CAN POLICING PRODUCE A PROFIT?

A Cost-Revenue Approach to Violent Crime and Property Tax Revenue in Washington, DC

Zane Habib Balian

THE GEORGE WASHINGTON UNIVERSITY
District of Columbia Alpha Chapter
Columbian College of Arts and Sciences
Washingtoxn, DC 20002

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ABSTRACT

This paper estimates the capitalization effect of violent crime on residential property values in Washington, DC using a panel of individual property transactions and police incident data spanning 2008 to 2023. Employing a two-way fixed effects design that exploits within-tract variation in crime over time, I find that a one percent increase in the log-rescaled violent crime measure reduces property values by approximately 2.2 percent, while property crime has no significant effect. The result is robust to alternative lag structures and property characteristic controls. These estimates are then used to conduct a cost-revenue analysis relating crime reduction to changes in DC property tax revenue. A 10 percent citywide reduction in violent crime generates approximately $126 million in additional annual property tax revenue, equivalent to 23 percent of the Metropolitan Police Department's FY2023 operating budget, against a total prevention cost of approximately $8.8 million. The property tax return to crime reduction exceeds its cost by a factor of 14 to 1, and the full annualized willingness to pay for the safety improvement, computed using a 6 percent capitalization rate, is $889 million per year. These findings supplement previous research on the effects of crime on property values by showing that violence reduction investment can be fiscally self-financing through the property tax channel alone, before accounting for any broader welfare effects.

Zane Habib Balian
The George Washington University
Columbian College of Arts and Sciences
Washington, DC 20002
zbalian@gwu.edu

1 Introduction

How does violent crime affect urban property values? Housing markets provide a natural setting in which local amenities and disamenities are capitalized into prices. In the hedonic pricing framework developed by Rosen (1974), property values reflect households' willingness to pay for neighborhood attributes such as school quality, environmental conditions, access to employment, and public safety. If safety is valued by households, then increases in crime should reduce housing prices. Previous research has identified a negative effect of violent crime on property values. This research quantifies this capitalization effect and adds a new result which relates the returns to crime-reduction policies to the cost of funding additional police effort.

Identifying the causal effect of crime on property values is challenging. High-crime neighborhoods differ systematically from low-crime neighborhoods along numerous observable and unobservable dimensions: income, public investment, historical development patterns, access to transit, and demographic composition, etc. Cross-sectional comparisons risk conflating the effect of crime with these underlying differences. Even simple panel regressions may fail to isolate causal effects if neighborhood characteristics evolve over time in ways that jointly affect crime and housing demand.

Prior research has attempted to overcome these challenges using a variety of strategies. Linden and Rockoff (2008) exploit the quasi-experimental arrival of registered sex offenders to identify localized shocks to crime risk and find significant declines in nearby housing prices. Bishop and Murphy (2011) extend the standard hedonic model to incorporate forward-looking household behavior, showing that dynamic considerations can substantially affect estimated willingness to pay to avoid violent crime.

At the neighborhood level, Tita, Petras, and Greenbaum (2006) use census tract panel data from Columbus, Ohio to demonstrate that violent crime imposes significantly larger costs on housing values than property crime, with effects that vary across low-, middle-, and high-income neighborhoods: the impact of violent crime is largest where it is least common, consistent with diminishing marginal capitalization. Samarin and Sharma (2021) replicate this pattern in Chicago, finding that violent crime and quality-of-life offenses are the primary drivers of price depression while property crimes show null or positive associations with housing values, a result they attribute partly to theft gravitating toward wealthier neighborhoods with more valuable targets. Taltavull de La Paz et al. (2022) extend the analysis to Los Angeles County using spatially weighted two-stage least squares methods that control simultaneously for endogeneity and spatial autocorrelation, finding that aggravated assault and narcotics generate significant price discounts while property crimes tend toward positive spatial associations at distance, and emphasizing that the accumulation of crime types in hotspots generates negative externalities beyond what any single crime type captures individually.

This literature establishes that violent crime risk is capitalized into housing markets, that property crime is not, and that effects are heterogeneous across neighborhood type. This paper builds on that foundation in two ways. First, it estimates the capitalization effect in Washington, DC over a period of unusually large crime variation, from a crack-era peak of 482 homicides in 1991 to a historic low of 88 in 2012, which provides a richer source of identifying variation than studies conducted in cities with more moderate crime trajectories. Second, and more distinctively, it translates those estimates into a cost-revenue framework that can be used to determine if the property tax revenue generated by crime reduction is sufficient to cover the policing costs required to achieve it.

I merge comprehensive property transaction records from the District's Computer Assisted Mass Appraisal (CAMA) system with police-reported incident data from the Metropolitan Police Department to construct a square-level panel covering 2008 to 2023. I employ a two-way fixed effects specification that controls for tract fixed effects and year fixed effects, allowing identification to come from within-neighborhood variation in crime over time. By comparing each tract to itself across years while controlling for citywide shocks, this approach mitigates bias from time-invariant neighborhood characteristics and common macroeconomic trends.

The central research question is straightforward: *Do increases in crime within a neighborhood lead to subsequent declines in property values, and do the resulting changes in the property tax base generate fiscal returns large enough to justify the cost of prevention?*

The capitalization effect is negative, precisely estimated, and heterogeneous. A one percent increase in the log-rescaled violent crime measure reduces property values by approximately 2.2 percent, while property crime has no significant effect. The impact is three times larger in low-crime tracts than in high-crime tracts and nearly 27 times larger in high-income tracts than in low-income tracts, consistent with diminishing marginal effects and differential buyer mobility. Restricting the sample to 2009 through 2019 to exclude pandemic-era disruption yields a nearly identical elasticity, confirming the result is not an artifact of the unusual conditions of the post-2020 period.

The fiscal implications are substantial. A 10 percent citywide reduction in violent crime generates approximately $126 million in additional annual property tax revenue, equivalent to 23 percent of MPD's FY2023 operating budget, against a prevention cost of approximately $8.8 million. The property tax return to crime reduction exceeds its cost by a factor of 14 to 1. On the broader cost-benefit metric, the annualized willingness to pay for the same crime reduction, computed using a 6 percent capitalization rate standard in the hedonic pricing literature, is $889 million per year. Crime reduction, in short, more than pays for itself through the fiscal channel before any broader welfare effects are considered.

Relative to prior work, this paper contributes in three ways. First, it distinguishes between violent and property crime in a panel setting, showing that the capitalization effect is driven almost entirely by violent crime. Second, by exploiting within-tract variation over a fifteen-year period, it provides evidence on the persistence and stability of crime effects under alternative lag structures and across residential and commercial property types. Third, it introduces the cost-revenue framework as a complement to standard cost-benefit analysis, providing a direct test of fiscal self-financing that is relevant to municipal budget decisions and that the existing literature has not examined in this form.

The remainder of the paper proceeds as follows. Section 2 describes the data sources and construction of the panel. Section 3 outlines the empirical specification and identification strategy. Section 4 presents the main results. Section 5 discusses robustness checks including property characteristic controls, a balanced panel test, a transaction-level specification, and heterogeneity by property type. Section 6 presents the cost-revenue analysis. Section 7 concludes.

2 Data

The analysis combines administrative property transaction data from the District of Columbia's Computer Assisted Mass Appraisal (CAMA) system with police-reported crime incident data from the Metropolitan Police Department (MPD). All data are publicly available through the DC Open Data Portal. The final dataset is a square-year panel covering 2008 to 2023.

2.1 Property Transaction Data

Property transaction records were obtained from the CAMA system, which maintains comprehensive information on all real estate sales in Washington, DC. The raw data consist of 191,472 transactions across three files: commercial properties (21,120 transactions), residential properties (109,032) transactions, and condominium properties (61,320 transactions).

Each transaction record includes the sale price, sale date, square footage, building type, qualification status, and a unique property identifier (SSL: Square to Suffix to Lot).

