogenous independent variables. Of the limited number of studies (six studies) that do address the endogeneity of crime, the effect of crime is generally identified using questionable instruments.

The purpose of our chapter is two-fold: (1) we provide the first critical review of the extensive literature that has dealt with the impact of crime on property values, and (2) we exploit a unique nine-year crime panel at the neighborhood level to estimate models that properly address the endogeneity of crime and allow us to overcome other specification errors that have plagued previous studies.

Our results suggest that of the two major categories of crime (property and violent), only violent crimes exert a meaningful influence upon neighborhood housing values. A 10 per cent increase in violent crimes within a neighborhood is found to reduce housing values by as much as 6 per cent.

Introduction

Maintaining public safety is a major responsibility of local governments that accounts for a significant percentage of their budgets.[1] To spend these dollars most effectively, reliable estimates of the costs of crime are needed to guide policy decisions. Unfortunately, this has proven to be a difficult task, principally because public safety (i.e., the absence of crime) is a non-market good, whose price can only be estimated implicitly. Typically, the market chosen to implicitly estimate the value of crime prevention is the housing market, where a hedonic price model is estimated that includes a measure of neighborhood crime among the regressors.

The hedonic model approach to estimating the costs of crime is attractive theoretically. The seriousness that people attach to crime should be reflected in what they are willing to pay to live in a low crime neighborhood. Moreover, by including crimes of different types in the hedonic model, their relative importance can be determined. However, a major difficulty arises in estimating the crime-housing price relationship; namely, the endogeneity of crime must be dealt with in order to obtain consistent estimates. While the endogeneity of crime is widely recognized, it is remarkable that of the 18 hedonic studies we were able to find that included crime as an explanatory variable, 12 treated crime as an *exogenous* variable. Of the six studies that do instrument crime, only two tested the validity of the instruments, and in one case an overidentification test *rejected* the exogeneity of the instruments. Undoubtedly, the endogeneity of crime has been skirted in the literature for but one reason – it is extremely difficult to identify variables that satisfy the conditions required of a valid instrument: 1) that the variables be highly correlated with the endogenous crime measure, and 2) that the variables be uncorrelated with the error term and correctly excluded from the estimated housing price equation.

The purpose of this chapter is two-fold: 1) we provide the first critical review of the extensive literature that has dealt with the impact of crime on property values, and 2) we exploit a unique nine-year crime panel at the neighborhood level to estimate models that properly address the endogeneity of crime and allow us to overcome other specification errors that have plagued previous studies. Our crime data are for census tracts in Miami-Dade County, Florida, and they span the years 1999-2007. We combine these data with repeat sales price indexes for single-family homes that we estimate for each tract, using thousands of repeat sales prices obtained from the county's property tax rolls. The endogeneity of crime is dealt with in two ways – we first difference the housing price index and the crime measures which controls for various endogeneities in crime levels, and we instrument changes in crime to allow for correlation between the crime measures and current and past values of the idiosyncratic error. This first-differences instrumental variables estimator allows us to consistently estimate the implicit cost of crime. We separately estimate the effects of violent and property crime on housing values.[2] Our results indicate that only violent crime lowers neighborhood housing values. A one per cent increase in our preferred measure of neighborhood crime (number of violent crimes per acre) is found to reduce housing price by between .2 and .6 per cent.[3]

Literature review

Table 1 summarizes the 18 hedonic price studies that have included a measure of neighborhood crime among the explanatory variables. Fourteen of the studies find a negative, statistically significant relationship between one or more measures of crime and house value. Among the four studies that do not find a negative effect, one (Case and Mayer 1996) finds a positive, statistically significant effect.[4] Overall, the fact that almost 80 per cent of extant studies find that crime has a negative effect on property value makes it safe to conclude that crime matters to people and that they are willing to pay a higher housing price in order to avoid it. Unfortunately, however, little can be said regarding which crimes matter most based upon the studies that have been done. Seven of the 14 studies that find a negative crime effect measure crime with a single variable (four use total crime, two use property crime, and one uses homicides). The seven studies that have more than one crime variable yield highly mixed results. In particular, there appears to be little consensus on whether violent crime is more or less important to people than property crime.

Both studies employing a single crime variable and those using multiple crime variables have their limitations (apart from the endogeneity of crime problem discussed below). The single crime variable studies can be divided into two types – those that use a total crimes measure and those that measure the incidence of only a single type of crime (e.g., property crime, but not violent crime, or just homicides).[5] A simple count measure of the total number of crimes implicitly gives each type of crime the same weight. Generally, total crimes are limited to those tracked by the U.S. Federal Bureau of Investigation and labeled by the FBI as ‘Part I Crimes’ or ‘indexed crimes.’ These crimes include homicide, aggravated assault, rape, robbery, burglary, larceny, and auto theft. Clearly these crimes differ greatly in their ~~severity,~~severity and a rape or murder should not be equated with a larceny or auto theft; or, more generally, violent crimes should not be lumped together with property crimes.[6] Hence using a single total crimes measure is subject to considerable measurement error. Moreover, measuring just a single type of crime is subject to omitted variable bias, because, as we document below, violent and property crimes are highly collinear. Neighborhoods with a violent crime problem are also those with a property crime problem.

In light of these criticisms, to avoid obtaining biased estimates, the preferred approach is to include measures of the incidence of each type of crime in the hedonic model. At a minimum, separate violent and property crime variables should be included. The problem, of course, as alluded to above, is that violent and property crimes are highly collinear; hence, multicollinearity makes it difficult to separate out their separate influences. One solution to this problem is to have panel data and first difference the data. As we document below, crimes by type are far less collinear in changes than in levels.

Turning now to the endogeneity of crime problem, there are at least five mechanisms whereby crime may be endogenous in a housing price model. First, neighborhoods with cheaper housing attract lower income individuals, and it is well known that income and the propensity to commit crime are inversely related.[7] Evidence also exists which shows that criminals commit the vast majority of their crimes within their home neighborhood

(Reppetto 1974; Pope 1980). The implication is that criminals self-select neighborhoods to reside in with lower property values and commit many of their crimes within these neighborhoods. Second, neighborhoods with more expensive homes attract criminals by offering higher expected payoffs in terms of the market value of stolen goods. Third, crime statistics are limited to only those crimes that are reported to police. Reporting rates are known to be higher in more affluent neighborhoods (Skogan 1999). Fourth, some unobservables that increase the attractiveness of a property (e.g., large windows or a secluded back yard) also make the property an easier target for crime (Gibbons 2004).

The above mechanisms all suggest that higher housing prices may raise neighborhood crime levels. A final mechanism, that suggests that crime will be *less prevalent* in more affluent neighborhoods, is the deterrence provided by self-protection measures. Self-protection is expected to be greater in wealthier neighborhoods because property owners are more able to afford it and they have more at risk.

