Has Falling Crime Driven New York City's Real Estate Boom?

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Abstract

We investigate whether falling crime has driven New York City's post-1994 real estate boom, as media reports suggest. We address this by decomposing trends in the city's property values from 1988 to 1998 into components due to crime, the city's investment in subsidized low-income housing, the quality of public schools, and other factors. We use rich data and employ both hedonic and repeat-sales house price models, which allow us to control for unobservable neighborhood and building-specific effects. We find that the popular story touting the overwhelming importance of crime rates has some truth to it. Falling crime rates are responsible for about a third of the post-1994 boom in property values. However, this story is incomplete because it ignores the revitalization of New York City's poorer communities and the large role that housing subsidies played in mitigating the earlier bust.

Keywords: Crime; Hedonic method; House prices; Repeat-sales method

Introduction

The fall in crime in New York City over the past decade has been dramatic and has attracted national attention. Over the 1988 to 1998 period examined in this study, the murder rate fell by 69 percent, the violent crime rate fell by 53 percent, and property crimes dropped by 56 percent.[1] Falling crime spurred increased tourism and hotel occupancy, record-breaking numbers of applications to local colleges, and increased property values, according to media reports. By 1997, New York was ranked by a business magazine as the best city in the country to locate a business, and in a national poll it was ranked as the most desirable city in which to live. One national magazine announced in 1997 that New York was "roaring back," labeling it, "Comeback City" (Marks 1997).[2]

In this study, we examine this popular theory for New York City's comeback and find, perhaps unsurprisingly, that press reports of a boom during the late 1990s are somewhat exaggerated.

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[1] Changes in crime rates are the authors' calculations based on data from the New York City Police Department.

[2] Karmen (2000, 8–10) lists these and a number of other media claims in the late 1990s about the positive effects of crime reductions.

Around 1988, housing prices in New York City peaked. They fell 30 percent by 1994, when the city's housing boom began. Property values then proceeded to rise again, recovering 17.5 percentage points by 1998. This increase in the latter half of the 1990s seems relatively modest compared with the boom touted in popular discussion. One explanation might be that because our analysis tracks crime only through 1998, we miss the property value increase that occurred from 1999 until the terrorist attacks of September 11, 2001. However, as noted above, notions that New York was "roaring back" were well-established by 1997. We suspect an alternative explanation—that reports in the popular press conflated New York City with its wealthiest component, core Manhattan, which experienced faster increases than most of the city. At the same time, there is no doubt that other parts of the city did quite well in the 1990s. We document that New York's boom was strongest in both its wealthiest and its poorest areas, but it was weakest in the middle-income sections of the outer boroughs (Brooklyn, Queens, Staten Island, and the Bronx).

Were rising prices driven by falling crime? We address this question by decomposing trends in New York City's property values during 1988 to 1998 into components due to crime, the city's investment in subsidized low-income housing, the quality of public schools, and other factors. To do so, we use an extraordinarily rich data set that includes detailed information on properties sold in New York City, including actual transaction prices (rather than reported or assessed values), as well as information on crime, schools, and housing investment between 1988 and 1998. As in much of the previous research into property values and public services, we use a hedonic price model to control for numerous measured characteristics of housing and neighborhoods, and we use a fixed-effects specification to capture unmeasured, time-invariant, idiosyncratic characteristics of *neighborhoods*.

Notice that the success of such analyses hinges on the quality and availability of data on housing characteristics; therefore, hedonic analyses traditionally are bedeviled by omitted variable bias. Thus, we also investigate the relationship between property values and public amenities using a repeat-sales methodology, in which we control for unmeasured characteristics of *housing*, albeit at the cost of limiting the sample to buildings that sold more than once. For the purposes of comparison with other research, we also estimate simpler cross-section models.

To preview the results, we find that falling crime rates are responsible for six percentage points of the overall 17.5 percent increase in property values that New York City experienced from 1994 to 1998. Education quality and subsidized housing investment are about equally responsible for another seven percentage points of the increase (3.8 and 3.2 percentage points, respectively). The final 4.5 percentage points of the increase are not well explained by our model. During the earlier bust, crime and education quality played a relatively small role, largely because there was no strong trend. Crime rates peaked in the middle of the housing bust and only began their rapid decline after 1993. Subsidized housing investment, however, seems to have played a large role. In the absence of public investment in housing, property values would have fallen by 17 percentage points more than their actual 30 percent drop. So, the popular story touting the overwhelming importance of crime rates has some truth to it. Falling crime rates are responsible for about a third of the post-1994 boom in property values. This story is incomplete, however. Specifically, it ignores the revitalization of New York City's poorer communities and the large role housing subsidies played in mitigating the earlier bust.

This article is organized as follows. The next section reviews the literature on property values and crime, government investment in housing, and public schools. The following section describes the data and variables used for the study and presents some summary statistics. The third section presents the methodology, followed by a discussion of the results. A final section presents conclusions.

Literature Review

Although many studies have examined the relationship between property values and local public services, and, specifically, the effect of changes in crime, public housing investment, and public education on property values, some studies are particularly relevant to our research.

Crime

A number of studies have examined the effect of crime (particularly crime rates) on property values. Unsurprisingly, this research typically concludes that crime has a negative effect on property values. As an example, Thaler (1978) found an elasticity of home values with respect to crime rates of –0.07, whereas Haurin and Brasington (1996) found an elasticity of –0.05.

Most of these studies used cross-sectional data on self-reported home values from the decennial census (e.g., Burnell 1988; Gray and Joelson 1979; Hellman and Naroff 1979; Naroff, Hellman, and Skinner 1980; Rizzo 1979). A notable exception is Buck and colleagues (Buck et al. 1991; Buck, Hakim, and Spiegel 1993), who used 15-year panel data on towns near Atlantic City, New Jersey. Unfortunately, they based their analysis on assessed values rather than transaction prices, and they measured public services at the municipality level rather than the neighborhood level. Although Thaler (1978) and Lynch and Rasmussen (2001) used actual home sales data, both studies are cross-sectional. Thaler's research is based on an extremely small sample (398 house sales), and Lynch and Rasmussen measured crime as total crimes in a police precinct rather than per capita. A common problem with this literature is that omitted variable bias is likely to be present, given the quite limited sets of control variables (especially those measuring the quality of public services and neighborhoods). It is likely that such characteristics—notably the presence of dilapidated housing and school quality—will be correlated with both home prices and crime.[3]

Government Investments in Housing

Studies of the relationship between government investments in housing and neighborhood property values have yielded mixed results. Nourse (1963) and Rabiega, Lin, and Robinson

[3] The exception is Thaler (1978), who used extensive controls for neighborhood quality. Buck and colleagues (Buck et al. 1991; Buck, Hakim, and Spiegel 1993) used distance to Atlantic City, the assessed value of hotels, unemployment, density, and tax rates. Lynch and Rasmussen used census data measured at the tract level.

(1984), for example, found that public housing investment can have modest, positive effects on neighboring property values, whereas Lyons and Loveridge (1993), Goetz, Lam, and Heitlinger (1996), and Lee, Culhane, and Wachter (1999) found small, statistically significant negative effects on property values in some cases. At the same time, Briggs, Darden, and Aidala (1999) found little effect of scattered-site housing in Yonkers, New York; and Cummings, DiPasquale, and Kahn (2001) found little evidence of spillover effects from two place-based homeownership developments in Philadelphia. Although the sample of projects and comparison areas is quite small in these two studies, the pre-post design and the use of neighborhood controls make these persuasive. In contrast, Santiago, Galster, and Tatian (2001) found that proximity to a scattered-site public housing program in Denver may have been associated with an increase in the price of single-family homes. Unlike the previous works, they investigated both changes in price levels and trends after completion, offering hope that positive effects can be disentangled from other neighborhood dynamics, given sufficient data and a good experiment.[4]

Although disparate results may derive from a variety of differences in the methodologies and the characteristics of study sites, each of these studies is hampered, to some extent, by data limitations that make it difficult to disentangle the direction of causality and to isolate the effect of housing investment on property values.

In a series of recent works, Ellen, Schill, Schwartz, and Voicu (2001); Ellen, Schill, Susin, and Schwartz (2001); and Schill et al. (2002), adapted the Santiago, Galster, and Tatian (2001) model to study the effects of various housing programs in New York City, and they made significant strides in disentangling this causality. Using the same rich data we employ in this article, they found evidence of significant positive spillover effects. The large sample of properties and housing projects and the time period of the study, as well as the extensive complement of time-varying fixed effects, make this evidence particularly persuasive.

In sum, although consensus about the effects of subsidized housing investments on nearby properties has not yet been reached, recent research seems to suggest more optimistic findings. Two of these studies found little evidence of any spillover effects, whereas four found evidence of positive spillover effects.

Schools

In his seminal study of the relationship between schools and house prices, Oates (1969) found that both school expenditures and tax rates had large effects on property values in a sample of New Jersey towns. Subsequent research has investigated a broader range of measures aimed at capturing the characteristics of schools that matter to homeowners and that might influence house prices. These include alternative measures of inputs (e.g., school expenditures and student/teacher ratios) and outcomes (e.g., test scores). In principle, expenditures may be the most comprehensive measure of inputs, if differences in input prices can be accounted for, and, to the extent that inputs determine the quality of schools, school expenditures serve as a good

[4] This method was first presented by Galster, Tatian, and Smith (1999).

proxy for school quality (for which no single comprehensive measure exists). Unfortunately, there is no consensus regarding the relationship between school expenditures (or other measures of inputs) and any of the partial quality measures such as test scores.

The literature examining school expenditures and housing prices is voluminous, but a few recent studies are particularly relevant. Hayes and Taylor (1996), using data on 288 house sales in Dallas, found that test score gains increased home prices, whereas school expenditures were not statistically significant. In contrast, Downes and Zabel (1997), using a geocoded version of the American Housing Survey for the Chicago metropolitan area, found the opposite result—school expenditures matter to house prices. Bradbury, Mayer, and Case (1997), using aggregate data on Massachusetts towns and controlling for unobserved town characteristics, also found a positive relationship between school spending and appreciation in the price of housing.