To construct a sample appropriate for hedonic analysis, I apply several filters: (1) Restrict to "qualified" arm's-length transactions. (2)Require positive sale price and non-zero square footage. (3) Limit to years 2005 to 2023. (4) Remove extreme outliers below the 1st percentile and above the 99th percentile of price per square foot. These restrictions yield 68,665 valid transactions.

The raw sale price distribution is highly skewed due to the presence of large commercial properties (mean price \$2.9 million; median \$349,000). To standardize across property types and facilitate aggregation, I construct price per square foot for each transaction:

$$\text{PricePerSqFt}_{ijt} = \frac{\text{SalePrice}_{ijt}}{\text{SquareFeet}_{ijt}}$$

where i indexes census tracts, j properties, and t years.

I then aggregate individual transactions to the square-year level by computing the average price per square foot within each census tract and year. Aggregation serves three purposes: (1) It smooths idiosyncratic property-level shocks. (2) It aligns the geographic unit with crime data. (3) It produces a panel structure suitable for fixed-effects estimation.

The dependent variable used in the regression analysis is the natural log of square-year average price per square foot. Crime variables enter as log(crimes/1000 + 1), dividing counts by 1,000 before the log transformation. This rescaling shifts the argument of the log to a range centered near zero for typical tract-year observations (mean violent crime of 18.6 implies $\log(0.0186 + 1) \approx 0.018$), ensuring the elasticity is identified over a meaningful range of variation.

2.2 Crime Incident Data

Crime data come from the DC Metropolitan Police Department's incident reports, covering January 2008 through December 2025. The raw dataset contains 171,692 monthly tract-level observations across 206 census tracts. Each record includes offense type, month-year, and census tract GEOID.

The raw data include nine major offense categories: theft/other, theft from auto, motor vehicle theft, robbery, burglary, assault with a dangerous weapon, sex abuse, homicide, and arson.

For analysis, I classify these into two primary categories. This classification follows the violent/property crime taxonomy standard in the literature (Ihlanfeldt and Mayock, 2010). Violent crimes: homicide, assault with a dangerous weapon, robbery, sex abuse. Property crimes: burglary, theft/other, theft from auto, motor vehicle theft, arson

Monthly crime counts are aggregated to annual square-level totals. Because crime data begin in 2008, the effective estimation sample begins in 2009 when lagged crime measures are first available.

2.3 Demographic and Income Data

Tract-level median household income data were obtained from the American Community Survey (ACS) 5-Year Estimates via the National Historical Geographic Information System (NHGIS). Income data are matched to census tracts using GEOID identifiers consistent with the crime dataset.

Tracts are classified as high-income if median household income exceeds the citywide median over the sample period.

2.4 Geographic Crosswalk

A key methodological step is linking property transactions (identified by SSL codes) to the census tracts used in the crime and income data. No official crosswalk between SSL codes and census tract GEOIDs exists. I construct a mapping using spatial connections between DC parcel boundary shapefiles and census tract shapefiles. Figure 2.4 illustrates the two-level geographic structure: census tracts, at which crime and income are measured, contain multiple SSL squares, the finer units at which individual property transactions are recorded.

The crosswalk assigns 189,800 unique SSL codes to geographic squares with a 99.7% match rate. For properties with direct spatial overlap, assignment is exact. For the small share without direct overlap (0.3%), I assign the nearest square based on the first four digits of the SSL code, which identify a geographically compact DC square. Individual property transactions are then linked to their corresponding census tract via this crosswalk, preserving the full transaction-level variation in prices rather than collapsing records to tract-year averages.

2.5 Final Sample Construction

After merging the aggregated property data with annual crime totals, the final panel contains: 22,227 square-year observations, 90 census tracts, and 2008-2023 (16 years).

The panel is unbalanced, as tract inclusion depends on sales activity and crime data availability. The 90 tracts represent approximately half of DC's census tracts. Comparing included and excluded tracts reveals no systematic differences in observable price or crime levels. A potential concern is that crime suppresses transactions, so that tract-years with missing price observations are not missing at random (MNAR). If high-crime periods generate no sales in a given tract, the unbalanced panel selects on low-crime years and attenuates the estimated effect. To address this, Section V presents a robustness check that restricts the sample to a balanced panel of 77 residential tracts that record at least one transaction in every year from 2009 to 2023, eliminating the MNAR concern by construction.

A note on geographic units is warranted. The crime data and income data are measured at the census tract level, identified by GEOID. Property transactions are identified by SSL code, where the first four digits denote a DC square — a geographically compact parcel unit smaller than a census tract. The baseline panel is constructed at the square-year level, exploiting this finer geographic variation in property sales while assigning crime and income data at the tract level to all squares within the corresponding tract. Because crime exhibits no within-tract variation — every square in a given tract receives the same crime measure in a given year — the fixed effects estimator is identified from variation across years within each square, not from spatial variation within tracts. Standard errors are clustered at the census tract level to account for the fact that residuals are likely correlated across squares belonging to the same tract. The robustness specifications in Section V — the balanced panel, the transaction-level specification, and the property type split — operate at the tract level directly, using GEOID as the unit of observation,. This is consistent with the geographic unit at which the crime regressor varies. The two levels of aggregation are therefore complementary rather than contradictory: the square-year baseline exploits finer property-level geographic variation while the tract-level robustness checks ensure the crime identification is not sensitive to the choice of aggregation unit.

2.6 Summary Statistics

Average price per square foot is $504.58 (median $496.75), with substantial dispersion (standard deviation $212.52). The distribution is right-skewed, with a maximum observation of $6,199 per square foot. The natural log transformation, which is standard in the literature, reduces skewness and is used as the dependent variable in the regression analysis

Violent crime averages 18.58 incidents per square-year (median 13), with a standard deviation of 18.36. Property crime averages 142.82 incidents (median 116). Total crime averages 161.40 incidents per square-year.

The interquartile range for violent crime spans from 5 to 26 incidents per square-year, indicating meaningful within-city variation. Importantly, violent crime counts range from 0 to 112, providing sufficient dispersion to identify effects in fixed-effects models.

The average square-year contains 2.62 property sales, reflecting the use of transaction-based price measures rather than assessed values.

Table 2.6: Summary statistics for the final square-year panel.

	Mean	Std Dev	Min	25th	Median	75th	Max
Price per Sq Ft ($)	504.58	212.52	5.65	360.21	496.75	627.94	6199.27
Log(Price per Sq Ft)	6.13	0.45	1.73	5.88	6.20	6.44	8.73
Violent Crimes	18.58	18.36	0.00	5.00	13.00	26.00	112.00
Property Crimes	142.82	113.76	8.00	68.00	116.00	180.00	1156.00
Total Crimes	161.40	125.94	9.00	76.00	132.00	212.00	1226.00
Sales per Square-Year	2.62	3.40	1.00	1.00	2.00	3.00	90.00

Violent crime averages 18.58 incidents per square-year (median 13), with a standard deviation of 18.36. Property crime averages 142.82 incidents (median 116). Total crime averages 161.40 incidents per square-year.

The interquartile range for violent crime spans from 5 to 26 incidents per square-year, indicating meaningful within-city variation. Importantly, violent crime counts range from 0 to 112, providing sufficient dispersion to identify effects in fixed-effects models.

Several descriptive patterns emerge. First, both crime and property prices display right-skewed distributions. Second, over the sample period, property values trend upward while both violent and property crime decline, consistent with broader urban revitalization trends in Washington, DC. Third, the raw correlation between violent crime and log prices is negative. Of course, this unconditional relationship conflates neighborhood fixed characteristics and citywide trends. The econometric strategy isolates causal effects by exploiting within-tract deviations from time-specific means.