As noted above, while most studies have treated crime as exogenous to housing price, six studies have treated crime as an endogenous variable. However, only one of these studies (Gibbons 2004) fully validates the choice of instrumental variables (both first-stage regression results and, where equations are overidentified, tests of overidentifying restrictions were reported); and the instruments used by *all* studies are open to question. We review each of the studies that has treated crime as an endogenous variable below.

Rizzo (1979) is the first study to instrument crime in a hedonic price model. He regresses the 1970 median contract rent or owner-estimated house value for 71 neighborhood communities (which are collections of census tracts) within the city of Chicago on the total crime rate reported for 21 police districts, with each neighborhood community assigned to a police district. To instrument the crime rate the following variables are used: the proportion of the population between ages 15 and 24, median years of schooling, the unemployment rate, population density, the proportion of the population receiving welfare, ratio of males to females, and the labor force participation rate. In both the rent and house value regressions, the coefficient on the crime variable using the instrumental variables estimator is substantially larger in absolute magnitude than the coefficient in the OLS model. Rizzo interprets his results as lending support to the position that simultaneity between crime and house value (rent) is a significant problem. Rizzo reports neither the first-stage regression results nor overidentification test results. Moreover, a number of his instruments proxy neighborhood quality and therefore probably should not have been excluded from his hedonic model.[8] His evidence and conclusions are therefore open to question.

Chronologically, the next study to instrument crime is by Naroff et al. (1980). Using Boston census tracts as the units of observation, the authors jointly estimate two equations – one with the dependent variable equal to the median owner-estimated house value of the tract and the other with the dependent variable equal to the total crime rate. The two variables that enter the crime rate equation that do not appear in the value equation (and thereby serve as instruments for the crime rate) are the population density of the tract and a housing quality variable which is a combination of the percentage of the units which have more than one person per room and the percentage of the units that do not have complete plumbing facilities. The estimated elasticity of house value with respect to the crime rate is -1.67. However, little confidence can be placed in this result given the authors' choice of instrumental variables. In particular, it is baffling why the housing quality variable would enter the crime but not the house value equation.

The next study to instrument crime is by Burnell (1988). Like Rizzo, Burnell's data are for Chicago, but while Rizzo's communities lie within the city of Chicago, Burnell's are in the suburbs. Burnell uses 1980 data from 71 communities to estimate a system of four equations. His dependent variables are: median owner-estimated house value, the property crime rate, the number of full time police officers per thousand population, and per capita community revenue. The exogenous variables that enter the crime equation which are excluded from the house value equation are: the per capita community sales tax revenue, the average number of full time police officers per thousand population for contiguous communities, the average of the median housing values in contiguous communities, the average of the per capita sales tax revenue in contiguous communities, the community poverty rate, the median family income of the community, the median age of the community's population, the community unemployment rate, and the proportion of the population in the community that completed high school. The estimated elasticity of house value with respect to the property crime rate is -.10.

It is difficult to judge Burnell's identifying restrictions. No tests of exogeneity are reported. The idea of using variables defined for contiguous communities as instruments for the property crime rate within the home community may have merit, as they should not affect house value in the home community but may affect the crime rate if criminals shift from one community to another in response to differences in expected returns and the probability of apprehension. But many of Burnell's instrumental variables simply describe the home community and therefore likely affect what people are willing to pay to live in the community, which suggests that they should not have been excluded from the house value equation.

The only study from the 1990s to instrument crime is by Buck et al. (1993). Their study is unique in that it is the first to use panel data. For the years 1972-1986 they have property value and crime data for 64 communities located in the Atlantic City region of New Jersey. Unfortunately, they choose not to exploit the panel nature of the data (by, for example, including community fixed effects) but instead they estimate pooled cross-sectional models. Their dependent variable is an estimate of the market value of a square mile of land in the i^{th} community in the t^{th} time period computed from property tax assessments. Three types of property crime are accounted for among the explanatory variables: the larceny rate, the auto theft rate and the burglary rate. These crime rates are instrumented using 'a complete set of lagged variables,' the distance from the community to the Atlantic City CBD, and the population density and unemployment rate of the home community. The authors report that their instruments *failed* an overidentification test, but they argue that 'the test for overidentifying restrictions is not constructive in that it does not recommend a subsequent cause of action' (Buck et al.1993, p. 342). This statement is not totally accurate; the results from the overidentification test are constructive in that they indicate the need to continue to search for better instruments. Buck and his colleagues estimate numerous regressions for various subsamples of their data and obtain mixed results. When they use the full sample, the results are not only mixed but are somewhat counterintuitive: the larceny rate is not statistically significant, auto theft is positive and statistically significant, and the burglary rate is negative and statistically significant.

The final two studies that treat crime as endogenous to property value are more recent. Using 1999-2001 data for London, England, Gibbons (2004) regresses individual house prices (n=8084) on burglary and vandalism crimes per square kilometer, after assigning both homes and crimes to a 100 meter grid. In the fashion of a standard fixed effects estimator, Gibbons first transforms his data by taking deviations from local district averages of the variables. In this way, unobservables that are constant within the district are eliminated as a source of endogeneity. To further address the endogeneity of crime he instruments his two crime variables (the density of burglary and vandalism crimes against residences) using as instrument variables the density of burglary and vandalism crimes against non-residential buildings. In an alternative model, distance to the nearest public house or wine bar, and its polynomials, are used to instrument the density of vandalism crimes, based on the argument that alcohol consumption is an associated factor in many types of crimes. The instruments are found to be significantly correlated with the crime densities and pass Sargan's test for the validity of the overidentification.

Gibbons' results indicate that vandalism but not burglary crimes have an impact on home prices. A one-tenth standard deviation decrease in the local density of vandalism crime adds 1 per cent to the price of an average Inner London property. He explains his somewhat counterintuitive results by arguing that acts of vandalism (but not burglaries) motivate fear of crime in the community and may be taken as signals or symptoms of community instability and neighborhood deterioration in general.

Of the six studies that have addressed the endogeneity of crime problem, Gibbons' is the only one that does so in a convincing fashion. His instrumental variables are supported theoretically (i.e., he establishes a plausible link between the variables and crime, *a priori*), and the instruments pass the standard IV validation tests. There are, however, two concerns with his study. First, vandalism crimes are likely to be underreported relative to burglaries and other types of crime. It is not clear whether his instrumentation adequately handles the measurement error that arises from underreporting. Generally, measurement error in an independent variable causes an attenuation of its estimated effect, which suggests that Gibbons may have underestimated the negative effect that vandalism has on property value. But we might also expect that the reporting rate would vary across neighborhoods and thereby the measurement error may be correlated with house price. In this case, it is difficult to predict whether the estimated effect of vandalism on property value has been under- or over-estimated. If the instrumental variables are uncorrelated with possible spatial variation in reporting, then consistent estimates can still be claimed. But this seems unlikely in Gibbons' case, since the rate of vandalism against residences (the independent variable) and the rate of vandalism against non-residential property (the instrumental variable) are both likely to be more severely underreported in deteriorating neighborhoods.

