

When Does Crime Enter House Prices? Evidence from Chicago

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Abstract

I estimate how crime affects housing prices in Chicago. Although homebuyers presumably care about crime, most offenses have a near-zero effect on sale prices or rents. I argue that this is consistent with limited information: crime only changes buyers' valuations if they learn of an incident and revise their expectations about future exposure. First, price effects are hyperlocal even though the predictive value of crimes is not. Second, price effects strengthen with agents' local experience. Third, even within an offense, the effect depends on where at the property the incident occurred. These patterns are consistent with buyers responding to incidents they learn about rather than to the crime those incidents forecast.

1 Introduction

A home is most homeowners' largest asset and an important consumption good. Neighborhood crime threatens both dimensions: it undermines the safety of the environment in which families live and the value of the asset they hold. In a 2025 Redfin survey, personal safety and a low crime rate were the two most cited non-negotiables among prospective homebuyers.¹

This paper asks when crime affects housing prices and why. Which offenses are capitalized, and what distinguishes them from those that are not? Where does the discount emerge: in the price a seller asks, or in what a buyer ultimately pays? And which offenses lower rents?

I answer these questions using data from Chicago. I combine 530,395 single-family, condominium, and residential-income transactions on 337,461 properties from the Chicago Multiple Listing Service, spanning 2007 to 2025, with 5.59 million geocoded crime incidents, of which I analyze twenty-two offense categories. The Chicago Police Department recorded 5,657,404 offenses over this period, 5,591,677 of them (98.8 percent) geocoded. The transaction data follow properties

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¹A 2025 Redfin/Ipsos survey of 1,224 prospective homebuyers finds that personal safety (78%) and a low crime rate (74%) are the two most frequently cited "must-have" attributes among eighteen home and neighborhood features (Anderson, 2025).

from listing through sale, allowing me to distinguish the initial asking price from the final sale price and to observe time on market, withdrawals, and returns to market. Rental listings provide the corresponding outcomes in the lease market. The crime data give coordinates for each incident, allowing exposure to be measured around each property.

My identification strategy uses a repeat-sales framework, comparing the same parcel across multiple transactions, adjusting for tract-by-month fixed effects, and further differencing against crime in a 0.20–0.30 mile annulus around the same parcel (finer than the tract). Parcel fixed effects absorb permanent differences across properties; tract-by-month fixed effects absorb contemporaneous shocks shared by the surrounding neighborhood; and the near–far contrast allows for further within-tract adjustments. Exposure within 0.05 miles (264 feet) is contrasted with exposure in a 0.20–0.30 mile annulus; estimates across nine near–far ring definitions are reported in Appendix Table A2. Nonparametric distance profiles motivate these bands: for the property offenses, the price response is concentrated near the property and indistinguishable from zero beyond roughly a block. Moving the treatment band off the doorstep tests where the effect ends: the property-crime discounts disappear beyond the block, while homicide’s extends a few blocks (Section 5.6). I run this design for twenty-two offense categories on sale and lease listings alike, and validate each estimate using a placebo on crime occurring after the transaction, which cannot affect a price already set.

I find that some crimes affect housing prices, while most have near-zero effects. Of the twenty-two reported offense categories, four (homicide, burglary, narcotics, and vandalism) carry a sale-price discount across multiple exposure windows that passes the post-sale placebo at the offense’s central window (burglary’s placebo is significant at two and six months; two more, disorderly conduct and sex offenses, are statistically significant at a single long-horizon window). Homicide, among the rarest offenses, carries the largest discount at about three percent per incident, while common or heavily feared offenses such as theft, motor-vehicle theft, assault, and robbery show no detected discount. An offense can therefore be severe, frequent, or widely feared and still show no detectable discount.