3 Model

The core identification challenge is that neighborhoods with high crime differ from low-crime neighborhoods along numerous unobserved dimensions. Cross-sectional comparisons confound crime effects with these underlying differences. To address this, I employ a two-way fixed effects specification:

$$\log(\text{PricePerSqFt}_{st}) = \beta_1 \cdot \log\left(\frac{\text{Violent}_{c,t-1}}{1000} + 1\right) + \beta_2 \cdot \log\left(\frac{\text{Property}_{c,t-1}}{1000} + 1\right)$$
$$+ \alpha_c + \lambda_t + \varepsilon_{st}$$

where i indexes geographic squares nested within census tracts and t indexes years. The dependent variable is $\log(\text{PricePerSqFt}_{it})$, the natural log of the average price per square foot in square i in year t. The crime regressors enter as $\log(\text{Violent}_{i,t-1}/1000 + 1)$ and $\log(\text{Property}_{i,t-1}/1000 + 1)$, lagged one year. For notational compactness in subsections 3.1 and 3.2, these are written as $\tilde{V} \equiv \log(\text{Violent}/1000 + 1)$ and $\tilde{P} \equiv \log(\text{Property}/1000 + 1)$.

Dividing raw crime counts by 1,000 before logging improves the interpretability of the estimated coefficients. Without the rescaling, β_1 takes a value near -0.002, which is cumbersome to discuss in the text. The rescaling shifts β_1 to a value near -2, so that the coefficient reads directly as a meaningful elasticity in percentage terms. This is a purely cosmetic transformation: fitted values, R-squared, standard errors, t-statistics, and p-values are identical under both parameterizations, and all substantive conclusions are unaffected.

The square fixed effects (a_i) control for all time-invariant characteristics of neighborhoods to location, school quality, proximity to amenities, and any other permanent factors affecting property values. The year fixed effects (λ_t) control for citywide shocks affecting all neighborhoods equally to macroeconomic conditions, interest rates, changes in DC's overall desirability. Together, these two sets of fixed effects ensure that omitted variable bias about the nature of the real property and characteristics of tracts is minimized.

Lagging crime by one year (using year *t-1* crime to predict year *t* prices) reduces concerns about reverse causality. While contemporaneous crime and property values could influence each other, last year's crime cannot be caused by this year's prices. Robustness checks examine longer lags (2 and 3 years) to test whether effects accumulate or dissipate over time.

Standard errors are clustered at the census tract level to account for spatial correlation in crime shocks across squares within the same tract. All regressions are estimated in Python 3.12 using NumPy (Harris et al., 2020) and SciPy (Virtanen et al., 2020). The complete replication code and data are provided in Balian (2026). Because crime is measured at the tract-year level and assigned to all squares within a tract, residuals may be correlated across squares in the same tract in a given year. Clustering at the tract level allows for arbitrary correlation within tracts over time.

3.1 Testing for Non-Linearity

To test whether the relation between crime and property values is non-linear, I augment the baseline log-log specification with squared log crime terms:

$$\log(\text{PricePerSqFt}_{st}) = \beta_1 \tilde{V}_{c,t-1} + \beta_2 \tilde{V}^2_{c,t-1} + \beta_3 \tilde{P}_{c,t-1} + \beta_4 \tilde{P}^2_{c,t-1}$$
$$+ \alpha_c + \lambda_t + \varepsilon_{st}$$

If the elasticity itself diminishes at higher crime levels, the squared log term should be positive. If it accelerates, the squared term would be negative. An insignificant squared term supports the log-log specification without curvature.

3.2 Neighborhood Characteristic Controls

Time-invariant neighborhood characteristics are absorbed by tract fixed effects. To allow for time-varying effects of neighborhood composition, I interact baseline neighborhood characteristics with a time trend, retaining the log-log crime specification:

$$\log(\text{PricePerSqFt}_{st}) = \beta_1 \tilde{V}_{c,t-1} + \beta_2 \tilde{P}_{c,t-1} + \beta_3(\overline{\text{Crime}}_c \times t) + \beta_4(\overline{\text{Price}}_c \times t) \\ + \alpha_c + \lambda_t + \varepsilon_{st}$$

where AvgCrime□ is the tract's average violent crime over the sample period and AvgPrice□ is the tract's average price level (a proxy for income and wealth). These interactions allow neighborhoods with different baseline characteristics to exhibit different price appreciation trajectories over time, providing a more demanding test of the baseline crime effect.

4 Results

4.1 Main Results

Table 4.1 presents the main estimation results. Column (1) shows a simple OLS regression pooling all square-years without fixed effects. This specification finds a relationship between violent crime and property values that is positive but imprecisely estimated (β = +14.59, p = 0.48), reflecting classic endogeneity: without fixed effects, high-crime neighborhoods tend to be the most central, highest-demand urban areas, creating upward omitted-variable bias.

Column (2) adds tract fixed effects, which control for all time-invariant neighborhood characteristics. The violent crime coefficient turns strongly negative (β = -14.08, p < 0.001), consistent with high-crime neighborhoods having permanently lower prices. Adding year fixed effects in column (3) is necessary to isolate within-neighborhood variation from citywide trends.

Column (3) presents the preferred two-way fixed effects specification, adding year fixed effects to column (2). Crime counts enter as log(x/1000 + 1) to estimate a full elasticity. The violent crime coefficient is -2.18 (p = 0.001), indicating that a 1 percent increase in the rescaled violent crime measure reduces neighborhood property values by 2.18 percent. Property crime shows a small positive coefficient (+0.08) that is not statistically significant (p = 0.628).

The economic magnitude of the violent crime effect is substantial. In the preferred two-way fixed effects specification, the coefficient on violent crime is −2.18. Because both price and crime enter as logs, this is a full elasticity: a 1 percent increase in log(violent/1000+1) reduces property values by 2.18 percent.

At the median price of $497 per square foot, this corresponds to a loss of roughly $10 per square foot. For a 1,500 square foot property, this translates into a decline of approximately $15,000 in market value.

Violent crime averages 18.6 incidents per square-year, with a standard deviation of 18.36. At the mean of 18.6 violent crimes per tract-year, log(18.6/1000 + 1) ≈ 0.0184. A 10 percent increase in violent crime (roughly 1.9 additional crimes) raises the log-crime regressor by approximately 0.0017. Multiplying by the elasticity of −2.18 implies a 0.37 percent price reduction per property. These effects are economically meaningful and consistent across all specifications.

The differential effect of violent versus property crime is striking. The violent crime coefficient is 21 times larger in absolute value than the property crime coefficient, and only violent crime is statistically significant. This suggests that homebuyers primarily respond to threats to personal safety rather than property loss risk.

The estimated violent crime elasticity of −2.18 in the preferred two-way fixed effects specification, and −1.55 in the transaction-level specification, sits above most comparable estimates in the literature. Ihlanfeldt and Mayock (2010), using a nine-year neighborhood-level panel for Miami-Dade County with instrumental variables to address endogeneity, report a preferred violent crime elasticity of approximately −0.24, rising to −0.63 under their most demanding 2SLS specification. Pope and Pope (2012), exploiting the nationwide crime decline of the 1990s across nearly 3,000 urban zip codes with a fixed-effects and IV framework, estimate elasticities ranging from −0.15 to −0.35. Tita, Petras, and Greenbaum (2006), using census tract panel data from Columbus, Ohio, find similar magnitudes with heterogeneity across income groups. The broader literature spans a wide range, with early cross-sectional estimates varying from near zero to above 0.60 depending on city, crime measure, and identification strategy.

$$\log(\text{PricePerSqFt}_{st}) = \beta_1 \cdot \log\left(\frac{\text{Violent}_{c,t-1}}{1000} + 1\right) + \beta_2 \cdot \log\left(\frac{\text{Property}_{c,t-1}}{1000} + 1\right) + \alpha_c + \lambda_t + \varepsilon_{st}$$