A second limitation of Gibbons' study is that violent crimes are excluded from his hedonic model. As noted above, property and violent crimes are highly collinear in levels and therefore excluding violent crimes may result in omitted variable bias. Hence, what Gibbons observes as a negative effect of vandalism on property value may actually be the result of a more plausible effect of violent crime on property value being registered through the estimated vandalism coefficient.[9]

The most recent study to treat crime as endogenous in a hedonic price model is by Tita et al. (2006). Their data link 43 000 house sales from Columbus, Ohio for the years 1995-1998 to crimes at the census tract level for 189 tracts. They use three crime measures: the total crime rate (total crimes include the seven FBI indexed crimes), the property crime rate and the violent crime rate. They regress house price on the crime rate and the change (measured over the preceding year) in the crime rate, running separate regressions for each alternative measure of crime. In each equation, two crime variables (level and change) are treated as endogenous, using as instrumental variables the murder rate and its change, so that each equation is just identified. Four models are estimated: one for all tracts and separate equations for low, medium, and high income tracts. The key results are that a positive change in the violent crime rate reduces house value in low and high income neighborhoods and the level of both the violent and property crime rate reduces property value in high income neighborhoods. No explanation is given for the particular pattern of significant versus insignificant results observed across the types of neighborhoods and between levels and changes in crime.

The authors claim that murder is an ‘ideal’ instrument because it is significantly correlated to violent crime. Given that violent crime includes murder, such an association is expected. However, they offer no evidence in support of their claim. They also use murder to instrument property crime, but make no claim regarding the association between these two variables. Because their models are exactly identified, overidentification tests could not be done.

While the murder rate may be highly correlated with the endogenous crime measure included in the hedonic price model, it is unlikely to satisfy the other conditions required of a valid instrument. First, the murder rate may have its own direct effect on house value and therefore should not be excluded from the hedonic model. Second, undesirable neighborhood characteristics (e.g., the presence of crack houses) are likely to be correlated with both housing prices and the murder rate. As is generally true with the other studies utilizing instrumental variables to estimate the cost of crime, the ad hoc nature of Tita et al.’s instrumentation makes it difficult to interpret their results.

Having provided a critical review of the extant literature relating house value to neighborhood crime, we next provide some new evidence that addresses the limitations that we have identified in previous studies.

Data

Our panel data set is constructed from two sources – the Miami-Dade property tax rolls obtained from the Florida Department of Revenue (FDOR) and a database of crime incident reports maintained by the Miami-Dade County Sheriff's Office. These data sources and how we used them to construct our panel are described below.

Florida statutes require that each county annually submits its property tax roll in a standardized format to FDOR for auditing purposes. These rolls span the years 1995 to 2007. Among the items included on the rolls is a detailed code indicating the current land use of each parcel, the interior usable space for improved properties, and the price and date of sale for the two most recent property transfers. Additionally, three fields in the FDOR rolls indicate the township, range, and section (TRS) from the Public Land Survey System (PLSS) within which the parcel is situated.[10] The TRS can be used to place each parcel within a one-mile by one-mile square.

Our goal is to investigate the impact of crime on property value at the neighborhood level. Census tracts are the most common geographical areas used to represent urban neighborhoods. The U.S. Census Bureau defines a census tract as a homogenous area in regards to the characteristics of the population, their economic status, and their living conditions. A tract generally has between 2500 and 8000 residents. Because the FDOR tax rolls report only PLSS and not Census geography, we cannot assign parcels to census tracts.[11] However, we can approximate the Census geography using the TRS squares. To generate this approximation, we first determine the census tract within which the centroid of each TRS is contained using Geographic Information Systems technology.[12] The result of this process is a TRS-tract crosswalk that can be used to assign parcels from any tax roll to tract-like areas. Our neighborhoods can be interpreted as the best approximation to census tract geography using one-mile by one-mile squares.[13]

After using the TRS-tract crosswalk to assign each parcel to a tract, we then construct a number of variables to be used in our empirical model. One set of variables captures inter-neighborhood and intertemporal differences in commercial land use composition. More specifically, for each of 25 different commercial land use classifications, we construct two variables: a count variable measuring the total number of parcels of that land use type within the tract and a continuous variable measuring the total amount of interior square footage of that land use type within the tract. It is from these commercial land use variables that we construct our instrumental variables.

Another variable we construct for each tract/year record is a constant-quality house price index value. This value is obtained by using the information on the two most recent sales transactions to estimate standard repeat sales models separately for each tract:

$$\ln\left(\frac{\mathrm{P}_{i,t}}{\mathrm{P}_{i,t-n}}\right) = \sum_{k-1}^{\mathrm{T}} \beta_k \, D_{i,k} + \varepsilon_{i,t,t-n} \qquad (1)$$

where $P_{i,t}$ is the most recent selling price of property *i* at time *t*;

$P_{i,t-n}$ is the previous selling price of property *i* at time *t-n*;

β_k is the logarithm of the cumulative price index in period *t*;

$D_{i,k}$ is a dummy variable which equals -1 at the time of the initial sale, +1 at the time of the second sale, and 0 otherwise; and

$\varepsilon_{i,t,t\text{-}n}$ is the regression error term.

If β_t denotes the estimate of the year t coefficient, the price index value for year t (I_t) equals exp (β_t)*100.

Our other source of data is the crime incidence reports provided by the Miami-Dade County Sheriff's Office. This database, which contains information on more than 2 million crimes occurring between 1999 and 2007, identifies the date, time, and type (e.g., burglary, robbery) of each crime reported in unincorporated Miami-Dade County and the municipalities of Miami Gardens, Miami Lakes, Doral, Palmetto Bay and Cutler Bay. As of 2007, this collection of six jurisdictions had a total population of 1.3 million people, which amounts to roughly 54 per cent of the total population of Miami-Dade County. The most unique aspect of the crime database is that it contains the exact location at which the crime was committed. This enabled us to assign crimes to tracts in the same way we assigned homes to tracts, as described above. Crimes are broken down into their two major categories: violent crime (homicide, aggravated assault and robbery) and property crime (burglary, motor theft and larceny).[14]

In the literature that has investigated the effect of crime on property values, crime has been measured at the neighborhood level in three different ways (see Table 1): number of crimes versus number of crimes per resident versus number of crimes per acre (or other spatial unit). The latter two measures are commonly referred to as the crime rate and the crime density, respectively. Of the three alternative measures, the crime rate has been the most popular measure, presumably because it best registers the individual resident's risk of victimization. Criminologists have argued, however, that at the neighborhood level (in comparison to the city level, which is the level at which crime rates are reported by the FBI) the crime rate is a less reliable measure of residents' probability of being victimized because crime is higher where business activity is greater and the latter activity varies greatly across census tracts.[15] Moreover, Bowes and Ihlanfeldt (2001) have argued that crime density may be more highly correlated with the average resident's knowledge of crimes committed in his neighborhood. Crime density may also be a better measure of neighborhood crime than the crime rate if people wish to avoid exposure to crime. The latter equals the probability of being either a victim of crime or a witness to a crime. This probability is better measured by the number of crimes in a unit of space than by the number of crimes per resident. All three alternative crime measures are included in our panel so that we can experiment with each one in estimating our models.