Turning to the effect of school outputs on housing prices, Haurin and Brasington (1996) examined house sales in a cross-section of 140 school districts in Ohio and found that higher test scores raised house prices. In a cleverly designed study, Black (1999) investigated the relationship between school quality and property values, limiting her sample to homes near a school attendance boundary. Her estimates were based on a comparison of neighboring homes, which, she argued, should have similar housing and neighborhood characteristics but were located in different school catchment areas, although access to other public services and amenities was similar. She found that school quality mattered, and that controlling for unobserved neighborhood characteristics was important to estimating the effect of school quality on house prices. Most recently, Weimer and Wolkoff (2001) used the imperfect overlap of the boundaries of school districts and enrollment areas with the boundaries of towns providing other public services in Monroe County (in the state of New York) to identify the effect of elementary school output on property values. After controlling for student body composition, high school characteristics, and other public services, they found a strong positive relationship between elementary school test scores and house prices.

Although the debate about the relationship between school spending and test scores is far from resolved, it seems quite likely that local schools matter to housing prices. It is, however, unclear which school characteristics are most important—spending, test scores, or other inputs.

Data and Variables

New York City is a particularly attractive place to study neighborhood dynamics because of the availability of detailed data on neighborhood conditions and public services from a number of unique city data sources. These data sources include subcity information (both cross-sectional and longitudinal) about the location and nature of (1) property sales (address-specific), (2) crime, (3) individual school performance and student characteristics, and (4) housing production, renovation, and investment.

Property Sales Price Data

Under an exclusive arrangement with the New York City Department of Finance, we obtained a database that contains sales transaction prices for all apartment buildings, condominium apartments, and single-family homes during the period 1975 to 1998.[5] For our analysis we select two samples from this database: one comprising all sales for the period 1988 to 1998 (to be used in the hedonic regressions), and one including only the repeat sales for the same period (to be used in the repeat-sales regressions). The time span was selected to coincide with the period for which both crime and education data were available. The sample, including all sales, has 246,743 observations (transactions). The repeat-sales sample comprises 50,445 repeat sales, representing 24,484 properties. Of these properties, 23,077 were sold in two different years, 1,338 were sold in three different years, 68 were sold in four different years, and one was sold in five different years. The result is a repeat-sales data set consisting of 25,947 observations (sales pairs).[6]

The properties in each of the two samples are spread across 74 police precincts, 59 community districts, and 32 community school districts (CSDs) that administer elementary and middle schools, which we use to link to data capturing the characteristics of public services. These boundaries, which we use to attach the neighborhood data to the sales price data, are artificial to some extent, but they reflect important subcity institutions. In particular, although linking properties to data on the local school using the catchment area for each public school might be ideal, the quality and characteristics of CSDs are likely to be quite important to property values. Interestingly, in New York City, although many students reportedly attend their zoned schools, many also attend schools elsewhere in their CSDs because of the availability of specialized programs, the characteristics of the schools, or just convenience. Also, it is reportedly much more common to attend a school elsewhere in the CSD than to cross boundaries to attend a school in another CSD. Further, CSDs during this period were important administrative units, setting policies over a wide range of educational issues, including curriculum, enrichment programs, and criteria for gifted programs.

We measure crime at the police precinct level, but if data at a finer level such as the census tract were available, such data would have some advantages because the crime rate is not uniform within a precinct. However, the police report precinct-level crime data not only to researchers, but also to consumers. Hence, precinct-level crime measures may be the most salient to households choosing a neighborhood. Community districts, which are the level at which we measure housing investment, are the most artificial boundaries we use. They have the advantage of being quite similar to police precincts (the boundary lines are usually identical), and are intended by the city to follow (large) neighborhoods.

Tables 1 and 2 contain some descriptive statistics for the two sales data sets. The mean real house price per unit for the sample of repeat sales is $229,472 (in 1999 dollars), 13 percent higher than the mean value of all house sales. On average, a house's price decreases by

[5] Because sales of cooperative apartments are not considered sales of real property, they are not recorded and are not included in this analysis.

[6] Pairs of repeat sales that occurred within one year of each other are excluded from the sample.

Table 1. **Descriptive Statistics for the Sample of Repeat-Sale Houses**

	Pairs of Repeat Sales	
	Number	Percent of New York City Total
Borough		
Manhattan	4,992	19.2
Bronx	2,311	8.9
Brooklyn	6,163	23.8
Queens	9,113	35.1
Staten Island	3,368	13.0
New York City total	25,947	100.0
House prices		
Real house price per unit (in 1999 dollars)		
Mean		229,472
Standard deviation		317,713
N		50,445
Percent change in real house price per unit between sales (in 1999 dollars)		
Mean		–5.5
Standard deviation		44
N		25,947

Source: Authors' calculations based on data from New York City Department of Finance.
Note: Only repeat arms-length sales of residential buildings with a sales price per unit of at least $5,000 are included. Buildings are excluded if the number of residential units or one of the neighborhood variables is missing. Pairs of repeat sales that occur within one year of each other also are excluded.

Table 2. **Descriptive Statistics for the Sample of All Houses**

	Sales	
	Number	Percent of New York City Total
Borough		
Manhattan	31,103	12.6
Bronx	23,000	9.3
Brooklyn	69,468	28.2
Queens	91,569	37.1
Staten Island	31,603	12.8
New York City total	246,743	100.0
House prices		
Real house price per unit (in 1999 dollars)		
Mean		203,307
Standard deviation		469,351
N		246,743

Source: Authors' calculations based on data from New York City Department of Finance.
Note: Only arms-length sales of residential buildings with a sales price per unit of at least $5,000 are included. Buildings are excluded if the number of residential units or one of the neighborhood variables is missing.

5.5 percent between sales. The sales distribution across the city is similar in the two samples. Most sales are located in Brooklyn and Queens, reflecting their large share of smaller properties. The smaller properties sell more frequently than apartment buildings, which are more common in Manhattan and the Bronx.

Property Characteristics

We supplement the transactions data with building characteristics from an administrative data set gathered for the purpose of assessing property taxes (the Real Property Assessment Data [RPAD] file). These are used as explanatory variables in the hedonic regressions. RPAD contain information about buildings rather than individual units (except in the case of condominiums). Nonetheless, these building characteristics explain variations in prices surprisingly well, suggesting the data are rich enough for estimating hedonic price equations.[7] In a cross-section regression including only RPAD variables, the R^2 is 0.68.

Crime

The New York City Police Department (NYPD) provided us with data on the number and nature of crimes going back to 1988 for each of the city's 75 police precincts. Unlike many earlier analyses, these data permit us to examine the effects of different types of crime. There are 14 types of crimes recorded in the raw data.[8] It is unlikely, however, that households evaluate the crime situation using many different measures. Rather, it is more likely that fewer dimensions, summarizing the many, enter the household's decision making. Therefore, in this article, we focus on two composite crime categories: violent crime and property crime.[9] Violent crime includes murder, rape, robbery, and assault. Property crime includes burglary, larceny, automobile theft, and other felonies. Following the recent criminology literature, we include misdemeanor arrests as a proxy for the "zero tolerance" or "broken windows" policing introduced around 1994 by Mayor Giuliani and Police Chief Bratton (Eck and Maguire 2000; Kelling and Sousa 2001).[10] Misdemeanor arrests are fairly constant from 1989 to 1993, but then they soar, rising from 143,000 in 1993 to 241,000 by 1998, an increase of 68 percent. Note that between 1994 and 1998, reported misdemeanors actually fell by 13 percent, so the increase in arrests appears to reflect the change in policy rather than a change in criminal activity.

[7] See the article by Ellen, Schill, Susin, and Schwartz (2001) for more detail on the data and parameter estimates on the building characteristics in a similar model.

[8] The complete list of crime types is available from the authors on request.

[9] We use complaints to quantify crime because they likely provide a more accurate measure of criminal activity than arrests or convictions.

[10] The police report three categories of misdemeanors: misdemeanors, misdemeanors—youth, and misdemeanor summons. We include data from the first two categories in our measure of misdemeanors because they are highly correlated across precincts, and both categories increased sharply in 1994. Misdemeanor summons are not highly correlated with the other two variables and do not show the same increase in 1994. In addition, unlike the variables we use, the misdemeanor summons variable has no distinction between reports/complaints and arrests (the two are identical in the police figures).

NYPD also provided us with precinct population counts for three census years: 1980, 1990, and 2000. We imputed the precinct population for each year in the study period based on the population in the census years and the average annual growth rate in the intercensus period. Then we constructed the logarithm of crime rate variables to be used in regressions.[11]

As shown in table 3, both violent and property crime drop significantly between 1988 and 1998. The mean violent crime rate decreases from 2,578 to 1,108, with an average decline of 48.6 percent. Similarly, the mean property crime rate drops from 7,412 to 3,256, with an average decline of 54.6 percent. Meanwhile, the misdemeanor arrest rate increases on average by almost 60 percent. Note that the relatively large standard deviations for 1988 and 1998 suggest significant cross-sectional variation in crime rates, whereas the relatively small standard deviations for the 10-year changes suggest less variation across precincts in temporal patterns of violent and property crimes.