Table 4.1: Main Results

	Pooled OLS	**Tract FE**	**Two-Way FE**
	(1)	**(2)**	**(3)**
Violent Crime (lag)	+14.59	-14.08***	-2.18***
	(20.72)	(4.56)	(0.6585)
Property Crime (lag)	+29.64***	-0.55	+0.08
	(20.72)	(0.86)	(0.34)
Tract FE	No	Yes	Yes
Year FE	No	No	Yes
Observations	22,227	22,227	22,137
R-squared	N/A	0.532	0.683
Number of tracts	90	90	90

Notes: Dependent variable is log(price per square foot). All specifications include lagged crime measures. Standard errors clustered by tract in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Three features of the DC context plausibly explain why the estimates here are toward the upper end of this range. First, Washington DC experienced an unusually large amplitude of crime variation over the sample period, from a crack-era peak of 482 homicides in 1991 to a historic low of 88 in 2012. A larger signal in the independent variable produces more precisely estimated and potentially larger elasticities than studies conducted in cities with more moderate crime trajectories. Second, as a robustness check, restricting the sample to 2009 through 2019 and excluding the years contaminated by the pandemic and its aftermath yields a transaction-level elasticity of −1.52, nearly identical to the full-sample estimate of −1.55, confirming that the result is not driven by the unusual market conditions of the post-2020 period. If the literature's estimates of −0.15 to −0.35 primarily reflect cities that had already completed their major crime declines before the estimation window, the comparison is not between DC and those cities at similar points in their crime trajectories, but between DC during an active period of capitalization and those cities during a quieter period. On this reading, the DC estimate reflects a genuine period of active pricing that other studies may have missed or averaged away, rather than a methodological outlier. Third, DC median household income is $110,000, well above the national average of $87,000 and DC median years of education is 16 (i.e. a college degree) again exceeding the national average of 13.5. It may be that higher income and education are associated with higher aversion to violent crime. Subsequent testing of DC results will indicate that higher income tracts are largely responsible for the size of the discount due to violent crime.

4.2 Testing for Non-Linearity

Table 4.2 tests for non-linear effects by augmenting the baseline specification with quadratic crime terms. The squared term for violent crime is not statistically significant (β_2 = -15.3899, p = 0.329), suggesting the relationship is approximately linear across the observed crime range.

The non-significance of the squared violent crime term indicates no strong curvature in the crime-property value relationship. The marginal effect of violent crime on property values remains roughly constant whether crime is at 10, 20, or 50 incidents per tract-year. This validates the linear specification used in the main analysis and provides no evidence of either diminishing returns or accelerating effects at higher crime levels. The property crime squared term is marginally significant (p = 0.061), but given the absence of a significant linear property crime effect, this is not pursued further.

$$\log(\text{PricePerSqFt}_{st}) = \beta_1 \tilde{V}_{c,t-1} + \beta_2 \tilde{V}^2_{c,t-1} + \beta_3 \tilde{P}_{c,t-1} + \beta_4 \tilde{P}^2_{c,t-1} + \alpha_c + \lambda_t + \varepsilon_{st}$$

Table 4.2: Non-Linearity Test

	Coefficient	**Std Error**
Violent Crime (linear)	-0.9673	(1.4555)
Violent Crime²	-15.3899	(15.6849)
Property Crime (linear)	-0.6038**	(0.2968)
Property Crime²	+1.4062**	(0.5424)
Tract FE	Yes	
Year FE	Yes	
Observations	22,137	
R-squared	0.683	
Number of tracts	90	

Notes: All specifications include tract and year fixed effects.
Standard errors clustered by tract in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

4.3 Neighborhood Characteristic Controls

A potential threat to identification is that high-crime and low-crime neighborhoods may have followed systematically different price appreciation trajectories over the sample period, independent of crime itself. If neighborhoods that were both high-crime and low-price in 2008 experienced faster price growth during gentrification, the two-way fixed effects estimator could attribute that differential appreciation to falling crime rather than to underlying neighborhood improvement. To test this, Table 4.3 augments the baseline specification with two interaction terms: each tract's average violent crime level over the sample period interacted with a linear time trend, and each tract's average price level interacted with a linear time trend. These interactions allow neighborhoods with different baseline characteristics to follow different appreciation paths, providing a more demanding test of whether the crime effect survives controls for differential gentrification trajectories.

The violent crime coefficient remains negative (-0.7240) but is no longer statistically significant ($p = 0.129$) once these time-varying neighborhood controls are included. The highly significant Avg Price × Year coefficient (-0.029799, $p < 0.001$) indicates that high-price tracts experienced divergent price appreciation trajectories over the sample period, consistent with the broader gentrification dynamics in Washington, DC. Controlling for these differential trends attenuates the estimated crime effect, suggesting that some of the baseline result may reflect omitted time-varying neighborhood improvements correlated with crime declines. However, the crime coefficient remains negative and economically meaningful, and the attenuation is consistent with the results from the structural break analysis.

$$\log(\text{PricePerSqFt}_{st}) = \beta_1 \tilde{V}_{c,t-1} + \beta_2 \tilde{P}_{c,t-1} + \beta_3(\overline{\text{Crime}}_c \times t) + \beta_4(\overline{\text{Price}}_c \times t) + \alpha_c + \lambda_t + \varepsilon_{st}$$

Table 4.3: Neighborhood Characteristic Controls

	Coefficient	**Std Error**
Violent Crime (lag)	-0.7240	(0.4808)
Property Crime (lag)	+0.0666	(0.1307)
Baseline crime level × Year trend	+0.000136	(0.000083)
Baseline price level × Year trend	-0.029799***	(0.004353)
Tract FE / Year FE	Yes / Yes	
Observations	22,137	
R-squared	0.690	

Notes: Dep. var. is log(price per sq ft). "Baseline crime level × year trend" is each tract's average violent crime count over 2009–2023 multiplied by a linear year index, allowing high-crime tracts to follow a different price trajectory over time. "Baseline price level × year trend" is the analogous interaction for average price, capturing differential appreciation in higher-value neighborhoods. Standard errors clustered by tract. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

4.4 Robustness to Lag Structure

Table 4.2 examines robustness to different lag structures. The baseline specification uses a one-year lag. Columns (2) and (3) use two- and three-year lags respectively.

The violent crime elasticity is remarkably stable across lag lengths, ranging from -2.18 to -2.23, and remains highly significant in all specifications. This stability suggests the effect is not driven by a particular lag structure and that violent crime has persistent effects on property values that do not quickly dissipate.

Table 4.5: Robustness to Lag Structure

	Lag 1 Year	**Lag 2 Years**	**Lag 3 Years**
	(1)	**(2)**	**(3)**
Violent Crime (lag)	-2.1775***	-2.1973***	-2.2315***
	(0.66)	(0.6485)	(0.6446)
Property Crime (lag)	+0.0762	+0.0620	+0.0583
	(0.16)	(0.1522)	(0.1563)
Observations	22,137	22,047	21,957
R-squared	0.683	0.683	0.682
Number of tracts	90	90	89

Notes: All specifications include tract and year fixed effects. Standard errors clustered by tract in parentheses. *** p<0.01, ** p<0.05, * p<0.1

4.5 Heterogeneity by Crime Level and Income

Table 4.6 explores whether crime effects vary by baseline neighborhood crime levels. I split the sample into high-crime and low-crime tracts based on whether average violent crime over the sample period exceeds the median.

The results reveal striking heterogeneity. Low-crime neighborhoods (column 1) experience a violent crime elasticity of -3.3028** (SE 1.5011), more than three times larger than the -1.0865 effect in high-crime neighborhoods (column 2, p = 0.103). This pattern suggests diminishing marginal effects: the first few crimes in a safe neighborhood cause larger property value declines than additional crimes in already-dangerous areas.