Our panel includes nine years of data for 130 tracts, yielding a total of 1170 tract/year observations. As described below, we first difference the data and allow for distributed lagged effects going back four years. This leaves us with 520 observations. For 18 of these observations a house price index value could not be computed due to an insufficient number of sales transactions, resulting in a final sample size of 502 observations.

Models used to estimate the housing price-crime relationship

To estimate the effect of crime on housing price, we first difference the data and regress the change in the housing price index on changes in crime. Focusing on housing price *changes* rather than *levels* helps mitigate possible bias resulting from each of the sources of the endogeneity of crime identified above. For example, crime may be worse in neighborhoods with lower housing prices because criminals are more likely to live in these neighborhoods and, as noted above, criminals tend to commit their crimes close to where they live. Criminals may also be more likely to live in neighborhoods experiencing less price appreciation. To the extent that this relationship between the location of criminals and neighborhood affluence is time-invariant, the standard first differences estimator provides consistent estimates of the effect of crime on housing prices, in the presence of arbitrary correlation between the regressors and that portion of the composite error term that is constant within the tract.[16] Similar arguments can be made in favor of first differencing the data for the other four sources of crime endogeneity.

Another advantage from first differencing the data is that multicollinearity among the variables measuring each type of crime is substantially reduced. Table 2 presents correlation coefficients between property and violent crime, in both levels and changes, using as the crime measure the number of crimes per acre within the tract.[17] Levels of crime density are highly collinear, with a correlation coefficient of .71. In contrast, the correlation between changes in violent and property crime density is only .22.

The basic model we estimate to investigate the effect of crime on housing price can be expressed as:

$$\Delta \mathrm{I}_{i,t} = \gamma_t + \sum_{j=1}^{J} \sum_{h=0}^{H} \beta_{j,t-h} \, \Delta C_{i,j,t-h} + \Delta \varepsilon_{i,t} \qquad (2)$$

where $\Delta \mathrm{I}_{it}$ is the change in the repeat sales price index in tract *i* between year *t* and year *t-1*; γ_t are time fixed effects (i.e., year dummy variables); and j represents the two different types of crime (property and violent).

For each type of crime, we include the current change (C_t - C_{t-1}) and four lags (C_{t-1} - C_{t-2}, C_{t-2} - C_{t-3}, C_{t-3} - C_{t-4}, C_{t-4} - C_{t-5}). The expectation is that the full impact of a spike in crime on housing price will occur with a lag. First, it takes time for new information regarding the safety of a neighborhood to be fully assimilated by market participants. Second, current changes in housing prices reflect to some extent prices agreed upon before the increase in crime occurred, given that the price is recorded at the time of closing and considerable time may expire between when an agreement is reached and the date of closing.

The two most widely-used panel data estimators, the first difference estimator and the fixed effects estimator, are consistent under the assumption of strict exogeneity, which rules out any correlation between the regressors and the error term *in any time period, current, future or past* (Wooldridge 2002, p. 254). An important implication of strict exogeneity is that current values of the dependent variable cannot affect current or future values of the explanatory variables. For example, if an increase in $\Delta \mathrm{I}_t$ causes an increase in ΔC_{t+2}, strict exogeneity is violated. Thus, although the first difference estimator is consistent in the presence of correlation between reported criminal activity and the tract-specific fixed effect, the first difference estimator will generally be inconsistent if our crime variables are correlated with that portion of the composite error term that varies over time.

While strict exogeneity may or may not hold for violent crime, it is unlikely to hold for property crime. Criminals in search of valuable goods to steal are likely attracted to neighborhoods with rising housing values. In this case, the increase in price in period t will raise property crimes in t or more likely $t+\mu$, resulting in a violation of strict exogeneity.

The solution to the strict exogeneity problem is to instrument ΔC_t and possibly ΔC_{t-1}, depending on one's assumptions. Under the assumption of sequential exogeneity, there is no contemporaneous feedback and ΔI_t is allowed to affect only future changes in crime (ΔC_{t+1}, ΔC_{t+2}, …). In this case, only ΔC_t requires instrumentation. However, if we allow for the crime levels to be correlated with contemporaneous as well as past values of the error term, then both ΔC_t and ΔC_{t-1} must be instrumented. It is reasonable to assume sequential exogeneity, because it seems unlikely that criminals would react to changing neighborhood housing values without some delay. Just like home buyers in their response to changing neighborhood crime levels, it takes time for criminals to obtain and assimilate new information regarding neighborhood crime opportunities and change their behavior accordingly. Nevertheless, we estimate (2) instrumenting just ΔC_t, but also run regressions where both ΔC_t and ΔC_{t-1} are instrumented.

In most cases, when using panel data the choice of instruments is an easy one. Lagged levels of the endogenous change variables typically make good instruments because levels and changes are correlated and the lagged values are generally assumed to be exogenous. But this strategy will not work in our case, because lagged changes in crime enter our model as explanatory variables.[18] Another challenge that instrumentation poses for our model is that there are four variables (two crime types, with ΔC_t and ΔC_{t-1} endogenous) that must be instrumented, if the assumption of sequential exogeneity is not made.

Our instrumentation strategy involves using our data on the commercial land uses that exist within each tract. Recall that there are 25 different commercial land use categories, and for each one we know the number of establishments and the total square footage within the tract. We first difference these data and alternatively use lagged changes in the number of establishments and in the total square feet of interior usable space as instruments. In each case there are 25 instrumental variables – one for each land use category.

The logic underlying our choice of instruments is that the criminal's opportunity set depends on the commercial land uses that exist within the tract. Some land uses are excellent targets for some types of crime, while other land uses favor other types of crime. For example, a new convenience store provides an opportunity to commit a robbery (a violent crime), while more offices create more locations that can be burglarized (a property crime). Hence, we expect that changes in particular types of crime are correlated with past changes in particular commercial land uses.

A concern regarding our instruments is that the commercial activity within a neighborhood may have positive or negative direct effects on nearby property values. Negative effects are possible if some types of commercial land use generate traffic, noise or other negative externalities. Positive effects are possible if commercial activity provides convenience for shopping or entertainment. However, these effects, whether positive or negative, are likely to be the result of differences in average levels of overall commercial activity rather than marginal changes within individual land use categories. For example, homes may sell for more in neighborhoods that offer more total shopping opportunities, but another shoe store is not likely to alter the attractiveness of the neighborhood.

In short, our argument is that a marginal change in a single commercial land use category will affect the opportunity landscape for criminal activity but will not affect the attractiveness of the neighborhood (apart from the effect that the change in land use has on attractiveness through its effect on crime). One way to bolster our argument is to purge from our set of instruments those land use categories that include shopping, eating and drinking establishments. If marginal changes do matter, they would seem to matter more for these particular land uses, resulting in important changes in our results. As reported below, paring down our list of instruments in this way has little effect on our results.