Table 3. **New York City Crime Statistics Based on Precinct-Level Data, 1988 to 1998**

Variable	Year 1988	Year 1998	10-Year Percent Change, 1988 to 1998
Violent crime rate			
Mean	2,578	1,108	−48.6
Minimum	213	109	−73.9
Maximum	30,195	7,868	−5.8
Standard deviation	3,584	970	13.6
Property crime rate			
Mean	7,412	3,256	−54.6
Minimum	2,521	1,039	−67.9
Maximum	114,759	46,735	−27.4
Standard deviation	13,284	5,391	7.2
Misdemeanor arrest rate			
Mean	2,149	3,241	59.1
Minimum	243	655	−58.0
Maximum	16,976	12,343	240.1
Standard deviation	2,113	2,056	53.4

Source: Authors' calculations based on data from New York City Police Department.
Note: The crime rate represents the number of crimes per 100,000 people. Statistics are based on 74 precincts (all the New York City precincts except precinct 22—Central Park). The last column shows statistics of the percent change in crime rate (*not* the percent change in the statistics over the 10-year period). The violent crime rate is an aggregate of the murder, rape, robbery, and assault rates. The property crime rate is an aggregate of the burglary, larceny, automobile theft, and other felonies rates.

School Performance and Student Characteristics

With the cooperation of the New York City Board of Education, we compiled a data set that includes information for the period 1988 to 1998 about the full set of elementary and middle

[11] Crime rate is defined as the number of crimes per 100,000 people.

schools in New York City's 32 CSDs, which oversee and administer the elementary and middle schools.[12] We focus on elementary schools and middle schools because of the strong tie between residential location and the choice of elementary and middle schools. Most elementary and middle school students in public schools choose either their local zoned school or another school in their CSD. Although New York City also offers zoned high schools, there is considerably more choice in the high school grades, and more students commute to attend high schools at some distance from their homes; therefore, the tie between housing and school quality is weaker. The sample size varies between 803 schools in 1988 and 930 schools in 1998.[13] For each school, we have data on enrollment; educational outcome variables such as performance on standardized reading and math tests (i.e., the percentage of students scoring at or above the national median); inputs (i.e., the percentage of teachers with more than five years of experience, the percentage of teachers with a master's degree, the teacher-pupil ratio, and attendance rates);[14] and the socioeconomic characteristics of the students (i.e., the percentage of students eligible for free or reduced-price lunch and the percentages of limited English proficient [LEP], black, Hispanic, and Asian students).

The school-level data are used to compute CSD weighted means of the available variables, with weights given by school enrollment. The newly created CSD-level variables are used in the regression analysis. We choose to keep only the education inputs and the socioeconomic characteristics of the students in the regressions. Education outcomes (i.e., test scores) are excluded because regression analyses indicate that variations in inputs and student body characteristics explain most of the variation in outcomes.[15] Thus, including measures of outcomes along with inputs and student characteristics adds little to the quality of controls, while introducing near multicollinearity and complicating the interpretation of results.

Measures of resources directly available to students, including the percentage of teachers with a master's degree, the percentage of teachers with more than five years of experience, and the teacher-pupil ratio, are expected to exhibit a positive effect on house values. Regular attendance can be viewed as a prerequisite for good performance or may reflect a stricter discipline code, which may be appreciated by parents. Thus, attendance rates likely are positively correlated with house values. Larger schools might be expected to be associated with lower house prices, following the notion that larger schools provide less personalized instruction, fewer resources, and poorer quality education. At the same time, if larger schools offer a broader range of services, larger enrollment may imply higher house prices. The percentage of LEP students and the percentage of students eligible for free or reduced-price lunch are

[12] The selection of elementary and middle schools is based on information on the highest grade in school. Schools with the highest grade less than or equal to grade 9 are included in the elementary/middle category. The year 1988 denotes the 1987 to 1988 academic year, 1989 denotes the 1988 to 1989 academic year, and so forth.

[13] Schools with missing enrollment data in a given year are excluded from the sample for that year.

[14] Several educational input variables are unavailable for some years in the sample. To cope with this problem, we imputed the missing values via linear extrapolation, based on the coefficient estimates from simple ordinary least squares (OLS) regressions of the relevant variables on a time trend.

[15] We estimate two production functions for each of the two education output variables: one without and one with school district fixed effects. The adjusted R^2 without fixed effects is 0.83 for the math score regression and 0.92 for the reading score regression. When including fixed effects, the adjusted R^2 increases to 0.92 for the math score regression and to 0.96 for the reading score regression.

two variables that likely are correlated with the cost of education. This is because of the greater cost of educating students from disadvantaged backgrounds and those in need of remedial language courses. Therefore, we expect a negative relationship between these variables and house values. Finally, homeowners appear to be sensitive to the racial composition of the local schools, usually yielding negative correlations between house prices and the representation of black and Hispanic students, and yielding a positive correlation between house values and the share of Asian students.[16]

As shown in table 4, educational inputs do not show a strong trend over the time period we study. There is a large increase in the percentage of teachers with master's degrees, but there is a large decrease in the percentage of teachers with more than five years of experience. The teacher-pupil ratio and attendance rates both increase modestly. The racial mix in schools seems to have changed little, on average, between 1988 and 1998. The percentages of Asian students and Hispanic students increase by 3.7 and 1.4 percentage points, respectively, and the percentage of black students declines by 0.8 percentage points. However, these small average changes mask heterogeneity in the dynamics of CSD racial composition. For example, the change in the percentage of blacks varies between –9.5 and 14.1 percentage points, with a standard deviation of 4.4 percentage points.

Housing Production, Renovation, and Investment

The New York City Department of Housing Preservation and Development (HPD) provided us with data on the precise location (address, borough, community district, block, and lot) of all housing built or renovated under the city's Ten-Year Capital Plan, as well as of a significant number of other publicly supported projects completed between 1976 and 1998 (e.g., projects sponsored by the federal government and pre-1987 city-sponsored projects).[17] Announced in 1985 by former mayor Edward I. Koch, the Ten-Year Plan represents the largest local housing investment initiative in U.S. history, encompassing a wide variety of programs to stimulate the production and rehabilitation of housing (see the article by Schill et al. [2002] for more detail).[18] From HPD data, we created annual community district–level counts of Ten-Year Plan projects by tenure type (i.e., homeownership and rental), as well as counts of all other projects. After attaching these project counts to the sales data sets, three housing investment variables were computed for use in regressions. These variables show for each sale the log number of projects in a given category completed up to the year of sale (including the year of sale).[19]

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[16] Although a very interesting and complex issue, the justification of the racial mix effect on house values is not within the scope of this article.

[17] HPD data do not cover public housing. Also, before 1987, the data coverage excludes many projects undertaken by federal programs.

[18] For discussions of the origins of the Ten-Year Plan, see the articles by Schwartz (1999) and Van Ryzin and Genn (1999).

[19] We use projects instead of units because our earlier research (Ellen, Schill, Schwartz, and Voicu [2001]; Ellen, Schill, Susin, and Schwartz [2001]) showed a large project fixed effect but relatively small—and sometimes not significant—scale (i.e., number of units) effects.

Table 4. **New York City Education Statistics Based on School District–Level Weighted Means, 1988 to 1998**

Variable	Year 1988	Year 1998	10-Year Change, 1988 to 1998
Percent of teachers with more than five years of experience			
Mean	75.0	62.3	−12.7
Minimum	64.0	52.7	−24.8
Maximum	94.6	74.4	0.7
Standard deviation	8.1	5.1	5.7
Percent of teachers with a master's degree			
Mean	55.1	78.1	23.0
Minimum	25.4	65.4	7.8
Maximum	80.0	89.3	59.3
Standard deviation	11.9	7.0	8.9
Teacher-pupil ratio (percent)			
Mean	5.9	6.1	0.2
Minimum	5.3	5.2	−1.5
Maximum	6.9	6.9	1.1
Standard deviation	0.4	0.5	0.5
Percent of students attending daily			
Mean	87.8	90.4	0.5
Minimum	82.9	86.6	0.4
Maximum	92.8	94.8	5.2
Standard deviation	2.6	2.0	1.3
Percent of students with limited English proficiency			
Mean	10.4	16.2	5.8
Minimum	1.4	3.4	0.5
Maximum	30.3	44.4	14.0
Standard deviation	6.0	9.2	4.0
Percent of students who receive free lunch			
Mean	63.3	76.9	13.6
Minimum	13.6	22.1	−0.8
Maximum	84.4	93.7	26.8
Standard deviation	18.8	16.8	7.0
Percent of Asian students			
Mean	6.7	10.4	3.7
Minimum	0.1	1.0	−1.3
Maximum	34.1	38.9	17.8
Standard deviation	8.4	11.3	4.5
Percent of black students			
Mean	38.7	37.9	−0.8
Minimum	7.2	4.3	−9.5
Maximum	88.8	88.3	14.1
Standard deviation	25.4	26.8	4.4

Table 4. **New York City Education Statistics Based on School District–Level Weighted Means, 1988 to 1998** *(continued)*

Variable	Year 1988	Year 1998	10-Year Change, 1988 to 1998
Percent of Hispanic students			
Mean	36.3	37.7	1.4
Minimum	5.9	7.2	−9.2
Maximum	82.8	89.3	15.2
Standard deviation	23.5	23.6	5.6
Percent of white students			
Mean	18.3	14.0	−4.3
Minimum	0.0	0.3	−5.3
Maximum	74.9	63.6	5.3
Standard deviation	20.1	16.3	6.8
Average school enrollment			
Mean	928	939	1.7
Minimum	591	407	−49.0
Maximum	1,418	1,568	32.3
Standard deviation	201	234	17.0

Source: Authors' calculations based on data from New York City Board of Education.
Note: Statistics are based on all 32 New York City elementary and middle school districts. The school district–level variables are weighted means of the corresponding school-level variables, with weights given by school enrollment. For enrollment, statistics are shown for the 10-year percent change; for the other variables (which are themselves percentages), statistics on the 10-year change are shown.