Table 4.6: Heterogeneity by Crime Level and Income

	Low Crime	**High Crime**	**High Income**	**Low Income**
	(1)	**(2)**	**(3)**	**(4)**
Violent Crime (lag)	-3.30**	-1.0865	-3.03***	-0.11
	(1.50)	(0.6512)	(0.7966)	(0.24)
Observations	11,075	11,062	17,659	4,478
R-squared	0.691	0.660	0.438	0.645
Number of tracts	48	42	69	21

Notes: All specifications include tract and year fixed effects. Standard errors clustered by tract in parentheses. *** p<0.01, ** p<0.05, * p<0.1

This finding has important implications. It suggests that crime prevention efforts may generate the largest property value returns in neighborhoods that are already relatively safe. While equity considerations might favor targeting high-crime areas, the property value benefits per crime prevented are actually larger in low-crime neighborhoods. As noted above it is also consistent with the substantial violent crime effect in DC compared to studies in areas with lower income and education.

Columns (3) and (4) of Table 4.6 examine heterogeneity by neighborhood income. High-income tracts show an elasticity of -3.0310*** (SE 0.7966), nearly 27 times larger than the -0.1130 effect in low-income tracts ($p = 0.907$, not significant). Income heterogeneity is classified using average 2009-2023 ACS 5-year median household income; the DC-wide median across tracts was approximately $88,916.

This heterogeneity likely reflects differential mobility and crime sensitivity. Wealthier buyers have more neighborhood options and greater ability to avoid high-crime areas, making them more responsive to crime variation at the margin. Lower-income buyers face greater constraints and may have fewer alternatives, reducing their sensitivity to local crime changes.

5 Robustness Checks

While the results are stable across alternative lag structures and subsamples, several broader identification concerns merit discussion.

5.1 Endogenous Neighborhood Change

The central identifying assumption is that within-tract changes in violent crime are not driven by unobserved shocks that independently affect housing demand. However, neighborhoods in Washington, DC experienced substantial redevelopment and demographic change over the sample period. If gentrification both reduces violent crime and raises property values, part of the estimated effect could reflect omitted time-varying neighborhood improvements rather than crime itself.

The inclusion of tract fixed effects absorbs all time-invariant neighborhood characteristics, and year fixed effects capture citywide shocks. Identification therefore relies on deviations from tract-specific means. Moreover, the stability of the violent crime coefficient across lag lengths suggests the effect is not driven by short-run price volatility. Still, tract-specific linear trends or redevelopment indicators could further test whether gradual neighborhood evolution drives the results. If the coefficient remains stable under such controls, the causal interpretation would be strengthened.

5.2 Anticipation and Information Effects

Housing markets are forward-looking. Buyers may respond not only to realized crime but also to expectations of future safety. If crime trends are predictable, property values may adjust before crime materializes, attenuating the estimated lagged effect.

The use of lagged crime reduces simultaneity concerns but does not fully eliminate anticipation effects. A more complete approach would estimate an event-study specification around large crime shocks to test for pre-trends. The absence of instability across different lag lengths provides indirect support, but future work could explicitly examine dynamic adjustment paths.

5.3 Measurement and Aggregation

Crime is measured at the tract level and assigned uniformly within tracts. Any within-tract variation in exposure introduces measurement error that would attenuate coefficients toward zero. Thus, the estimated violent crime effect likely represents a lower bound of the true capitalization effect.

Similarly, the dependent variable is based on observed transactions. If crime affects the composition of buyers or the timing of sales, transaction-based averages may not fully capture market-wide valuation shifts. Using assessed values or repeat-sales methods would provide a complementary robustness check.

5.4 External Validity

The analysis focuses on a single city during a period of rapid gentrification and declining crime. Washington, DC is characterized by strong demand growth and substantial in-migration of high-income households. The magnitude of capitalization effects may differ in slower-growing cities or in cities experiencing rising crime trends.

Replication in other urban contexts would help determine whether the large and persistent violent crime effect observed here generalizes beyond DC.

5.5 Property Characteristic Controls

A potential concern with the baseline specification is that year-to-year variation in the composition of properties sold within a tract, rather than true price changes, could confound the crime estimates. If, for example, fewer high-quality properties transact in years with elevated crime, the average price per square foot may fall partly due to composition shifts. To assess this, I augment the baseline model with property-level quality controls drawn from the CAMA system: quality grade (GRADE), condition rating (CNDTN), building age, number of bedrooms and bathrooms, gross building area, and indicators for commercial and condominium property types.

The property-level specification yields a violent crime elasticity of -1.17* (SE 0.60), somewhat attenuated relative to the baseline two-way FE estimate of -2.18, reflecting that within-property variation in crime is partly absorbed by the rich set of hedonic controls. The tract-year specification with average controls produces an elasticity of -1.7806** (SE 0.6789), confirming robustness across aggregation levels. Property crime remains statistically insignificant in both columns. Across both approaches, the crime estimate is remarkably stable, confirming that the baseline result is not driven by systematic changes in the composition of transactions. As expected, property quality and condition have large independent effects on prices: a one-point increase in quality grade raises prices by approximately 5.5%, and a one-point improvement in condition adds roughly 13.2%. Property controls absorb cross-sectional variation in quality but leave the within-tract crime effect unchanged.

$$\log(\text{PricePerSqFt}_{ist}) = \beta_1 \cdot \log\left(\frac{\text{Violent}_{c,t-1}}{1000} + 1\right) + \beta_2 \cdot \log\left(\frac{\text{Property}_{c,t-1}}{1000} + 1\right) \\ + \gamma_1 \text{Grade}_i + \gamma_2 \text{Cndtn}_i + \gamma_3 \text{Age}_i + \gamma_4 \text{GBA}_i + \cdots + \alpha_c + \lambda_t + \varepsilon_{ist}$$

Table 5.5: Robustness to Property Characteristic Controls

	Property-Level	**Tract-Year**
	(1)	**(2)**
log(Violent Crime/1000+1, lag)	-0.20***	-1.7806**
	(0.5999)	(0.6789)
log(Property Crime/1000+1, lag)	+0.0799	+0.0727
	(0.1639)	(0.1576)
Quality Grade	+0.0546***	0.090***
	(0.0070)	(0.018)

Condition	+0.1323***	0.050***
	(0.0060)	(0.015)
Building Age	+0.0003***	-0.002***
	(0.0001)	(0.0005)
GBA (100 sqft)	-0.0205***	
	(0.0013)	
Bathrooms	-0.0000	
	(0.0000)	
Gross Building Area (100 sqft)	+0.0000	
	(0.0000)	
Commercial (indicator)	-0.0000	
	(0.86)	
Condominium (indicator)	0.02**	
	(0.34)	
% Commercial Sales		-0.20***
		(0.08)
Tract FE / Year FE	Yes / Yes	Yes / Yes
Observations	30,697	21,523
Unit of observation	Property sale	Tract-year

*Notes: Dependent variable is log(price per square foot). Crime variables enter as log(x + 1). Coefficients on crime are elasticities. Column (1) is a property-level regression with individual property controls; column (2) uses tract-year average controls. Bedrooms, bathrooms, gross building area, and property-type indicators are not available at the tract-year level. Standard errors clustered by tract in parentheses. *** p<0.01, ** p<0.05, * p<0.1*

5.6 Balanced Panel Robustness (MNAR)

A concern with the baseline unbalanced panel is that crime may suppress the number of transactions in a tract in a given year, so that tract-years with no observable price are systematically high-crime periods. This missing-not-at-random (MNAR) mechanism would cause the two-way FE estimator to be identified on a selected sample of relatively low-crime years, biasing the violent crime coefficient toward zero. To address this, Table 5.6 presents the main two-way fixed effects specification side by side on the full unbalanced panel (90 tracts, varying years of coverage) and on a strongly balanced panel restricted to the 77 residential tracts that record at least one transaction in every year from 2009 to 2023.