Ultimately, the validity of our instrumentation strategy rests upon the performance of the instruments. Specifically, are they highly correlated with the endogenous variables? Are they correctly excluded from the price equation? Are they uncorrelated with the error term of the price equation? As reported below, statistical tests directed at answering these questions support our chosen instruments.

Spatial variation in neighborhood crime

Before discussing the results from estimating the house price models, the substantial variation in crime across neighborhoods is shown with the data. First, the average number of crimes per acre and the average number of crimes per 1000 persons were computed for each tract over the nine years that make up our panel. This statistic can be interpreted as the average reported criminal activity in the tract over the duration of the panel. Next, the mean of these averages was computed for each quintile in the crime distribution, where the bottom quintile is the 20 per cent of tracts with the lowest crime densities (rates) and the top quintile is the 20 per cent of tracts with the highest crime densities (rates). These means are reported in Table 3 for total crime, violent crime and property crime.[19]

The mean number of total indexed crimes per acre (per 1000 population) is 13 (6) times greater in quintile 5 than in quintile 1. Substantial variation in crime is also found for each of the two categories of crime. This is especially true for the violent crimes. For example, the average number of violent crimes per acre in the fifth quintile is 32 times greater than the density of violent crimes in the first quintile.

We also computed the average annual change in the crime density and in the crime rate over the panel for each tract. The means of these averages, again broken down by quintiles, are also reported in Table 3. Once again, there is considerable variation across quintiles, regardless of the type of crime. In the lower quintiles, the mean changes are all negative, indicating that, within these tracts, crime tends to fall from one year to the next. In contrast, within the fifth quintile the mean change is always positive, and in most cases this is true within the fourth quintile, as well.

Besides the crime quintiles, at the bottom of Table 3 we report means by quintiles for household income and the rate of house price appreciation, the latter measured over the last four years of the panel, 2004 to 2007. The income quintiles show that the neighborhoods in our sample differ widely in income. In the 20 per cent of tracts with the highest incomes, mean income is more than twice as high as in the 20 per cent of tracts with the lowest incomes. Similarly, there is variation in house price appreciation, with prices rising more than twice as fast in the top appreciation quintile in comparison to the bottom appreciation quintile.

Finally, instead of using their own quintiles, we report mean household income and the mean appreciation percentage within the violent crime and property crime density quintiles. As expected, average household income is lowest in the top violent crime quintile and highest in the bottom violent crime quintile. In the case of property crime, while household income is again the lowest in the top crime quintile, it is highest in the second quintile. What may appear surprising is that house price appreciation is highest within the highest crime quintile, for both violent and property crime. This seeming anomaly is explained by the tendency of lower priced homes to appreciate at a more rapid rate than higher priced homes within Miami-Dade County over the specified time period (2004-2007).

Which measure of crime is best?

In this section we present the results from estimating OLS versions of (2), using the three alternative measures of crime: the number of crimes, the crime rate and the crime density. These regressions are run to determine which measure performs best in explaining neighborhood housing prices.

Of the three measures, changes in crime density explain the greatest variation in the change in the price index (see Table 4). The crime rate, which has been the most used measure of neighborhood crime, performs the worst. These results are consistent with those obtained by Bowes and Ihlanfeldt (2001), who also find that crime density outperforms the crime rate in explaining inter-neighborhood variation in housing prices. A number of arguments were made above for why density turns out to be the preferred measure of crime. In light of the findings presented in Table 4, we estimate our 2SLS models using crime density as our measure of neighborhood crime.

The OLS crime density results (Column 3 of Table 4) show that none of the changes in property crime has a statistically significant effect on the change in the house price index. In contrast, three of the violent crime changes (*t-2*, *t-3* and *t-4*) are significant with negative signs.

Strict exogeneity tests

Our strict exogeneity tests are those suggested by Wooldridge (2002). They involve adding to (2) crime levels measured for different time periods. If strict exogeneity is satisfied, crime levels for the current or any future or past period of the nine year panel should not be statistically significant. There are two restrictions on our choice of level variables to include in (2): 1) if we wish to maintain our sample size, future levels ($t + \mu$) cannot both be included and 2) because we use lagged changes in crime as explanatory variables, level variables from any two consecutive periods (e.g., *t* and *t-1*) cannot be included. In light of these restrictions we include C_t for each type of crime separately and together. We repeat these estimated regressions using C_{t-1} in place of C_t. In all cases (see Table 5), the results show that levels of both violent crime and property crime are statistically significant, indicating that neither type of crime is strictly exogenous. This implies that (2) should be estimated by 2SLS, with both changes in violent and property crime requiring instrumentation.

Results from estimating the house price index models

Estimates of (2) using 2SLS are reported in Table 6. Column 1 presents the results from instrumenting ΔC_t for violent and property crime. Recall that this estimation strategy assumes sequential exogeneity. Dropping this assumption, Column 2 presents the results from instrumenting both ΔC_t and ΔC_{t-1} for both crime categories.

Two different but related instrumental variable sets are available. One set uses lagged changes in the interior usable square footage of individual commercial land use categories as instruments. The other uses lagged changes in the number of establishments falling within these categories.

To determine which IV set to employ, as well as the lag length of the variables, we experimented with both sets and possible lags. The IV set based on changes in interior space and a two-year lag was found to yield the highest joint F-statistics in the first-stage regressions of the 2SLS models.[20] These variables also convincingly passed Hansen's J-statistic test (see bottom of Table 6). This is a test of the joint hypothesis that the instrumental variables are uncorrelated with the error term and correctly excluded from the estimated change in house price index equation.

When instrumenting just ΔC_t (Column 1) none of the property crime variables is statistically significant. Of the violent crime variables, ~~.~~ the *t* and *t-1* change variables are not significant, but the *t-2*, *t-3* and *t-4* changes are all highly significant with a negative sign. The timing pattern revealed by these results suggests, for example, that an increase in the density of violent crime in 2004 (above that reported in 2003) would first lower neighborhood housing prices in 2006 (relative to their values in 2005). Summing up the estimated coefficients on the significant violent crime variables yields an estimate of the Long Run Propensity (LRP). The LRP represents the change in the house price index that would occur within the neighborhood if there was a permanent increase in the crime measure. The LRP and its p-value are reported at the bottom of Table 6. The estimated LRP for violent crime is -1177, with a p-value equal to .000. We also use the estimated LRP to calculate an elasticity of house value with respect to crime, evaluated at the point of means. This elasticity is -.238, indicating that a 10 per cent increase in the violent crime density within a neighborhood reduces house values by 2.4 per cent.