Why, then, are effects confined to particular offenses, when buyers are presumably averse to crime in general? A simple framework helps us think about this question. For an incident to affect the house price, three conditions must hold: (a) buyers must learn of the incident; (b) learning of it must raise their expectation of future crime of that type at the property; and (c) buyers must be willing to pay to avert that crime at the property. The first condition concerns whether information arrives, the second what it implies about future crime, and the third what avoiding that crime is worth: a buyer who sees a car stolen from the curb may expect more car thefts on the block and still pay no less for the house.

Three lines of evidence help explain this pattern. The first concerns observability. Holding the offense fixed, I compare incidents at a dwelling’s frontage with those inside it. For burglary, incidents at the frontage are associated with discounts in seven of twelve windows and incidents inside the dwelling in none, and the difference between the two grows with the exposure window

and is significant at twelve months; vandalism shows the opposite pattern (Section 5.2). Agent experience results point to how that information travels. For homicide, discounts are larger when the buyer’s agent has more experience in the same neighborhood, also after controlling for the agent’s total experience (Table OA25). Although asking prices do not respond to crime on average, they fall with some nearby crimes when the listing agent is more experienced; for narcotics, this gradient reflects the agent’s total rather than local experience. Agents are not randomly assigned to transactions, so these patterns are associations rather than causal effects. The second concerns relevance. The four priced offenses share a risk that stays with the address: to the house (burglary, vandalism), to the people living in it (homicide), or to the block in front of it (narcotics), while motor-vehicle theft, whose loss falls on a movable, insured asset, is a precise zero. This pattern is consistent with relevance but does not test it, since offenses differ in many ways at once. The closer test holds the offense fixed: within vandalism, damage to property lowers sale prices in half of the exposure windows and damage to vehicles in none, although neither estimate is significant at the central window (Section 5.2). The third concerns how buyers use crime information once they observe it. Crime in the surrounding neighborhood, a fifth to three tenths of a mile from the property, forecasts future crime near the property for nearly every offense, priced and unpriced alike. Yet discounts do not rise with how strongly an offense predicts its own recurrence. Among the three priced property offenses the discount per unit of recurrence varies about fivefold, and narcotics, which recurs most predictably, has the lowest ratio; non-domestic assault recurs four times more predictably than burglary and carries no detectable discount. Recurrence alone does not determine which offenses are capitalized.

The second set of results shows that sale-price discounts generally emerge after listing rather than in the seller’s initial ask. Asking prices adjust little overall, while capitalization appears through margins that vary by offense: narcotics lowers the sale-to-list ratio, sex offenses lengthen time on market, and homicide both lengthens time on market and raises the probability of withdrawal; for homicide, the offense with the largest effect, time on market increases by roughly six days relative to a median of 42 days (Appendix Table A1). The broader pattern is that crime affects multiple margins as properties move through the market, so transaction prices alone provide an incomplete measure of market adjustment.

Finally, I apply the same design to rental listings. Burglary lowers asking rents under parcel fixed effects, and sex offenses and liquor violations lower them under parcel-by-era but not parcel fixed effects. Narcotics leaves asking rents unchanged but raises the probability that a rental listing returns to market, and homicide’s rental estimate is too imprecise to sign. Because the data record asking rents rather than negotiated rents, these estimates describe what landlords post.

The estimates are robust to time-varying block characteristics and the choice of distance bands, and inference is robust to Conley spatial standard errors. Adding building permits, vacant buildings, business licenses, tree canopy, and total 311 service requests, each measured as the near-far stock twelve months before sale, moves the coefficients by hundredths of a percentage point, although with all five controls burglary and narcotics are significant only at ten percent, so the discount

does not appear to reflect these measured block characteristics correlated with local crime. Conley standard errors at 0.5 and 1.0 km, reported in Table OA31, are below the clustered standard errors for every priced offense; inference uses the clustered ones (Conley, 1999). With 77 community-area clusters, homicide and burglary remain significant at five percent and vandalism at ten percent, while narcotics is not ($p = 0.12$; Table OA31). For the property offenses, the discount is concentrated in the innermost ring across nine near-far ring definitions (Appendix Table A2), so the contrast is not an artifact of the chosen cutoffs.