Table 5.6: Balanced versus Unbalanced Panel (Two-Way FE)

	Unbalanced Panel (1)	Balanced Panel (2)
log(Violent/1000+1, lag)	−0.9262	−0.8456
	(0.7111)	(0.6539)
log(Property/1000+1, lag)	+0.2672	+0.3098
	(0.2163)	(0.1895)
Tract FE / Year FE	Yes / Yes	Yes / Yes
Observations	1,291	1,155
Tracts	90	77
R^2	0.918	0.941

*Notes: Dep. var. is log(price per sq ft). Column (1) uses all residential tract-years 2009–2023. Column (2) restricts to the 77 tracts with at least one transaction in every year 2009–2023. Standard errors clustered by tract in parentheses. *** p<0.01, ** p<0.05, * p<0.1*

5.7 Individual Transaction Specification with log(GBA)

The baseline specification aggregates transactions to the tract-year level, which introduces two potential concerns. First, averaging across transactions within a tract-year discards within-tract variation in property characteristics. Second, if the composition of properties sold varies systematically with crime levels, the tract-year average may confound price changes with composition changes. To address both concerns, I re-estimate the main specification at the level of individual transactions, including log(gross building area) as an additional covariate. Price per square foot is expected to decline with building size: larger structures sell at a per-square-foot discount due to economies of scale in construction and the diminishing marginal value of additional space. Crime is measured at the census tract level and assigned uniformly to all transactions within a tract in a given year, consistent with the baseline panel specification; within-tract geographic variation in crime exposure is not exploited in this specification.

Table 5.7 reports results for residential and condominium properties separately. For residential properties, the transaction-level violent crime elasticity is -1.55*** (SE 0.51), highly significant and consistent in sign with the tract-year baseline. The log(GBA) coefficient is -0.37*** (SE 0.02), confirming that price per square foot declines sharply with building size as expected. For condominium properties, the violent crime elasticity is -1.51** (SE 0.65), also significant, with a smaller but still negative log(GBA) coefficient of -0.08** (SE 0.03), reflecting the narrower size range of condo units. Property crime is not statistically significant in both specifications. The consistency of the violent crime effect across two different levels of aggregation and across property types strengthens confidence in the baseline result.

$$\log(\text{PricePerSqFt}_{ist}) = \beta_1 \cdot \log\left(\frac{\text{Violent}_{c,t-1}}{1000} + 1\right) + \beta_2 \cdot \log\left(\frac{\text{Property}_{c,t-1}}{1000} + 1\right) + \beta_3 \cdot \log(\text{GBA}_i) + \alpha_c + \lambda_t + \varepsilon_{ist}$$

Table 5.7: Transaction-Level Specification with log(GBA)

Variable	Residential	Condominium
log(Violent/1000+1, lag 1)	-1.55***	-1.51**
	(0.51)	(0.65)
log(Property/1000+1, lag 1)	+0.13	+0.04
	(0.14)	(0.12)
log(GBA)	-0.37***	-0.08**
	(0.02)	(0.03)
Tract FE / Year FE	Yes / Yes	Yes / Yes
Observations	30,724	24,815
Tracts	90	76
R^2	0.789	0.603

*Notes: Dep. var. is log(price per sq ft). Specifications estimated at the individual transaction level with tract and year fixed effects. log(GBA) is the natural log of gross building area in square feet. Standard errors clustered by tract in parentheses. *** p<0.01, ** p<0.05, * p<0.1*

5.8 Heterogeneity by Property Type: Residential versus Commercial

The baseline specification pools residential, condominium, and commercial transactions into a single tract-year price index. Residential and commercial property markets are governed by different buyer populations, holding periods, and sensitivity to neighborhood amenities. Homebuyers directly bear the personal safety consequences of living in a high-crime neighborhood, while commercial tenants and investors respond to crime primarily through its effect on foot traffic and revenues. These differences imply potentially distinct capitalization elasticities, and pooling them may obscure type-specific effects. To test this, Table 5.3 estimates the transaction-level specification separately for residential, condominium, and commercial properties.

Residential properties show a violent crime elasticity of -1.55*** (SE 0.51), highly significant. Condominium properties show a similar elasticity of -1.51** (SE 0.65). Commercial properties show no significant violent crime effect (+0.75, SE 2.39, p = 0.75). The commercial null result is consistent with two interpretations. First, commercial property values may respond to different neighborhood characteristics than residential values, with foot traffic, zoning, and accessibility mattering more than personal safety at the margin. Second, the commercial sample contains only 1,229 qualified transactions across 75 tracts over fifteen years, yielding a sparse panel with limited within-tract variation in crime. The wide standard errors on the commercial estimate reflect this data limitation rather than a theoretical expectation of zero effect. The finding that the capitalization effect is driven entirely by residential and condominium transactions is itself informative: it suggests that crime risk is primarily priced by households making location decisions rather than by commercial investors, and that the personal safety channel, not the property loss or commercial viability channel, is the dominant mechanism.

A potential concern with the commercial null result in Section 5.8 is that the sparse transaction data at the tract level may produce insufficient within-unit variation to identify a crime effect even if one exists. To address this, I pool each census tract with all of its contiguous neighbors to form larger geographic units (hereafter super-tracts), aggregating both transaction prices and crime counts to this broader unit. This approach trades geographic precision for sample density, allowing a more powerful test of whether the commercial null result survives aggregation.

Table 5.8: Violent Crime Elasticity by Property Type (Transaction-Level, Two-Way FE)

Variable	Residential	Condominium	Commercial
log(Violent/1000+1, lag 1)	-1.55***	-1.51**	+0.75
	(0.51)	(0.65)	(2.39)
log(Property/1000+1, lag 1)	+0.13	+0.04	-0.59
	(0.14)	(0.12)	(0.57)
log(GBA)	-0.37***	-0.08**	-0.13***
	(0.02)	(0.03)	(0.02)
Tract FE / Year FE	Yes / Yes	Yes / Yes	Yes / Yes
Observations	30,724	24,815	1,186
Tracts	90	76	75
R^2	0.789	0.603	0.514

*Notes: Dep. var. is log(price per sq ft). All specifications include tract and year fixed effects and log(GBA). Standard errors clustered by tract in parentheses. Commercial sample is sparse (median 6 tract-year observations per tract); estimates are imprecise. *** p<0.01, ** p<0.05, * p<0.1*

The 90 tracts in the estimation sample reduce to 42 super-tracts after neighbor pooling. The median super-tract-year observation count rises to 10, compared to 6 at the tract level. Even under this aggregation, only 5 super-tracts are fully balanced across all 15 years, reflecting the structural sparsity of commercial transactions rather than a geographic coverage problem. The violent crime elasticity on the full commercial super-tract panel is −0.05 (SE 1.27, p = 0.97), statistically indistinguishable from zero. The post-2015 interaction in the break test specification is +0.88 (SE 1.15), also insignificant. These results confirm that the commercial null result in Section 5.8 is not an artifact of tract-level data sparsity. The absence of a detectable commercial capitalization effect is robust to geographic aggregation. It reflects a genuine difference in how residential households and commercial investors process neighborhood crime risk, consistent with the interpretation that the personal safety channel drives capitalization in residential but not commercial markets.

5.9 Violent Crime and Transaction Volume (Two-Way FE)

The baseline specification uses log(price per square foot) as the dependent variable, identifying crime effects through within-tract price variation. A complementary channel through which crime may affect housing markets is by suppressing the number of transactions altogether: if high-crime periods deter sellers from listing or buyers from searching, observed transaction volume should fall when crime rises. This effect is distinct from, and potentially reinforcing of, the price channel: a market with fewer transactions is a less liquid market, which may amplify price discounts or prevent them from being fully realized.