As discussed above, we expect a positive relationship between housing investment and property values. There are several reasons why public investments in housing production or rehabilitation might be expected to raise the value of surrounding properties. First, they replace blighted properties or land with new structures or improve existing structures in need of rehabilitation. Because housing is fixed in space, the enhanced physical appearance of the houses produced or rehabilitated should generate, in principle, spillover benefits that could be capitalized into the value of neighboring properties.[20] Second, where programs actually create new housing, population may grow, which may promote new commercial activity, a greater sense of safety, and general economic growth. Third, as vacant and derelict land is converted into habitable housing, nearby property owners may decide to remain in the community rather than move away. They also may be more likely to invest in maintaining their own homes, thereby generating additional positive neighborhood effects (Galster 1987).

[20] In addition to physical regeneration, rehabilitation of abandoned and deteriorated properties may reduce neighborhood crime rates, which, in turn, leads to higher property values. However, this spillover mechanism is not relevant in our case because we explicitly control for crime in the house price regressions.

Table 5 displays summary statistics for the housing investment variables. For the Ten-Year Plan, the average number of homeownership projects completed until 1998 is slightly higher (92) than the number of completed rental projects at that time (80).[21] The relatively large standard deviations reveal substantial variation across community districts. This is not surprising, because more than half the Ten-Year Plan housing units are located in just 10 of the city's 59 community districts (Schill et al. 2002). The average number of "other" projects completed by 1998 is much lower (54); almost all of these were completed before 1988 (51).[22]

Table 5. **New York City Housing Investment Statistics Based on Community District–Level Data, 1988 to 1998**

	Year Completed		10-Year Change,	Completed until		10-Year Change,
Variable	1988	1998	1988 to 1998	1988	1998	1988 to 1998
Ten-Year Plan homeownership projects						
Mean	5.69	7.24	1.54	8.47	91.81	83.34
Minimum	0.00	0.00	−19.00	0.00	0.00	0.00
Maximum	32.00	61.00	54.00	52.00	462.00	410.00
Standard deviation	7.74	10.28	10.96	11.99	99.29	90.11
Ten-Year Plan rental projects						
Mean	4.76	3.80	−0.97	7.63	80.39	72.76
Minimum	0.00	0.00	−14.00	0.00	0.00	0.0
Maximum	32.00	44.00	28.00	38.00	699.00	664.00
Standard deviation	6.37	8.02	5.94	9.85	121.23	114.69
Other projects						
Mean	0.51	0.05	−0.46	51.63	54.32	2.69
Minimum	0.00	0.00	−5.00	0.00	0.00	0.00
Maximum	5.00	1.00	1.00	235.00	238.00	17.00
Standard deviation	0.97	0.22	0.97	56.64	58.03	3.73

Source: Authors' calculations based on data from New York City Department of Housing Preservation and Development.
Note: Statistics are based on all 59 New York City community districts. "Other projects" includes projects sponsored with federal funds and pre-1987 city-sponsored projects.

[21] Note that, on average, the homeownership projects are much smaller than the rental projects. As shown by Schill et al. (2002), the number of units for homeowners represents only one-fifth of the total number of units built or renovated under the Ten-Year Plan.

[22] The relatively low number of other projects is probably in part because of the incomplete coverage of these projects in the HPD database. The fact that most of them were completed before 1988 is not surprising. Some of these projects are pre-1987 city-sponsored projects, and some are federally sponsored projects, which have declined sharply since the mid-1980s (mainly because of major reductions in federal funds for Section 8 housing that occurred during the Reagan administration).

Methodology

The centerpiece of this research is a hedonic model in which housing prices are modeled as a function of the structural characteristics of the house and the characteristics of the neighborhood, because consumers value the elements of the bundle that compose a housing unit. Factors such as the size of the house and the quality of the local neighborhood can be thought of as characteristics purchased with a housing unit. Supply and demand shifters do not enter the model; instead, they are captured by an annual "pure price" of housing that remains after accounting for the hedonic factors. A simple hedonic house price model is written:

$$P_{int} = \alpha + \beta_{\mathbf{Z}}\,\mathbf{Z}_{in} + \beta_{\mathbf{X}}\,\mathbf{X}_{int} + \beta_{\mathbf{W}}\,\mathbf{W}_{n} + \beta_{\mathbf{G}}\,\mathbf{G}_{nt} + \rho\,\mathbf{I}_{t} + \varepsilon_{int}, \quad (1)$$
$$i = 1, \ldots ,\mathbf{I};\ n = 1, \ldots ,N;\ t = 1, \ldots ,T.$$

P_{int} represents the logarithm of the price of house i in neighborhood n at time t. $\mathbf{Z}_{in}$ represents a vector of time-invariant characteristics of house i in neighborhood n, e.g., lot size and frontage. $\mathbf{X}_{int}$ represents a vector of time-varying characteristics of house i in neighborhood n at time t, e.g., floor space and number of apartments (a multifamily building characteristic). $\mathbf{W}_n$ represents a vector of time-invariant characteristics of neighborhood n, e.g., size, borough, and location. $\mathbf{G}_{nt}$ represents a vector of time-varying characteristics of neighborhood n at time t, e.g., government services, crime, and quality of public schools. $\mathbf{I}_t$ represents a vector of dummy variables that take a value of one in observations for houses in year t. ε_{int} represents an error term with the usual properties. That is, the price of the ith house in the nth neighborhood in time t is determined by both the time-varying and the time-invariant characteristics of the house itself, by the time-varying and the time-invariant characteristics of the neighborhood, and by the general price level.

We interpret the coefficients for the time dummies ($\mathbf{I}_t$) as the pure price of housing (holding housing unit characteristics constant). The magnitudes of the time coefficients are determined by demand and supply factors, such as income and construction costs, that affect the pure price of housing. Equation 1 yields estimates of the marginal effects of housing characteristics and neighborhood characteristics on house prices (βs), as well as estimates of the price indices ρ. Note, however, that this requires a great deal of detailed data. Unfortunately, if there are variables that are unmeasured or for which data are unavailable, such as location or amenities such as parks or mass transit, coefficients for the included variables may be biased—including the coefficients for the other housing characteristics and the estimated price indices. The extent of the bias depends on the extent to which the omitted variables are correlated with the included variables.

One approach to addressing any omitted variable bias uses repeat-sales data. More specifically, if the omitted variables are time-invariant characteristics of the house or the neighborhood, an unbiased estimate of the price indices can be formed by making use of multiple observations on the ith house. Writing an equation for the logarithm of the price of the ith house in neighborhood n in time t', following equation 1, yields

$$P_{int'} = \alpha + \beta_{\mathbf{Z}}\,\mathbf{Z}_{in} + \beta_{\mathbf{X}}\,\mathbf{X}_{int'} + \beta_{\mathbf{W}}\,\mathbf{W}_{n} + \beta_{\mathbf{G}}\,\mathbf{G}_{nt'} + \rho\,\mathbf{I}_{t'} + \varepsilon_{int'}. \quad (2)$$

Subtracting equation 2 from equation 1 gives an equation for the change in the log price of housing:

$$P_{int} - P_{int'} = \beta_{\mathbf{X}} (\mathbf{X}_{int} - \mathbf{X}_{int'}) + \beta_{\mathbf{G}} (\mathbf{G}_{nt} - \mathbf{G}_{nt'}) + \rho (\mathbf{I}_t - \mathbf{I}_{t'}) + \varepsilon_{int} - \varepsilon_{int'} \tag{3}$$

or

$$\Delta_{t,t'} P_{in} = \beta_{\mathbf{X}} \Delta_{t,t'} \mathbf{X}_{in} + \beta_{\mathbf{G}} \Delta_{t,t'} \mathbf{G}_n + \rho \Delta_{t,t'} \varepsilon_{in}, \tag{4}$$

where $\Delta_{t,t'}$ indicates change between time t and time t'.

Equation 4 can be estimated directly using only information on the changes in the price of the house, changes in the time-varying characteristics of the house and the neighborhood, and dummies for the year t and year t', and by using a data set that includes a sufficient number of repeat sales. If all the omitted variables are time invariant, equation 4 does not suffer from omitted variable bias, because only the time-varying characteristics remain in the equation. Further, if the structural characteristics of the house remain essentially unchanged (or the sample includes only houses with constant structural characteristics), equation 4 can be rewritten:

$$\Delta_{t,t'} P_{in} = \rho \Delta_{t,t'} \mathbf{I} + \beta_{\mathbf{G}} \Delta_{t,t'} \mathbf{G}_n + \Delta_{t,t'} \varepsilon_{in}. \tag{5}$$

The change in the log price of a house between time t and t' is determined by the change in the overall price level and the change in the neighborhood characteristics. Equation 4 or 5 yields estimates of the effect of changes in neighborhood characteristics on the price of housing ($\beta_{\mathbf{G}}$), as well as estimates of the price indices ρ that are free of any bias due to the omission of time-invariant characteristics of the neighborhood. Thus, the centerpiece of our empirical work in this article is a model of this type in which the time-varying neighborhood characteristics include crime and schooling characteristics, as well as data on housing investment.

The cost of this repeat-sales approach, however, is that the model yields no estimates of the effects of variables that are time-invariant or that change only rarely or slowly. Further, using a repeat-sales methodology limits the sample to houses that have sold at least twice in the study period. Hence, the repeat-sales sample size is small, and the sample may be nonrandom. To test the sensitivity of our results to different specifications, we also report estimates from a hedonic model, of the kind specified in equation 1.

A potential drawback of using transactions data is that we only observe the price of a building when it is sold, so buildings that turn over less frequently are underrepresented in the sample. If we do not account for this, the results will be representative of the population of buildings that change hands, rather than of the population of New York City. For example, our hedonic sample contains about as many observations in Staten Island as in Manhattan, although Manhattan has triple the population (table 2).[23] To make the estimates of the price of housing in New York City representative of the city's population, all regressions are

[23] This is true of all studies based on actual transactions data, but it is particularly important in New York City because of its high density. Manhattan is extremely dense and has fewer, but larger, buildings than Staten Island. Hence, there are fewer sales of buildings in Manhattan.

weighted by the population per sale (in the police precinct). Under this weighting scheme, for example, Manhattan has triple the weight of Staten Island in estimating the overall price of housing each year, in proportion to its population. However, producing appropriately weighted (i.e., unbiased) estimates reduces the precision of the estimates.