The results presented in Column 2 of Table 6 are those obtained from instrumenting both ΔC_t and ΔC_{t-1}. One of the property crime variables, the *t-1* change, is negative and significant, but only at the 10 per cent level. Among the violent crime variables, the *t-3* and *t-4* change variables are negative and highly significant. The implied violent crime LRP is -1020 (p-value=.000) and the implied elasticity is -.206. Hence the violent crime results from instrumenting both ΔC_t and ΔC_{t-1} are quite similar to those obtained from instrumenting just ΔC_t.[21]

While the tests of strict exogeneity indicated the need to instrument both violent and property crime, the 2SLS results presented in Table 6 are quite similar to the OLS results presented in Table 4. Both sets of results yield the same conclusion; namely, that violent crime, but not property crime, affects what home buyers are willing to pay for housing in different neighborhoods.

A possible limitation of the results presented in Table 6 (and Table 4) is that they are generated from an equation that includes only year dummies as control variables. Omitted variable bias may result if there are variables, other than the crime variables, that are changing over time within tracts that are both correlated with crime and have their own direct effect on neighborhood housing price. We took two approaches to address this issue. First, we added to the model current and lagged changes in three variables: the total interior square footage of commercial space within the tract, the total interior square footage of industrial space within the tract, and the total interior space of multi-family housing within the tract. Only the LRP for multi-family housing is statistically significant, with a positive sign. Apparently, the multi-family variables capture changes in the attractiveness of the neighborhood that are not accounted for if only the crime and year variables are included in the model. However, the inclusion of the multi-family variables had little effect on the estimated crime LRPs. The results continue to show, with the same consistency across specifications, that violent crime is the only type of crime that matters to neighborhood housing price.

The other approach we took to investigate the possibility of omitted variable bias involved using our instrumental variables. If the crime variables are correlated with the error term as the result of omitted variables, unbiased estimates can be obtained by instrumenting the crime variables. As noted above, in the specification that does not assume sequential exogeneity, we are instrumenting ΔC_t and ΔC_{t-1} for both types of crime. But omitted variables may bias the estimated coefficients on the other crime variables (ΔC_{t-2}, ΔC_{t-3}, and ΔC_{t-4}).

We therefore instrumented all ten crime variables entering (2) (ΔC_t, ΔC_{t-1}, ΔC_{t-2}, ΔC_{t-3}, ΔC_{t-4} for both categories of crime). For this model the instrumental variable set based on changes in the number of establishments within each commercial land use category, lagged four years, best satisfied our IV validation tests. For all ten endogenous variables the first-stage F-statistics are significant at the 1 per cent level and the exogeneity of the instrumental variables could not be rejected based on Hansen's J-statistic. However, simultaneously treating all ten crime variables as endogenous clearly stretches what can meaningfully be done with our data. These results should therefore be interpreted with an appropriate degree of caution.

The results are reported in Column 3 of Table 6. Once again, none of the property crime variables is significant. Of the violent crime variables, the *t-3* and *t-4* changes are significant, mirroring the results presented in Column 2, where ΔC_t and ΔC_{t-1} are ~~instrumented .~~ instrumented. The results are therefore consistent with the OLS and other 2SLS results, which point to violent crime as the type of crime that matters to property values. However, the estimated LRP (-3104) and elasticity (-.627) are substantially larger than those suggested by the previous results.

Conclusion

In this chapter we first reviewed the extensive literature that has related crime to housing values. We found that the most remarkable feature of this literature is that the vast majority of studies treat crime as an exogenous variable in estimated hedonic models. Of the minority of studies that endogenize crime, each study either uses a questionable instrumentation strategy or suffers from other possible specification errors.

In an attempt to deal with the endogeneity of crime and avoid other methodological weaknesses found in previous studies, we utilize a nine-year panel of crime for Miami-Dade County at the neighborhood level. Using state-of-the-art panel data estimation techniques we find that home buyers are willing to pay nontrivial premiums for housing located in neighborhoods with less violent crime. Our most reliable estimates suggest that the elasticity of house value with respect to the neighborhood density of violent crime is roughly equal to -.25. We find no evidence that property crime has an impact on housing prices.

The unimportance of property crime may reflect the fact that such crimes cause far less psychic harm to victims in comparison to violent crime. Moreover, self-protection measures are more effective in deterring property crimes. In particular, alarm systems can effectively protect residents from both burglary and auto theft.[22] In comparison, there are a limited number of options that can offer protection against most acts of violence. The best option may be to self-select peaceful living environments, which our results suggest is an option that many households adopt.

Table 1
Crime/Property Value Studies

Author(s) and Date of Publication	City	Year(s)	Data	Crime Variables	Instruments	Validation of Instruments	Findings
Ridker and Henning 1967	St. Louis	1960	Median owner-estimated house value for 167 tracts; total indexed crimes by police district	Crime rate	None	None	Crime rate not significant.
Kain and Quigley 1970	St. Louis	1967	579 rent values, 275 owner-estimated values; crime rate of Pauley block	Crime rate of Pauley block	None	None	Crime rate not significant for renters or owners.
Gray and Joelson 1977	Minneapolis	1970	Median owner-estimated house value for census tracts; eight different types of crime by tract	Various crime rates	None	None	Out of eight types of crime only vandalism and burglary are significant: 1% increase in burglary caused $336 decline in house value,

							1% increase in vandalism caused $117 decrease in house value.
Thaler 1978	Rochester	1971	398 home values; property crimes for census tracts	Property crime rate of tract	None	None	Standard deviation increase in property crime reduces house value by 3%.
Hellman and Naroff 1979	Boston	1972-1974	Median owner-estimated house value in 147 census tracts; index crimes per tract	Crime rate of tract	None	None	Elasticity of house value with respect to crime rate = -.63.
Rizzo 1979	Chicago	1968-1972	1970 median rent for 111 blocks; 1970 median owner-estimated value for 68 blocks; 324 single-family sales; for above three data sets total	For block and individual sales regressions: total crime rate, property crime rate, violent crime rate of block; for 71 neighborhood communities,	Crime is instrumental in only the regression run for the 71 neighborhood communities, instruments = the proportion of population 15-24, median years of	None	Generally, all crime variables have negative and significant effects across the alternative data bases. For the data base where crime is instrumented, estimated crime

			crime, violent crime, property crime by block; median owner-estimated house value for 71 neighborhood communities; for the latter data, the 71 communities are assigned total crimes reported for 21 police districts	total crime rate of police district	schooling, unemployment rate, population density, proportion of population on welfare, males to females, and labor force participation rate		coefficients are much larger than those obtained with OLS.
Naroff, Hellman, Skinner 1980	Boston	1970	Median owner-estimated house value for census tracts; total crimes of tract	Crime rate of tract	Population density of tract	None	Elasticity of house value with respect to crime rate = -1.67.
Dubin and Goodman 1982	Baltimore	1978	1765 single-family sales; 12 different types of crime for crime	Principal components for non-violent property crime	None	None	Unit increase in C1, C2 and C3 reduces house value by $795, $3143