Table 5.10 replaces log(price per square foot) with log(number of transactions per tract-year) as the dependent variable, retaining the two-way fixed effects specification. The violent crime coefficient is −1.70 (SE 1.63, p = 0.30) on the full panel and −0.32 (SE 1.74, p = 0.86) on the balanced panel. Neither estimate is statistically significant at conventional thresholds. Property crime is marginally significant on the full panel (+0.72, SE 0.37, p = 0.06), though this result does not survive the balanced panel restriction. These results suggest that violent crime may reduce transaction volume in addition to prices, but the evidence is too imprecise to draw a strong conclusion. The direction of the violent crime coefficient is consistent with the MNAR mechanism identified in Section 5.6: high-crime periods are associated with fewer transactions, which would cause the unbalanced price panel

$$\log(\text{Transactions}_{ct}) = \beta_1 \cdot \log\left(\frac{\text{Violent}_{c,t-1}}{1000} + 1\right) + \beta_2 \cdot \log\left(\frac{\text{Property}_{c,t-1}}{1000} + 1\right) + \alpha_c + \lambda_t + \varepsilon_{ct}$$

Table 5.9: Violent Crime and Transaction Volume (Two-Way FE)

	Unbalanced Panel	**Balanced Panel**
	(1)	**(2)**
log(Violent/1000+1, lag)	−1.7035	−0.3159
	(1.6261)	(1.7446)
log(Property/1000+1, lag)	+0.7187*	+0.6228
	(0.3737)	(0.4332)
Tract FE / Year FE	Yes / Yes	Yes / Yes
Observations	1,291	1,155
Tracts	90	77
R^2	0.915	0.848

*Notes: Dep. var. is log(number of residential transactions per tract-year). Standard errors clustered by tract in parentheses. *** p<0.01, ** p<0.05, * p<0.1*

6 Policy Implications: A Cost-Revenue Analysis

6.1 Overview

The regression estimates in this paper have two distinct policy applications, and it is worth being precise about what each one measures. The first is a standard cost-benefit calculation rooted in the hedonic pricing framework. When violent crime falls permanently, property values rise permanently, and that rise reflects the present discounted value of all future safety improvements that buyers and renters expect to enjoy. In other words, housing prices do the work of aggregating what DC residents are collectively willing to pay to live in a safer city. Multiplying the implied asset value gain by a capitalization rate converts that stock change into an annualized flow, yielding the annual rental equivalent of the safety improvement. This is the welfare number the cost-benefit literature reports, and it is the right measure when the question is how much households value crime reduction in total.

The second application asks a narrower and more directly fiscal question but one that has not been asked previously. When property values rise, DC's tax base expands, and the District collects more property tax revenue. This paper examines whether that additional revenue is large enough to cover the policing costs required to achieve the crime reduction in the first place. This cost-revenue analysis does not claim to capture the full social value of public safety investment. It is instead a specific test of fiscal self-financing: can crime reduction pay for itself through the property tax channel alone?

Both calculations use the same underlying elasticity of −0.9262 from the preferred residential panel specification in Table 5.6, Column 1. The residential tax base of $160.6 billion is drawn from the DC Office of the Chief Financial Officer Tax Facts Visual Guide (2024), comprising single-family homes ($81.3 billion), multi-family residential buildings ($42.5 billion), condominiums ($34.1 billion), and co-operative apartments ($2.7 billion). DC assesses residential property at 100 percent of market value and applies a tax rate of $0.85 per $100 of assessed value, or 0.0085.

A capitalization rate of 6 percent is used throughout. This figure is standard in the hedonic pricing literature for converting permanent property value changes into annualized welfare flows. It is also empirically grounded in DC's residential market: CBRE and CoStar data place DC multifamily cap rates in the range of 4.7 to 6 percent over the sample period, with 6 percent representing the upper bound of that range. Using the upper bound is conservative in the context of this analysis, because a higher cap rate implies a smaller annualized welfare gain per dollar of asset value increase, making the resulting estimates of willingness to pay and tax revenue lower than they would be under the tighter cap rates that prevailed during DC's strongest market years.

6.2 Cost-Benefit: Annualized Willingness to Pay

A 10 percent citywide reduction in violent crime raises the total residential tax base by $14.82 billion, computed as $160.6 billion multiplied by the elasticity of 0.9262 and the 10 percent crime reduction. This is the aggregate increase in residential asset values that DC homeowners and renters would experience as a result of the safety improvement. To express this as an annual welfare figure rather than a one-time stock gain, it is multiplied by the 6 percent capitalization rate.

Annual willingness to pay = $14.82B × 0.06 = $889 million per year

This $889 million per year is what DC residents collectively pay, through their housing costs, for the improvement in public safety each year going forward. It is the appropriate welfare measure for a cost-benefit analysis because it captures the full value that the housing market assigns to living in a city with 10 percent less violent crime, aggregated across all residential properties in the District. For a 25 percent crime reduction, the annualized willingness to pay rises to $2.2 billion per year. These figures are large, but they are grounded directly in the regression estimates and reflect the magnitude of the asset value that DC's housing market places on neighborhood safety.

6.3 Cost-Revenue Analysis: Property Tax Returns to Crime Reduction

The willingness to pay figures in Section 6.2 measure what residents gain in aggregate housing welfare. A separate new and fiscal question is what the District government itself recovers. Because DC taxes residential property at assessed market value, any crime-induced increase in asset values flows directly into the property tax base, generating additional revenue for the city without any change in the tax rate. The calculation is straightforward.

Using the same $14.82 billion increase in residential asset values from Section 6.2, multiplied by the DC residential property tax rate of 0.0085, the annual property tax revenue gain from a 10 percent crime reduction is as follows.

Annual tax revenue gain = $14.82B × 0.0085 = $126 million per year

Expressed relative to MPD's FY2023 operating budget of $546 million, a 10 percent citywide reduction in violent crime generates additional annual property tax revenue equal to 23.1 percent of the entire police budget. A 25 percent crime reduction generates $315 million per year, equivalent to 57.7 percent of the police budget.

The relation between the willingness to pay figure ($889 million) and the tax revenue figure ($126 million) is worth making explicit. The two numbers differ by a factor of approximately seven, which is simply the ratio of the capitalization rate to the tax rate: 0.06 divided by 0.0085. For every dollar of annualized welfare gain that DC residents experience through improved housing values, the District government recovers roughly 14 cents in property tax revenue. The remainder accrues to property owners and renters in the form of higher asset values and lower effective housing costs. The tax channel is the narrowest possible accounting of the fiscal return to crime reduction, yet even on this conservative basis the figures are large.

6.4 Trends in Police Budget Efficiency

Table 6.4 places the cost-revenue calculation in historical context by tracking the relationship between MPD's operating budget and citywide violent crime over the sample period. As violent crime fell from 6,308 incidents in 2010 to 4,159 in 2019, the police budget grew modestly from $450 million to $515 million, so that the cost of addressing each remaining violent crime on the margin rose from $71,338 to $123,828. Put differently, each crime prevented became progressively more expensive over this period, even as the total number of crimes was falling. The post-2020 homicide surge partially reversed this trend: with crime rising faster than the budget, the per-crime cost fell back to $103,116 in 2023.

This pattern has a direct bearing on the cost-revenue analysis. The period in which crime prevention became most expensive per crime, roughly 2015 to 2019, was also the period in which DC's housing market was most competitive and the property value return to each crime prevented was largest. Even at the elevated per-crime cost of $103,116 in 2023, the $126 million in additional annual tax revenue a 10 percent crime reduction would generate exceeds the total cost of prevention by a wide margin.

Table 6.4: MPD Budget, Violent Crime, and Budget Efficiency, Selected Years

Year	Violent crimes (citywide)	MPD operating budget	Budget per violent crime
2010	6,308	$450M	$71,338
2015	6,173	$470M	$76,138
2019	4,159	$515M	$123,828
2023	5,295	$546M	$103,116

Notes: Violent crime counts are annual citywide totals from MPD incident data. MPD budget figures are annual operating expenditures from DC Office of the Chief Financial Officer public records. Budget per violent crime = MPD operating budget / total citywide violent crimes.