Possible Biases

Several possible sources of bias are worth discussing. First, it is argued frequently that residents of higher-income neighborhoods are more likely to report property crimes, biasing regressions toward finding a positive effect of property crimes on home prices. This is a likely problem in cross-section studies. However, it cannot be a problem in panel studies (such as this one) where neighborhood (or building) fixed effects capture such differences in reporting behavior. The same is true for all other neighborhood characteristics that we do not include in our regression, such as the neighborhood poverty rate or homeownership percentage. The exclusion of any neighborhood characteristics that are fixed over time will not cause bias.

We estimate regressions using changes in variables, rather than the levels, which reduces the potential for bias. For example, in repeat-sales models, the change in price for each building is regressed on the change in crime in the surrounding neighborhood. Hence, the results are biased only if *changes* in neighborhood characteristics are correlated in some way with our hedonic measures (crime, educational quality, and housing investment). This is possible, but it is unlikely to be a significant source of bias, because the socioeconomic characteristics of neighborhoods change only slowly. This specification represents a significant improvement over much of the existing literature.

Second, a natural concern is that city officials may have aimed to select "strong" sites for subsidized housing investments, places where they believed property values were beginning to increase (or were likely to). However, even if city officials had wanted to choose promising sites, they faced considerable constraints. First, the site had to be owned by the city, which meant it had to have been abandoned by its previous owner and vested in an in rem proceeding for delinquent property taxes. The city's stock of abandoned properties is overwhelmingly concentrated in its poorest neighborhoods (Scafidi et al. 1998). Second, certain programs required large, mostly vacant, contiguous parcels of land on which to build.

In any case, interviews with city officials suggest that the city did not give its best vacant sites to subsidized developments. In many instances, the city was also interested in realizing a high return from its land holdings.[24] Overall, the process of selecting individual sites, although perhaps not fully random, was far from one that sought to pick winners systematically.

[24] As Anthony Gliedman, a former Commissioner of New York City's Department of Housing Preservation and Development, stated, "Why would we do market-rate sites with the Partnership [a major program developing homeownership projects]?" (Orlebeke 1997).

Decomposing House Price Appreciation by Neighborhood Quality

To gauge the relative importance of the various neighborhood determinants of property values, we decompose house price appreciation trends into neighborhood quality components. The component of appreciation rate due to changes in a variable (X) is computed by taking the difference between a price index that accounts for that variable (i.e., holds it constant) and a price index that does not account for it. One method we use to estimate the two price indices is to estimate a house price regression twice. First, we estimate a "short" regression with X excluded to obtain an estimator of the price index that does not account for X. Second, we estimate a "long" regression with X included to obtain the price index that does account for X. More specifically, we estimate several repeat-sales regressions in which we add neighborhood variables sequentially. First, we estimate a regression with year dummies only; second, we add in crime variables; and third, we add in school and housing investment variables. The first regression provides estimates of price indices that do not account for any time-variant neighborhood characteristics. The second regression gives estimates of price indices that account for crime (but not for schools and housing investment). The third regression computes estimates of price indices that account for crime, schools, and housing investments. The price appreciation component due to changes in crime then can be computed by taking differences between the coefficients for the year dummies from the first and second regressions. Similarly, the component due to changes in school and housing investment variables is obtained by differencing the year dummy coefficients from the second and third regressions.

Zabel (1999) argued that this approach is flawed, because the short regression clearly suffers from omitted variable bias. He proposed estimating only a single regression, with the full set of controls, and then decomposing the index as follows:

$$\Delta \hat{r} = \Delta \hat{\rho} + \Delta \overline{X}_1 \hat{\beta}_1 + \Delta \overline{X}_2 \hat{\beta}_2 + \ldots + \Delta \overline{X}_K \hat{\beta}_K \tag{6}$$

Here, $\Delta \hat{r}$ is the change (from one year to the next) in the unadjusted price index (i.e., from a regression without any Xs). Similarly, $\Delta \hat{\rho}$ is the change from one year to the next in the adjusted price index (from a regression with Xs). Recall that this is the pure price of housing that reflects supply and demand factors but not hedonic effects. The price appreciation due to the hedonic measures, X_i, is $\Delta \overline{X}_i \hat{\beta}_i$ for each of the K X-variables.[25]

This decomposition is straightforward in a hedonic regression or in a repeat-sales regression where properties sell each year, which were the cases that Zabel discussed. In the usual repeat-sales context, where properties do not sell every year, there is a practical difficulty with this formulation—the change in X is observed only when a sale occurs. For example, when a property sells in the first and third periods, X is not observed in the second period. We might consider calculating the average change in X between the first and second periods using only buildings that sold in the first and second periods. Alternatively, we might consider calculating this statistic using information on X in the second period that was not used in the repeat-sales regression. However, both possibilities seem unlikely to preserve the "adding up"

[25] The equality holds in general because the OLS regression line always goes through the mean of the data.

property of equation 6. We want to use all the information on X that the repeat-sales regressions use, not more or less.[26]

To calculate the average change in X between any two consecutive years, we use a repeat-sales estimate of the average value of X in any year. That is, we regress the change in X for each building on the change in year dummies,[27] just as a repeat-sales index regresses the change in price on the change in year dummies. One such auxiliary regression is estimated for each time-varying X in our data set. The coefficients for the change in year dummies are estimates of the average X in any year, which are consistent with the repeat-sales estimates of the price indices. These estimates can be differenced, giving us the average change in X from year to year, which is what we need. This method of calculating $\Delta \overline{X}_i \widehat{\beta}_i$ preserves the accounting equality of equation 6.

Using this method, we decompose the annual and cumulative growth rates in real house prices into the following components:

1. Pure price change (given by the coefficient estimates for the year dummy variables)

2. Return due to change in violent crime

3. Return due to changes in other crime variables (property crime and misdemeanor arrests)

4. Return due to change in school quality

5. Return due to change in the composition of the school community

6. Return due to change in housing investment

Results

Crime and the City

Figure 1 displays violent crime rates and property values from 1975 to 1998. The price index is calculated using the repeat-sales methodology, and it is adjusted for inflation using the consumer price index for urban consumers (CPI-U-X1).[28] After 1980, property values boomed as New York City recovered from the fiscal crisis of the mid-1970s and the national recession of 1980 to 1982. Prices peaked around 1989, falling by about 35 percent (log points) by 1994,

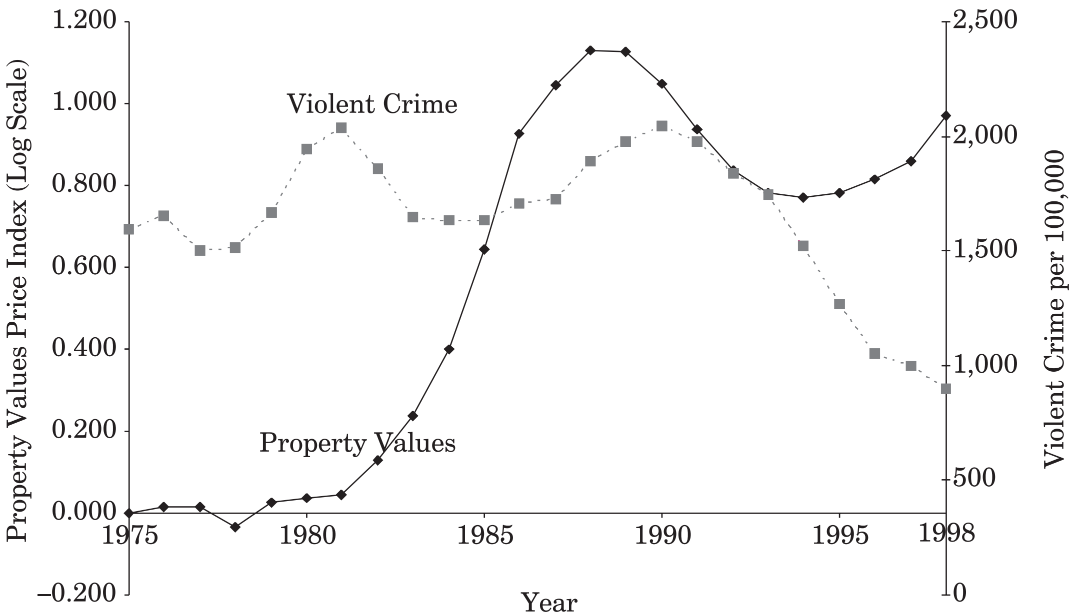
[26] In this study, we do observe X in the second period, but that information is not included in the repeat-sales regression.

[27] We use the same weights (population per sale in the precinct) that we use in the main regressions.

[28] Prior to 1983, CPI-U overestimated inflation in the price of owner-occupied housing. In 1983, the Bureau of Labor Statistics shifted to a rental-equivalence approach to measure owners' costs, and it issued the CPI-U-X1 to apply this correction to earlier years.

before recovering to about 15 percent below the peak by 1998. Thus, compared with the 1989 to 1994 decline, the post-1994 boom is relatively modest.