			reporting areas	(C1), violent crime (C2) and shopping center crime (C3)			and $3721, respectively.
Burnell 1988	Chicago suburban communities	1980	Median owner-estimated value for 71 communities; property crime for each community	Property crime rate	Community fiscal & demographic characteristics	None	Elasticity of house value with respect to property crime rate = -.10.
Clark and Cosgrove 1990	Multiple metropolitan areas	1979	596 owner-estimated home values from Public Use Micro-data Sample	Murder rate estimated for each house as a function of distance of house from CBD	None	None	Elasticity of house value with respect to murder rate = -.125.
Buck, Hakim, Spiegel 1993	Atlantic City region	1972-1986	Total assessed value for 64 communities; larceny, auto theft, burglary crimes for each community	Larceny, auto theft, burglary crime rate of community	Lags of explanatory variables	Failed over-ID test	Highly mixed, with no consistent finding across alternative model specifications.
Taylor 1995	Baltimore	1980	Median owner-estimated	1970 assault rate, Δ assault rate, Δ murder	None	None	Δ assault rate and Δ murder rate negative,

			house value for 223 census tracts; assault and murder rates by tract	rate			significant effect on Δ value, level of assault rate positive, significant effect.
Case and Mayer 1996	Eastern Massachusetts towns	1982-1992	Repeat sales price index for 168 towns; total crimes per town	Crime rate of town	None	None	Crime rate has positive effect on house price.
Bowes and Ihlanfeldt 2001	Atlanta	1991-1994	22 388 single-family sales; index crimes for census tracts	Density of total crimes in tract	None	None	Unit increase in crime density reduces house value by 3 to 6 %.
Lynch and Rasmussen 2001	Jacksonville	1994-1995	2800 single-family sales; index crimes for 89 police beats	Cost of violent crime, cost of property crime	None	None	Only houses in top two cost of crime deciles suffer nontrivial loss in value from crime.
Schwartz, Susin, Voicu 2003	New York	1975-1998	Repeat sales for 25 947 sales pairs; crimes for 75 police precincts	Violent crime rate, property crime rate	None	None	Only violent crime rate is significant, unit increase in violent crime rate reduces house value by

							.12%.
Gibbons 2004	London	1999-2001	10 464 individual single-family sales; burglaries, vandalism crimes for 100 meter grid	Burglaries / kilometers, vandalism / kilometers	Commercial crime densities, distance to nearest public house or wine bar	Yes	A one-tenth standard deviation decrease in the local density of vandalism crime adds 1% to house price; burglary crime rate not significant.
Tita, Petras, Greenbaum 2006	Columbus	1995-1998	43 000 single-family sales; index crimes by census tract	Total crime rate, violent crime rate, property crime rate (entered in levels and changes); separate regressions run for each type of crime	Murder rate	None	Crime is capitalized into house price at different rates for poor, middle, and wealthy neighborhoods, with violent crime most important.

Table 2
Correlation Coefficients – Levels and Changes in Neighborhood Crime Density

	Violent	Property	Δ Violent
Violent			
Property	.71		
Δ Violent	.06	-.04	
Δ Property	.01	.04	.22

Table 3
Neighborhood Characteristics (Means of Nine-Year Averages)

	Mean	Means by Quintile				
		(1)	(2)	(3)	(4)	(5)
Total crime [a]						
Density	.465	.074	.269	.411	.603	.952
Δ Density	-.006	-.049	-.019	-.007	-.000	.008
Rate	66.9	22.9	39.7	54.2	78.8	136.4
Δ Rate	-1.5	-6.6	-2.0	-.8	.0	1.9
Violent crime [b]						
Density	.063	.006	.018	.034	.065	.189
Δ Density	-.002	-.008	-.002	-.000	.000	.002
Rate	8.8	1.6	2.9	4.5	8.9	25.8
Δ Rate	-.2	-1.1	-.2	-.1	.0	.3
Property crime [c]						
Density	.406	.070	.253	.376	.533	.788
Δ Density	-.011	-.041	-.018	-.006	-.000	.008
Rate	58.6	21.4	36.5	48.4	68.7	115.6
Δ Rate	-1.3	-5.6	-2.0	-.8	.2	1.9
Household income	53 288	35 360	45 027	51 587	57 742	75 433
% Δ Index 2004-07	37.2	23.0	32.1	36.4	42.4	51.7
For violent crime [d]						
Income		64 504	60 242	54 141	49 944	40 347
% Δ Index		33.9	32.1	33.3	38.2	47.2
For property crime [e]						
Income		58 700	60 816	53 725	50 112	42 748
% Δ Index		35.1	33.4	35.9	37.3	43.2

[a] Total crime = murder + aggravated assault + robbery + burglary + motor theft + larceny.
[b] Violent crime = murder + aggravated assault + robbery.
[c] Property crime = burglary + motor theft + larceny.
[d] The quintile means reported are for the violent crime density quintiles.
[e] The quintile means reported are for the property crime density quintiles.

Table 4
OLS Estimates of Alternative Measures of Crime on House Price Index

	Crime [a]	Crime Rate [b]	Crime Density [c]
Δ Violent			
t	-.050	-.401	-19
	(.707) [d]	(.661)	(.896)
t-1	-.119	-1.200	-139
	(.350)	(.138)	(.279)
t-2	-.275	-2.004	-255
	(.007)	(.000)	(.002)
t-3	-.664	-3.797	-506
	(.000)	(.000)	(.000)
t-4	-559	-3.233	-413
	(.000)	(.000)	(.000)
Δ Property			
t	.018	.113	33
	(.515)	(.578)	(.237)
t-1	.024	.257	36
	(.353)	(.173)	(.131)
t-2	.012	.108	23
	(.563)	(.459)	(.293)
t-3	.010	.118	29
	(.629)	(.567)	(.232)
t-4	.014	.111	22
	(.501)	(.491)	(.335)
R-square	.317	.312	.324
Observations	502	502	502

[a] Number of crimes in tract
[b] Crime rate in tract = (#crimes in tract / population of tract) * 1000
[c] Crime density in tract = #crimes in tract / land area of tract in acres
[d] P-values in parentheses. These values are based on standard errors that are robust to heteroskedasticity and serial correlation.

Table 5
Strict Exogeneity Tests
(P-values) *

	(1)	(2)	(3)	(4)	(5)	(6)
Property crime						
t	.001				.027	
t-1		.001				.027
Violent crime						
t			.000		.000	
t-1				.000		.000
Joint F-statistic					.000	.000

* P-values are based on standard errors robust to heteroskedasticity and serial correlation.