This paper makes three contributions.

First, I measure crime exposure at the property and incident level across a broad set of offenses and use this common design to examine why some offenses are capitalized while others are not. Studies using local crime rates typically assign exposure at more aggregate geographies (Thaler, 1978; Schwartz et al., 2003; Gibbons, 2004; Ihlanfeldt and Mayock, 2010), making estimates sensitive to the spatial units used to construct exposure, the modifiable areal unit problem (MAUP), and making it difficult to distinguish crime on a property’s own block from broader neighborhood conditions. A separate event-study literature achieves much finer spatial resolution by linking individual properties to discrete, point-located events, including sex-offender arrivals (Linden and Rockoff, 2008; Pope, 2008a) and methamphetamine-lab discoveries (Congdon-Hohman, 2013), but typically focuses on a single crime or event type. Point-located designs have also been applied to murders in Sydney (Klimova and Lee, 2014), and tract- or ward-level panels to violent and property crime in the San Francisco Bay Area, Barcelona, Stockholm, Columbus, and Jacksonville (Bishop and Murphy, 2011; Buonanno et al., 2013; Ceccato and Wilhelmsson, 2011; Tita et al., 2006; Lynch and Rasmussen, 2001); at the city level, crime predicts out-migration and price declines (Cullen and Levitt, 1999; Besley and Mueller, 2012; Ajzenman et al., 2015). Across these designs the estimated effect of crime is usually negative, though not for every offense (Table OA4). Studies that separate offenses report zeros or positive estimates for some property crimes: Braakmann (2017) for burglary and vehicle crime, which he attributes to reverse causality, and Taltavull de La Paz et al. (2022) for property crime, vandalism, and non-aggravated assault committed farther from the home. I combine this spatial resolution with broad offense coverage, linking incidents across twenty-two offense categories to individual properties and estimating the price effect of an additional incident of each offense under a common design. This breadth allows me to ask not only which offenses are priced, but why. Gibbons (2004) conjectures that hard-to-observe crimes have weak price effects because prospective buyers rarely learn of them; this paper provides the first systematic test of that conjecture’s implications, across twenty-two offenses under a common design. Because offenses differ simultaneously in severity, exposure, visibility, and persistence, comparisons across offenses alone cannot establish which dimension drives capitalization. I therefore examine whether the size of the discount tracks an offense’s ability to predict its own recurrence and exploit within-offense variation in where incidents occur to probe the role of observability (Table 3, Appendix Table A5, and Tables OA10 and OA12).

Second, I trace how crime affects the housing transaction rather than observing only the clearing

price. I examine asking prices, time on market, withdrawal, and the sale-to-list price ratio, allowing me to distinguish whether crime is incorporated into the seller’s initial asking price or emerges through subsequent market adjustment (Table 4). Boggess et al. (2013) find that higher crime is followed by higher housing turnover, and Wentland et al. (2014) estimate price and time-on-market effects of registered sex offenders from MLS data; neither traces where in the transaction the adjustment occurs or does so across offenses.

Third, I examine how crime is capitalized in rental transactions, a market studied far less in the crime-capitalization literature. Existing rent studies cover few offenses. Most use aggregate crime rates for community areas (Rizzo, 1979), cities (Koirala et al., 2023), or labor-market locations (Donovan et al., 2024), often within compensating-differentials frameworks. Studies at the property level examine one or two offense types: Carrillo and Yezer (2021) show theoretically that rent and value hedonics can diverge and, in an application, compare responses to homicide and violent crime within 0.25 miles of individual units, and Klimova and Lee (2014) link Sydney rental listings to nearby murders and find no effect on rents. Relative to these studies, I link rental listings to incidents across twenty-two offense categories and trace crime through the margins of both markets (asking prices and rents, time on market, withdrawal, and closing prices) within one city and design (Tables 4 and 6).