Figure 1. **Property Values and Violent Crime**



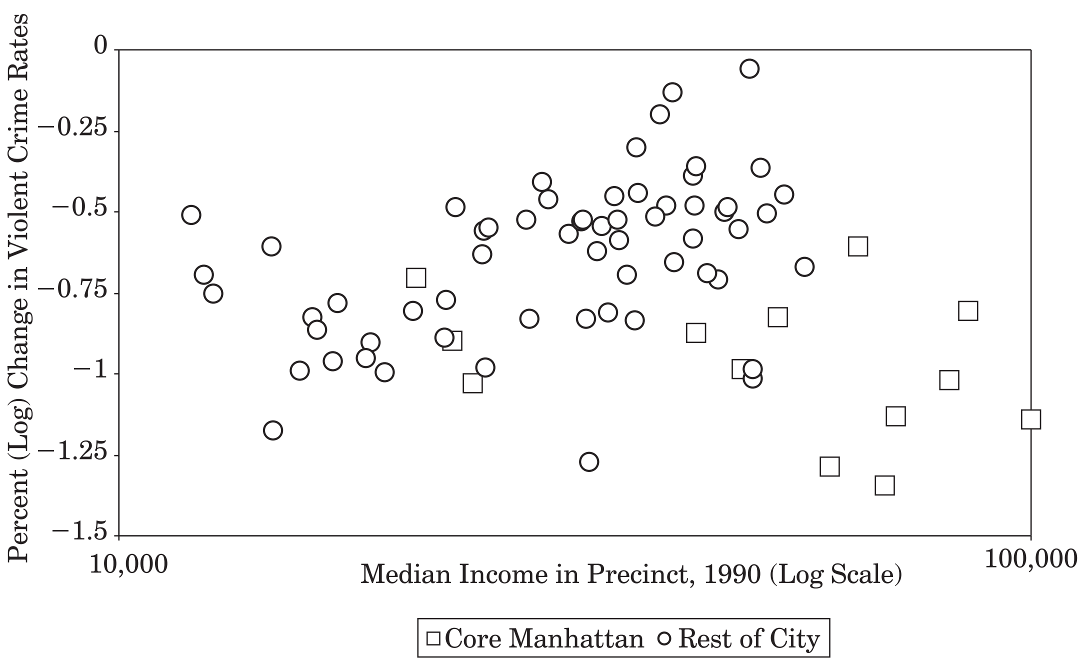
Violent crime fluctuated, displaying no strong pattern until 1990, after which it fell sharply. Eyeballing the two curves reveals no strong correlation. Although there is some tendency for crime and property values to move inversely in the later years shown in the figure, it is not strong. Crime fell steadily after 1990, while property values first declined and then rose.

Across the city, crime fell and property values rose most sharply in poorer areas, with core Manhattan, the area with the city's highest incomes and home prices, as a notable exception.[29] As shown in figures 2 and 3, core Manhattan saw crime fall as much as, if not more than, any area of the city, and property values rose more (fell less) than would have been predicted from the area's high income. In other words, both crime and property value changes display a U-shaped pattern. New York City's turnaround was strongest in both its richest and poorest areas. The city's middle-income areas experienced the smallest fall in crime rates, and they were the least affected by the upturn in property values of the late 1990s.

[29] Core Manhattan usually is defined as Manhattan south of 96th street. We approximate this definition with police precincts.

Figure 2. **Property Values and Income, 1988 to 1998**

Figure 3. **Violent Crime and Income, 1988 to 1998**



These simple descriptive figures go a long way toward telling the story that we examine more formally below. Property values rose most (fell least) in areas of the city where crime fell the most. The simple correlation between these measures across precincts is −0.48. In addition, the figures highlight a little-discussed aspect of New York City's late 1990s boom, the remarkable revival of the city's poorest areas.[30] Interestingly, these are the areas where the city made the most intensive investments in rebuilding the housing stock.

Hedonic Regression Results

The first two columns of table 6 display results from cross-section hedonic regressions for 1990 and 1995, which include many building characteristics, such as age, log square feet per unit, and a set of 18 building-type indicators (e.g., single-family detached, two-family home, and elevator apartment building). Both regressions perform well, explaining 77 percent and 76 percent of the variation in house prices. The coefficient for violent crime is −0.185 in 1990 and −0.137 in 1995. These are large and statistically significant effects, but they are not implausible. Because these variables are measured as log crime per capita, and violent crime fell by roughly 50 percent over the period, they indicate that this magnitude of crime drop could be expected to raise property values by roughly 7 to 9 percent.

The third column reports the estimates from the panel hedonic regression, including fixed effects for the intersection of precincts and school districts, thus controlling for any time-invariant neighborhood characteristics. Here, the coefficient for violent crime drops slightly from the 1995 value and substantially from the 1990 value, to −0.13, remaining statistically significant at the 1 percent level. The coefficient for property crime is negative, as expected, although it is not statistically significant. These results indicate that the fall in crime since 1988 has raised property values by about 6 percent (again, taking half the coefficient).

These results are in line with our expectations about the importance of omitted neighborhood characteristics in models of this type. A natural interpretation is that the cross-section effect of violent crime is biased toward too large an effect, because crime rates are proxies for other characteristics of the neighborhood.

In this regression, a variable capturing misdemeanor arrests is included to proxy for the zero tolerance policing, as discussed above. Zero tolerance policing should raise property values to the extent that it reduces public nuisances.[31] However, it could lower property values to the extent that police enforcement is misdirected at law-abiding neighborhood residents.[32] Here,

[30] This renaissance also has been documented by Daniels and Schill (2001).

[31] Zero tolerance policing also should raise property values if it reduces crime, but we presumably have controlled for that. Misdemeanor arrests might also proxy for misdemeanor crimes, which would alter our interpretation of the variable. We do not explore this possibility because we estimate generally small and insignificant effects of felony property crimes, and because, as discussed above, misdemeanor arrests and offenses do not move closely together in a time series.

[32] There have been numerous press accounts of overly aggressive policing in New York City. For an academic view, see, for example, the articles by Fagan and Davies (2000) and Fagan, Zimring, and Kim (1998).

Table 6. **House Price Hedonic Regressions**

Variable	Cross-Section, 1990	Cross-Section, 1995	Pooled Cross-Section, Time Series, 1988 to 1998
Violent crime	−0.1854***	−0.1374**	−0.1301***
(log rate per 100,000)	(0.0567)	(0.0656)	(0.0361)
Property crime			−0.0097
(log rate per 100,000)			(0.0569)
Misdemeanor arrests			0.0237
(log rate per 100,000)			(0.0245)
Percent of teachers with more	−0.0006	−0.0032	0.0035
than five years of experience	(0.0082)	(0.0094)	(0.0027)
Percent of teachers with	−0.0193**	−0.0059**	0.0003
a master's degree	(0.0079)	(0.0028)	(0.0005)
Average school enrollment (logs)	0.6279*	−0.0157	−0.0103
	(0.3469)	(0.1856)	(0.2282)
Teacher-pupil ratio (percent)	0.4951***	−0.0301	−0.0079
	(0.1706)	(0.1335)	(0.0204)
Percent of students attending daily	0.0609***	0.0397*	0.0069
	(0.0202)	(0.0216)	(0.0095)
Percent of students who are:			
Eligible for free lunch	−0.0018	0.0063	−0.0016
	(0.0050)	(0.0039)	(0.0019)
Black	−0.0020	−0.0041*	−0.0056
	(0.0026)	(0.0023)	(0.0044)
Hispanic	0.0019	–0.0060**	−0.0098***
	(0.0034)	(0.0026)	(0.0038)
Asian	0.0315***	0.0090*	−0.0049
	(0.0079)	(0.0046)	(0.0042)
Limited English proficient	−0.0341***	−0.0064	−0.0040
	(0.0128)	(0.0053)	(0.0031)
Ten-Year Plan homeownership	−0.0322	−0.1090***	0.0588**
housing projects (logs)	(0.0299)	(0.0227)	(0.0229)
Ten-Year Plan rental housing	−0.0756***	−0.0126	0.0045
projects (logs)	(0.0265)	(0.0290)	(0.0147)
Other housing projects (logs)	0.0607***	0.0333*	−0.1343**
	(0.0228)	(0.0189)	(0.0632)
Year 1989			−0.0298
			(0.0206)
Year 1990			−0.1006***
			(0.0348)
Year 1991			−0.2068***
			(0.0453)
Year 1992			−0.2742***
			(0.0449)
Year 1993			−0.2938***
			(0.0464)
Year 1994			−0.3262***
			(0.0535)

Table 6. **House Price Hedonic Regressions** *(continued)*

Variable	Cross-Section, 1990	Cross-Section, 1995	Pooled Cross-Section, Time Series, 1988 to 1998
Year 1995			−0.3706***
			(0.0575)
Year 1996			−0.3864***
			(0.0667)
Year 1997			−0.3664***
			(0.0723)
Year 1998			−0.2847***
			(0.0934)
R^2	0.770	0.758	0.816
N	20,532	21,726	246,743

Note: The dependent variable is log real house price per unit. All regressions are weighted by precinct population/number of sales. The Pooled Cross-Section, Time Series regression includes precinct × community school district indicators. All regressions include the following variables capturing characteristics of the property sold: building age and its square, log square feet per unit, the number of buildings on a lot, dummies for the presence of commercial units, extension, major alteration prior to sale, location on a block corner, vandalized buildings, other abandoned buildings, and odd shape. Also included are a set of 18 building classification dummies: single-family detached; two-family home; three-family home; four-family home; five/six-family home; more than six families, no elevator; walkup, units not specified; elevator apartment building, cooperatives; elevator apartment building, not cooperatives; loft building; condominium, single-family attached; condominium, walk-up apartments; condominium, elevator building; condominium, miscellaneous; multiuse, single-family with store; multiuse, two-family with store; multiuse, three-family with store; and multiuse, four or more families with store. All regressions are estimated with robust standard errors to relax the assumption of independence of observations within a given precinct–community school district cluster. Standard errors are in parentheses.
*$p < 0.10$. **$p < 0.05$. ***$p < 0.01$.

its coefficient is small, positive, and not statistically significant, which may reflect no effect of zero tolerance policing or that the positive and negative effects cancel each other out.