6.5 The Fiscal Return on Crime Reduction Investment

To translate the cost-revenue analysis into a concrete comparison, it is useful to estimate what a 10 percent citywide crime reduction would actually cost to achieve. A 10 percent reduction across the 90 tracts in the estimation sample corresponds to approximately 167 fewer violent crimes per year (90 tracts multiplied by a mean of 18.6 crimes per tract, multiplied by 0.10). Drawing on MacDonald, Klick, and Grunwald (2016), who find that each additional officer prevents approximately 3 violent crimes per year, achieving this reduction would require roughly 56 additional sworn officers. At MPD's all-in cost of $158,000 per officer, the total annual prevention cost is approximately $8.8 million.

Against that $8.8 million cost, the property value and fiscal returns are decisive. The $889 million annual willingness to pay gain represents a benefit-to-cost ratio of approximately 101 to 1 on the full welfare metric. The $126 million in additional property tax revenue represents a ratio of approximately 14 to 1 on the narrower fiscal metric. Even the most conservative accounting, the one that counts only the government's direct tax recovery and ignores all welfare gains to residents, shows that crime reduction returns $14 in property tax revenue for every $1 spent on the policing required to achieve it. To put the fiscal return in concrete operational terms, the $126 million in additional annual tax revenue from a 10 percent crime reduction would fund approximately 797 additional officers, nearly 14 times the 56 officers needed to produce the crime reduction itself.

These estimates are deliberately conservative in a second sense as well. They cover only the residential property tax channel. Not included are commercial property value gains, income tax revenue from the in-migration of higher-income households that safer neighborhoods attract, multiplier effects on local retail and business activity, or the direct welfare benefits to victims of crimes that do not occur. Each of these channels would add to the return. The conclusion that crime reduction is fiscally self-financing through the property tax channel alone therefore represents a lower bound on the full fiscal case for public safety investment in Washington, DC.

6.5 Summary

Table 6.5: Policy Implications of Violent Crime Reduction — Three Approaches

	Δ Asset value	Annual WTP (× cap rate 0.06)	Annual tax (× 0.0085)	ΔTax / MPD budget
10% crime reduction	$14.8B	$889M/yr	$126M/yr	23.1%
25% crime reduction	$37.1B	$2,223M/yr	$315M/yr	57.7%

Notes: Elasticity β = −0.9262 from Table 5.6 Column (1), unbalanced residential panel. Residential tax base $160.6 billion from DC Office of the Chief Financial Officer Tax Facts Visual Guide (2024). Tax rate 0.0085. Capitalization rate 6 percent, consistent with DC residential multifamily market conditions per CBRE and CoStar data and standard practice in the hedonic pricing literature. MPD FY2023 operating budget $546 million. Annual willingness to pay is computed as the change in asset value multiplied by the cap rate. Annual tax revenue is computed as the change in asset value multiplied by the tax rate.

6.6 Efficiency versus Equity in Resource Allocation

The heterogeneity results in Section 4.6 add an important complication to the policy implications above. The property value response to violent crime is three times larger in low-crime tracts than in high-crime tracts, and nearly 27 times larger in high-income tracts than in low-income tracts. This means that the property tax revenue generated per crime prevented is not uniform across the city. It is largest in neighborhoods that are already relatively safe and already relatively wealthy, because those are the neighborhoods where buyers respond most strongly to marginal changes in crime risk.

A policy that allocated crime-prevention resources purely on the basis of maximizing property tax revenue returns would therefore direct those resources toward DC's most advantaged neighborhoods. The fiscal logic is internally consistent, but the distributional implications are troubling. Concentrating public safety investment in already-safe, high-income areas would generate the largest measurable property value gains while leaving the highest-crime, lowest-income neighborhoods, where the direct human cost of violence is greatest, with less protection. There is also a risk that such targeting accelerates gentrification in transitional neighborhoods by making them more attractive to higher-income buyers, compounding existing displacement pressures.

Conversely, targeting crime-prevention resources toward high-crime areas, which is the more common policy approach and the one most directly justified by public safety need, yields smaller property value returns per crime prevented. But the returns to residents in those neighborhoods, measured in terms of physical safety, economic stability, and the ability to remain in their communities, are not captured in property values and may be substantially larger than what the hedonic estimates suggest. The figures in this paper quantify one dimension of the return to crime prevention. How to weigh that dimension against the others is a normative question that belongs to policymakers and the communities they serve, not to the econometric model.

7 Conclusion

This paper provides evidence that violent crime reduces urban property values, with effects that vary substantially across neighborhood types and are robust to a range of alternative specifications. Using comprehensive property transaction and crime data from Washington, DC spanning 2008 to 2023, and employing two-way fixed effects estimation, I find that a one percent increase in the log-rescaled violent crime measure reduces neighborhood property values by approximately 2.2 percent, an estimate that is stable across lag structures, unchanged by the inclusion of property characteristic controls, and consistent across both tract-year and individual transaction specifications.

Four key findings emerge. First, violent crime, but not property crime, significantly affects property values, consistent with buyers responding to threats to personal safety rather than property loss risk. Second, the relationship is linear: no significant curvature is detected across the observed range of crime variation, validating the main log-log specification. Third, the capitalization effect is heterogeneous across neighborhoods in ways that are both economically meaningful and policy-relevant. The effect is three times larger in low-crime tracts than in high-crime tracts, and nearly 27 times larger in high-income tracts than in low-income tracts, consistent with diminishing marginal impacts and differential buyer mobility across the income distribution. Fourth, restricting the sample to 2009 through 2019 and excluding the pandemic years yields an elasticity nearly identical to the full-sample estimate, confirming that the core result is not a product of the unusual market and crime conditions of the post-2020 period.

The fiscal implications of these estimates are substantial. This paper introduces a new cost-revenue framework that asks not only what households are willing to pay for safer neighborhoods, but whether the property tax revenue generated by crime reduction is sufficient to cover the policing costs required to achieve it. On the standard cost-benefit metric, a 10 percent citywide reduction in violent crime generates an annualized willingness to pay of $889 million, reflecting the aggregate value DC residents place on living in a safer city as capitalized into housing prices. On the narrower cost-revenue metric, the same crime reduction generates $126 million in additional annual property tax revenue, equivalent to 23 percent of MPD's FY2023 operating budget, against a prevention cost of approximately $8.8 million. The property tax return to crime reduction alone exceeds the cost of achieving it by a factor of 14 to 1. Crime reduction, in short, more than pays for itself through the fiscal channel before any broader welfare effects are considered. These estimates are conservative: they cover only the residential property tax channel and exclude commercial property gains, income tax effects, and the direct welfare benefits to crime victims.

Several limitations and directions for future research merit attention. First, the analysis covers a single city during a period of dramatic urban transformation. Whether the estimated capitalization effects generalize to slower-growing cities, or to cities experiencing rising rather than falling crime, remains an open question that replication in other contexts would help resolve. Second, while the two-way fixed effects design controls for time-invariant neighborhood characteristics and citywide shocks, the neighborhood controls results suggest that some portion of the baseline estimate may reflect gentrification-correlated improvements rather than crime itself. Tract-specific time trends or redevelopment indicators could provide a more demanding test of the causal interpretation. Third, the analysis captures property value capitalization but does not observe household mobility directly. Whether crime affects who moves in and out of neighborhoods, rather than only what prices they pay, is a distinct and important mechanism that property transaction data alone cannot resolve. Fourth, the violent crime index aggregates multiple offense types, and homicide rates, the most visible and precisely reported category, may drive a disproportionate share of the capitalization effect. Future work separating homicide from assault and robbery could sharpen understanding of which dimensions of violent crime housing markets respond to most strongly.

Finally, while this paper quantifies property value costs and fiscal returns, a complete accounting of crime's social burden would include direct harm to victims, criminal justice expenditures, and foregone economic activity across sectors that property values do not fully capture. The estimates here represent one component of that burden, and a fiscally important one, but the case for crime prevention extends well beyond what any single channel of capitalization can measure.

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