Table 6
Estimates of Crime Density on House Price Index
Two-Stage Least Squares

	(1) [a]	(2) [b]	(3) [c]
Δ Violent			
t	-139	413	766
	(.780) [d]	(.354)	(.421)
t-1	-190	915	-678
	(.321)	(.059)	(.388)
t-2	-281	-17	-518
	(.004)	(.924)	(.603)
t-3	-511	-547	-1488
	(.000)	(.000)	(.060)
t-4	-385	-473	-1615
	(.001)	(.002)	(.013)
Δ Property			
t	85	121	-109
	(.288)	(.126)	(.568)
t-1	55	-106	247
	(.124)	(.084)	(.325)
t-2	26	-58	353
	(.287)	(.161)	(.328)
t-3	30	-34	258
	(.245)	(.271)	(.219)
t-4	16	-8	3
	(.516)	(.780)	(.982)
Violent, LRP [e]	-1177	-1020	-3104
	(.000)	(.000)	(.019)
Elasticity [f]	-.238	-.206	-.627
Hansen's J-statistic	20.4	12.8	20.5
	(.674)	(.937)	(.306)
Observations	502	502	502

[a] Δ Violent crime density (t) and Δ Property crime density (t) are instrumented.
[b] Δ Violent crime density (t, t-1) and Δ Property crime density (t, t-1) are instrumented.
[c] Δ Violent crime density (t, t-1, t-2, t-3, t-4) and Δ Property crime density (t, t-1, t-2, t-3, t-4) are instrumented.
[d] P-values in parentheses. These values are based on standard errors that are robust to heteroskedasticity and serial correlation.
[e] LRP = long run propensity = sum of estimated violent crime coefficients that are statistically significant.
[f] Elasticity estimated at point of means.

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[1] Expenditure data from the Florida Department of Financial Services indicates that in 2006, Miami-Dade County, the subject of our empirical investigation, spent over $540 million on law enforcement. This figure constituted over 15 per cent of all county expenditures in this fiscal year.

[2] Our definition of property crime is identical to the definition employed by the FBI and includes the crimes of burglary, motor theft and larceny. The FBI defines violent crime as murder, rape, robbery and aggravated assault. To protect the identity of victims of rape, the addresses of rape crimes are not reported and therefore could not be assigned to census tracts. Hence we define violent crime as murder, robbery and aggravated assault.

[3] Crimes per acre (crime density) is preferred over crimes per person (crime rate) because the former variable has greater explanatory power, as we will document below.

[4] Case and Mayer is not the only study to find that crime increases housing prices. Taylor (1995) includes both levels and changes in crime rates in his attempt to explain the change in house values within Baltimore's neighborhoods during the 1970s. While the change variables produce the expected negative signs, crime levels are found to be positively related to value appreciation.

[5] Some studies include more than one type of crime, but just two or three types. For example, Gibbons (2004) includes vandalism and burglary crimes and Buck et al. (1993) include larceny, auto theft, and burglary crimes. These studies are subject to the same criticism noted below of single crime variable studies; namely, omitted crime types that may affect property values are correlated with included crime types, resulting in possible omitted variable bias.

[6] A number of studies have attempted to calculate the cost incurred by victims of different types of crimes, where total cost is inclusive of both out of pocket (monetary) cost and the psychic cost associated with pain and suffering. All of these studies demonstrate that violent crime carries a higher cost than property crime. For example, Cohen et al. (1995) estimate that the total cost of a rape is $87 000, while that of a motor vehicle theft is $4000 (measured in 1993 dollars).

[7] For a review of the literature that links income to crime, see Eide (1994).

[8] For example, it is well known that homeowners prefer lower density neighborhoods, invalidating the use of population density as an instrumental variable.

[9] Gibbons is fully aware of the problem created by excluding a violent crime measure from his hedonic model. He offers some evidence using data at a higher level of spatial aggregation (equivalent to counties in the U.S.) that suggests that the omitted variable problem may not be severe. These data show that changes in crime rates of different types of crimes are not highly correlated. But this evidence has little relevance to his model, since his crime measures are not change (i.e., first differenced) variables.

[10] More information on the PLSS can be found at http://nationalatlas.gov/articles/boundaries/a_plss.html. Each township/range/section combination typically corresponds to a one-mile by one-mile square. Although quite rare, it can be the case that the TRS code identifies a geographic area that is larger or smaller than one square mile. This might be the case, for instance, in sections near jagged borders (e.g., near the coast).

[11] The FDOR rolls can be matched with digital parcel maps using a unique parcel number from the tax rolls. Because these digital maps are only available for 2007, however, a number of problems arise when attempting to use the maps to determine the location of parcels from tax rolls prior to 2007. For instance, if a parcel was undeveloped in 2000, and then developed in 2001, the parcel identifier would generally change between the two years. Given the way the digital parcel map is formatted, this change in parcel number would make it impossible to identify the correct geography for the undeveloped parcel in 2000. As these types of matching problems appeared to lead to nontrivial loss of information, we decided to use the PLSS codes as the geographic identifiers, which do not suffer from the aforementioned matching problems.

[12] We use tract boundary files from the 2000 Census to generate this crosswalk.

[13] For the sake of expository flow, we refer to our neighborhood unit as a tract, as opposed to 'tract-like' hereafter.

[14] As noted above, although the times and dates of rapes are reported in our database, the locational information of the incident is excluded so as to protect the identity of the victim. Hence we exclude rape from our definition of violent crime.

[15] See Harries (1981) for a review of the literature on this issue.

[16] After first differencing the data, the endogeneity issue is whether criminals are more likely to move into neighborhoods experiencing less price appreciation. This is not clear, but what is clear is that endogeneity arising from the movement of criminals is less likely to be a concern in comparison to that which arises from the location of criminals.

[17] As noted above, we use three alternative measures of crime – the number of crimes in the tract, the crime rate in the tract (crimes/population), and the crime density in the tract (crimes/acre). The latter variable is our preferred measure because, as we document below, it explains the most variation in our dependent variable (the change in the price index).

[18] For a discussion on instrumentation in distributed lag models, see Wooldridge (2002, p. 307).

[19] Recall that we exclude one of the seven indexed crimes (namely, rape) because the location of these crimes is not reported.

[20] P-values on the F-statistics, which are robust to both heteroskedasticity and serial correlation, are in all cases equal to .000. In the case where sequential exogeneity is assumed and just ΔC_t is instrumented, there are only two endogenous variables. With three or fewer endogenous variables, Stock and Yogo's (2005) test for weak instruments can be done. Weak instruments refer to instruments whose correlation with an endogenous variable is not strong enough to avoid a biased IV estimator. Stock and Yogo's test involves comparing the first-stage F-statistic to a critical value. In our case the critical value is 11 and the two F-statistics are 17 (violent crime) and 13 (property crime); hence we can conclude that our instruments are not 'weak.'

[21]We also estimated (2) excluding from the set of instruments those land use categories that represent shopping, eating and drinking establishments. This change had little effect on our results. When instrumenting ΔC_t, the estimated LRP on violent crime is -1236 (versus -1177 using the full set of commercial land uses) and the estimated elasticity of the house price index with respect to violent crime is -.250 (versus -.238 using all commercial land uses). When instrumenting both ΔC_t and ΔC_{t-1}, the estimated LRP is -1042 (versus -1020) and the estimated elasticity is -.210 (versus -.206).

[22] Home buyers may also feel that their homeowner's insurance protects them from the monetary losses that may result from property crime.