Turning to educational quality and housing investment, many of the relevant coefficients in the cross-section hedonic regressions have unexpected signs or implausible magnitude, perhaps because of omitted variable bias. For example, the coefficients for teacher education, teacher experience, and housing investments have counterintuitive signs. In 1990, the coefficients for average school enrollment and teacher-pupil ratio have implausibly large magnitudes. The coefficient estimates have more plausible signs and magnitudes in the panel hedonic regression. The coefficients for education inputs have, in general, expected signs (the only exception is the negative coefficient for the teacher-pupil ratio), but none of these coefficients is statistically significant. Among the coefficients for school community variables, the only significant finding is the negative coefficient for representation of Hispanic students. If the percentage of Hispanic students rises by one percentage point, on average, a one percent drop in house values would result.

As also found by Ellen, Schill, Susin, and Schwartz (2001) and Schill et al. (2002), the Ten-Year Plan housing investments have a positive effect on house values, and this effect is statistically significant for homeownership projects. A 10 percent increase in the number of

Ten-Year Plan units in the community district results in a 0.5 percent increase in property values, on average. The negative bias in the cross-section estimates of the effects of the housing units is consistent with the location of these projects in very distressed neighborhoods. At the same time, the negative and significant effect of old city-sponsored and federally sponsored projects is somewhat surprising but consistent with findings in several recent studies of the negative effects from federally subsidized rental developments (Goetz, Lam, and Heitlinger 1996; Lee, Culhane, and Wachter 1999; Lyons and Loveridge 1993). The positive bias in the cross-section effect of these projects is also surprising, because they, too, were located predominantly in poor neighborhoods.

Repeat-Sales Regression Results

As shown in table 7, repeat-sales regressions are estimated three ways: (1) with no control variables, (2) with the crime variables added, and (3) with all control variables added. As shown in the second column, the violent crime coefficient is negative and statistically significant, and its magnitude is large (-0.24). The coefficients for property crime and misdemeanor arrests are positive but small and not statistically significant. However, these crime effect estimates may suffer from omitted variable bias if the crime variables are correlated with other time-varying socioeconomic factors that influence house prices but are not included in the regression.

Thus, we expand the specification to include additional neighborhood characteristics capturing school quality, community, and public housing investments (the third column in table 7). The results seem to confirm our suspicion. When the additional variables are included, the effect of violent crime drops to –0.12, a magnitude very similar to that of the panel hedonic regression estimate. Likewise, the coefficient for property crime, although still positive and not significant, is reduced by half. The magnitude and sign of the misdemeanor arrest coefficient are not affected, but its standard error declines, and it becomes statistically significant, suggesting that the positive effect of the zero tolerance policing on house prices prevails, although the effect is not large.

The repeat-sales coefficient estimates for the education and housing investment variables in most cases are reasonably close to their counterparts in the panel hedonic regression. Among the education inputs, the only notable differences compared with the hedonic regression results are that the coefficient for teacher-pupil ratio, although still not significant, now has the "right" sign (i.e., positive, in accordance with the intuition that more resources are valued by house buyers). Further, the effect of daily attendance becomes significant, albeit only at the 10 percent level. An increase of one percentage point in the average attendance rate in the school district leads to a 2 percent increase in property values, on average. One should be cautious about drawing strong conclusions about the effect of educational quality, because the variables are jointly insignificant ($F = 1.05, p = 0.39$).

Table 7. **House Price Repeat-Sales Regressions**

Variable	Basic Regression	Regression with Crime	Regression with Crime, Education, Demographics, and Housing Investments
Violent crime		−0.2422***	−0.1157**
(log rate per 100,000)		(0.0520)	(0.0485)
Property crime		0.0680	0.0324
(log rate per 100,000)		(0.0794)	(0.0638)
Misdemeanor arrests		0.0376	0.0379*
(log rate per 100,000)		(0.0274)	(0.0219)
Percent of teachers with more			0.0017
than five years of experience			(0.0026)
Percent of teachers with			0.0002
a master's degree			(0.0006)
Average school enrollment (logs)			−0.0004
			(0.1936)
Teacher-pupil ratio (percent)			0.0071
			(0.0314)
Percent of students attending daily			0.0204*
			(0.0110)
Percent of students who are:			
Eligible for free lunch			0.0016
			(0.0021)
Black			−0.0057
			(0.0038)
Hispanic			−0.0079**
			(0.0034)
Asian			−0.0024
			(0.0034)
Limited English proficient			−0.0106***
			(0.0035)
Ten-Year Plan homeownership			0.0524***
housing projects (logs)			(0.0177)
Ten-Year Plan rental housing			0.0539***
projects (logs)			(0.0176)
Other housing projects (logs)			−0.1332
			(0.1089)
Year 1989	0.0172	0.0303**	−0.0346**
	(0.0143)	(0.0150)	(0.0183)
Year 1990	−0.0325**	−0.0019	−0.1003***
	(0.0157)	(0.0198)	(0.0286)
Year 1991	−0.1487***	−0.1156***	−0.2262***
	(0.0217)	(0.0266)	(0.0411)
Year 1992	−0.2494***	−0.2237***	−0.3525***
	(0.0202)	(0.0257)	(0.0493)
Year 1993	−0.2783***	−0.2611***	−0.3657***
	(0.0166)	(0.0219)	(0.0557)

Table 7. **House Price Repeat-Sales Regressions** *(continued)*

Variable	Basic Regression	Regression with Crime	Regression with Crime, Education, Demographics, and Housing Investments
Year 1994	−0.3023***	−0.3230***	−0.4097***
	(0.0278)	(0.0367)	(0.0609)
Year 1995	−0.3009***	−0.3629***	−0.4636***
	(0.0252)	(0.0450)	(0.0654)
Year 1996	−0.2887***	−0.3819***	−0.4603***
	(0.0197)	(0.0475)	(0.0735)
Year 1997	−0.2161***	−0.3203***	−0.4344***
	(0.0226)	(0.0502)	(0.0785)
Year 1998	−0.1271***	−0.2494***	−0.3635***
	(0.0267)	(0.0631)	(0.0970)
R^2	0.236	0.246	0.273
N	25,947	25,947	25,947

Note: The dependent variable is the change in log real house price. All regressions are weighted by precinct population/number of sales. All regressions are estimated with robust standard errors to relax the assumption of independence of observations within a given precinct–school district cluster. Standard errors are in parentheses.
$^{*}p < 0.10$. $^{**}p < 0.05$. $^{***}p < 0.01$.

Turning to school community variables, note that the coefficients for the race variables are remarkably close in magnitude to those estimated in the hedonic regression, and that the negative effect of the percentage of LEP students is now significant at the 1 percent level. An increase of one percentage point in the average percentage of LEP students in the school district is associated, on average, with a 1 percent decline in property values. Surprisingly, the coefficient for the percentage of students eligible for free lunch, although still not significant, is now positive.

The effects of the Ten-Year Plan homeownership and rental projects are both positive and significant at the 1 percent level, and their magnitudes are similar. A 10 percent increase in the number of either homeownership or rental projects in the community district leads to an average increase in property values of approximately 0.5 percent. The coefficient on other housing projects, although still negative, is no longer statistically significant.

The overall similarity of the results from the panel hedonic regression and the repeat-sales regression is encouraging, suggesting that the combination of the fixed-effects specification and the rich set of explanatory variables used in the panel hedonic regression are sufficient, and omitted variable biases are not problematic.[33] Further, the similarity suggests that the repeat-sales sample is substantially representative of the whole sales sample.[34]

[33] In addition to the similarity of the neighborhood quality effects discussed above, the differences between the price-index coefficients in the two models are not statistically significant (as revealed by simple t-tests performed on pairs of corresponding coefficients).

[34] See the article by Meese and Wallace (1997) for a more formal test of the repeat-sales approach hypothesis that the repeat sales are representative of all homes sold during the sample period.

Alternate Specifications

The time dummies we use imply that all of New York City makes up a single housing market, which is a useful abstraction and simplifies the decomposition that we turn to below, but it is unlikely to be true. Instead, it is quite possible that demand and supply factors such as income and construction costs evolved differently in different parts of the city. A simple way of examining whether this specification causes bias is to replace the time dummies with a separate set of time dummies for each of the five boroughs. The main difference in this model is that the violent crime coefficient increases to −0.21. The education and housing investment results, as well as the other crime coefficients, are not much affected.

Another way to split the city into submarkets is by structure type. We estimate two separate regressions for one- to four-family houses and for larger buildings. The most notable differences are that the violent crime coefficient is larger for multifamily buildings (−0.22 versus −0.13), the school enrollment coefficient becomes statistically significant and increases to −0.28 for the smaller dwellings, and the effect of the Ten-Year Plan housing investments is one to two percentage points larger for the multifamily buildings. However, in none of these cases is there a statistically significant difference between the coefficients in the two regressions.

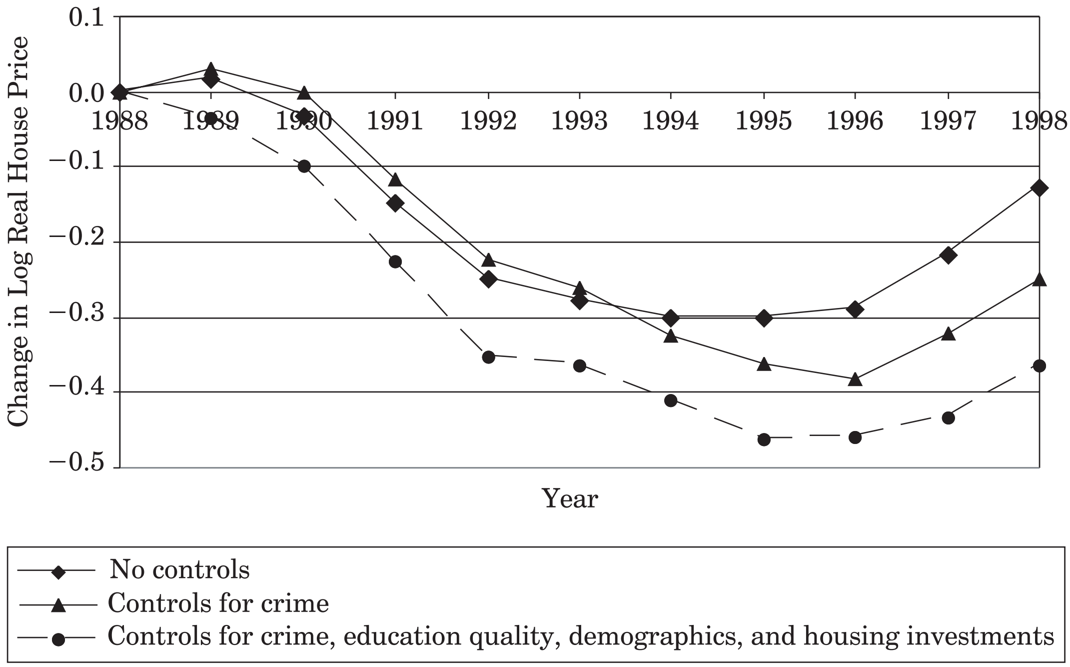
Effect of Neighborhood Quality Changes on Annual and Cumulative House Price Appreciation

Figure 4 plots the coefficients for the year indicators from the three regressions in table 7, displaying the citywide repeat-sales price indices estimated with various levels of control for house quality. Differences in slope between line graphs for a given year offer a visual evaluation of the effects of crime and other neighborhood characteristics on the annual appreciation rates.[35] As noted above, the magnitudes of these effects can be computed by taking differences between the coefficients for the year indicators from the three specifications.[36] The results indicate that controlling for crime causes little change in the pre-1994 price appreciation patterns, but it does have an effect on the post-1994 patterns. Controlling for crime lowers price indices by two percentage points in 1994, rising to 12 percentage points in 1998. Put simply, if crime had not fallen, prices would have risen less. Further, the fall in crime led to a systematic increase in the annual average growth rate in prices after 1993—three to four percentage points per year through 1996, and one to two percentage points in the final two years of our data. These findings are additional evidence that violent crime reduction played a role in the late 1990s boom in property values.

[35] Slope differences between the line graph for uncontrolled price indices and the line graph for crime-controlled indices reflect the effect of crime on annual appreciation. Slope differences between the line graph for crime-controlled indices and the line graph for indices with full controls reflect the joint effect of schools and housing investments.

[36] For example, the effect of crime changes on the 1991 to 1992 appreciation rate is given by: $(\rho_{92}{}^{nc} - \rho_{91}{}^{nc}) - (\rho_{92}{}^{c} - \rho_{91}{}^{c})$, where ρ_t^{nc} and ρ_t^{c} are the coefficient estimates for the year t indicator from the specification with no controls and the specification with crime controls, respectively.

Figure 4. **New York City Price Trends, 1988 to 1998**



Note: Changes in log real price are measured relative to the initial year, 1988.

Taken together, the results also suggest that changes in school quality, community, and publicly supported housing investments mitigated the abrupt drop in prices between 1989 and 1992. Holding these variables constant would have lowered the annual average growth rate in house prices by between 6.5 percentage points (in 1989) and 1.2 percentage points (in 1991). Further, changes in school and housing investment variables had a relatively small role in the post-1994 boom in New York City house values. They increased the average growth rate in prices in two years only—by 1.4 percentage points in 1995 and by 3.6 percentage points in 1997.

Although the above effect estimates are fairly simple to calculate and graph, they are based on regressions that do not include the full set of controls; therefore, they are subject to omitted variable bias. Hence, we also decompose house price appreciation into neighborhood quality components based on the regression with the full set of controls. The resulting repeat-sales estimators of the annual and cumulative appreciation rate components based on this approach are presented in table 8. A similar story as before emerges from these results. The annual return due to changes in violent crime rates is positive for most years, with larger values for the second half of the study period. For example, in the 1994 to 1998 period, the annual return varies between 0.8 and 2.0 percentage points, and the cumulative return is 6.0 percentage points, as compared with only 1.6 percentage points for the earlier years. The post-1994 cumulative return from the fall in violent crime represents approximately one-third the total real price appreciation for this period. The annual returns due to changes in the other crime variables are small (usually smaller than 0.5 percentage points) and negative for most years.

Table 8. **House Price Appreciation Rates (Percent Changes), Repeat-Sales Estimators**

Estimator	88–89	89–90	90–91	91–92	92–93	93–94	94–95	95–96	96–97	97–98	88–94	94–98	88–98
Pure price change (accounting for all controls)	−3.46	−6.56	−12.59	−12.63	−1.33	−4.39	−5.39	0.33	2.58	7.09	−40.97	4.62	−36.35
Change due to violent crime	−0.61	−0.69	0.05	0.60	0.66	1.59	2.02	1.96	0.78	1.27	1.60	6.03	7.63
Change due to other crime	−0.10	−0.44	−0.17	−0.22	−0.23	0.87	0.33	−0.45	−0.08	−0.40	−0.29	−0.60	−0.89
Change due to education inputs	0.67	0.80	0.29	0.09	−1.47	−1.02	3.22	−1.42	1.66	0.38	−0.64	3.83	3.19
Change due to school community	−0.16	−3.16	−2.19	0.50	−1.77	−0.23	−1.23	0.02	1.51	0.15	−7.00	0.45	−6.55
Change due to housing investment	5.38	5.08	2.99	1.58	1.25	0.78	1.19	0.78	0.80	0.42	17.06	3.19	20.25
Total price change	1.72	−4.97	−11.62	−10.07	−2.89	−2.40	0.14	1.23	7.25	8.90	−30.23	17.52	−12.71

Among the other neighborhood quality indicators, it is worth noting the consistently positive and, during the pre-1994 period, relatively large annual returns from housing investment. For the 1994 to 1998 period, housing investment has the largest cumulative return among all neighborhood characteristics, representing an increase of 17 percentage points in the appreciation rate. Annual returns from changes in education inputs are similar in magnitude to those from crime rate changes. However, for three years they are negative, which makes the cumulative return for the pre-1994 period negative (–0.6 percentage points), and it makes the cumulative return for the post-1994 period positive but relatively small (3.8 percentage points). The annual appreciation rate component due to changes in school community is predominantly negative before 1994 and mostly positive, albeit small, for the later years. Interestingly, the cumulative effect of the school characteristics controlled by the public sector—the education inputs—is positive. However, these variables are not statistically significant, and their effects are swamped by the effects of changes in the school community, which are not controlled by the public sector. One interpretation is that the reported dissatisfaction with public schools may reflect, at least in part, a response to the socioeconomic and demographic features of the students themselves, but that increased school inputs may have an offsetting effect. Disentangling these effects is certainly important and worthy of future research.

In the aggregate, changes in neighborhood quality indicators other than crime have a positive effect on the annual appreciation rate for all but three years. They also have a positive effect on both the 1988 to 1994 and the 1994 to 1998 cumulative appreciation rates. The pre-1994 cumulative return—which is due largely to housing investments—is 9.4 percentage points, and the post-1994 cumulative return—which is due almost equally to education quality and housing investments—is 7.5 percentage points. For the entire 1988 to 1998 period, the cumulative return from changes in school and housing investment variables is 16.9 percentage points.

Taken together, changes in neighborhood quality have a positive effect on price appreciation rates for most years during the study period. The positive annual return varies between 0.9 and 5.2 percentage points. The cumulative return for the entire period is substantial (23.6 percentage points), with the post-1994 return being somewhat larger than the return for 1988 to 1994 (12.9 vs. 10.7 percentage points).

Conclusion

New York City has experienced a large drop in crime over the past decade. Has falling crime driven the city's post-1994 real estate boom?

In this article, we address this question by using unique and extremely rich data and by employing both hedonic and repeat-sales house price models, which allow us to control for unobservable neighborhood and building-specific effects. These data and this methodology far surpass any previously used to study the effect of crime on property values. We find larger effects of violent crime on property values than have been estimated by previous researchers, and we find little effect of property crime. It must be noted, however, that these estimates may not be generalizable. As always, estimates are based on data from a particular time and place, and future work using data from different time periods and different locations is necessary to understand these relationships. We also find some evidence that there is a moderate positive bias in cross-sectional estimates of these coefficients.

We confirm the finding of Ellen, Schill, Susin, and Schwartz (2001) that areas with Ten-Year Plan housing investments enjoyed increased property values, using a quite different methodology (but similar data). Our results provide new evidence on the timing and overall magnitude of the effect of these investments. Our attempts to provide new estimates of the extent to which school quality capitalizes into building prices are less successful, yielding fairly imprecise estimates, possibly because there were no strong trends in New York City school quality during this period.

Finally, we decompose house price trends into neighborhood quality components, attempting to put a bottom line on the relative importance of these various factors. The Ten-Year Plan housing investments appear to have played a large role in alleviating the pre-1994 drop in prices. Without the public investment, prices would have fallen 17 percentage points more than the 30 percent they actually dropped. The housing developments, however, had a smaller effect on price growth rates after 1994. In the aggregate, changes in school quality indicators are associated with modest increases in price appreciation rates for most of the years covered, whereas changes in composition of the school community consistently put downward pressure on annual growth rates. The results suggest that additional work is warranted, particularly to attempt to disentangle the effects of changes in school inputs from the effects of changes in the socioeconomic and demographic characteristics of the students.

The decompositions indicate that the fall in violent crime since 1988 has raised property values by about 8 percent, with most of this effect accruing from 1994 and later. The fall in violent crime accounted for about one-third the total real price appreciation during the 1994 to 1998 period. The sales-weighted average of the price per housing unit in New York City is $203,000, indicating that the fall in crime was valued at more than $15,000 per household. So, although the fall in crime was hardly the only factor driving property values in the late 1990s, its effect was not small.

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