The remainder of the paper proceeds as follows. Section 2 develops the conceptual framework, which links crime capitalization to the information available to market participants and to belief updating about exposure relevant to the housing transaction. Section 3 describes the property-sale, rental-listing, and geocoded crime data and the construction of property-level crime exposure. Section 4 presents the empirical strategy for estimating the effect of an additional nearby crime incident on housing-market outcomes. Section 5 reports the main results, examines heterogeneity across offenses and incident locations, and assesses robustness. Section 6 interprets the results: why asking prices respond little, who carries the information, what the estimates mean for welfare, and how they bear on valuing crime reduction. Section 7 concludes.

2 Conceptual framework

The observed crime discount is the price consequence of a change in expected crime exposure. To make this explicit, suppose first that a household values occupancy at a property over a fixed horizon (thereby allowing us to ignore resale value). Let q_k denote the buyer’s subjective expected number of future incidents of offense k near the property over that horizon, and let w_k denote willingness to pay to avoid one expected incident, expressed as a fraction of the property’s value before the incident. Expected crime exposure therefore carries value $w_k q_k$.

Buyers enter the market with prior beliefs about q_k , formed from information already available about the location. A newly realized incident can affect valuation through two additional margins. First, information about the incident must reach the relevant market participant. Let $S_k \in [0, 1]$ denote the probability of information arrival: $S_k = 0$ means that the incident does not enter the

participant’s information set, while $S_k = 1$ means that information about it reaches the participant with certainty. Second, conditional on information arrival, the incident may change the participant’s subjective expectation of future exposure. Define

$$b_k(S_k) \equiv \frac{\partial q_k}{\partial C_k^o}, \quad (1)$$

where C_k denotes realized incidents of offense k and C_k^o those whose information reaches the participant. Thus $b_k(S_k)$ measures the change in expected future incidents generated by one additional observed incident. Writing b_k as a function of S_k allows the update per observed incident to differ across offenses with how often such incidents are learned of. Housing markets price what buyers can observe: asymmetric information about a property’s condition and its surroundings shows up in prices and in who buys (Garmaise and Moskowitz, 2004; Kurlat and Stroebel, 2015; Stroebel, 2016), and disclosure of a hazard moves prices only once buyers learn of it (Pope, 2008b; Bernstein et al., 2019; Hino and Burke, 2021; Bakkensen and Barrage, 2022; Gourevitch et al., 2023). In the crime setting, Gibbons (2004) argues that visible offenses such as vandalism are priced because they are hard to conceal from buyers, while information about other crimes stays with sellers and neighbors. The availability of information does not ensure that it enters a decision. Chetty et al. (2009) show that consumers respond more strongly to taxes included in posted prices, even when they know that additional taxes apply. Bordalo et al. (2012, 2013) show how salient payoffs and attributes receive disproportionate weight in choice. These mechanisms motivate distinguishing information arrival from the response to information, but salience need not operate through belief updating alone. The comparisons below do not separately identify information arrival, belief updating, and valuation.

Unlike S_k , $b_k(S_k)$ is not bounded between zero and one. If $b_k(S_k) = 0$, the participant learns about the incident but does not revise expected future exposure. A value between zero and one means that one additional realized incident raises expected future exposure by less than one incident; a value of one implies a one-for-one increase; and a value greater than one means that one realized incident leads the participant to expect more than one additional future incident over the relevant horizon. Importantly, this is a subjective belief update, not the objective recurrence rate of the offense.

The update concerns counts, not value. A buyer can expect more incidents and attach no value to avoiding them, so $b_k > 0$ with $w_k = 0$ and $\kappa_k = 0$. This is why relevance and updating enter as separate terms.

Following Rosen (1974), define the per-incident discount as the implicit price of an incident: the proportional change in the value of the property, P , per additional realized incident of offense k nearby, C_k ,

$$\kappa_k \equiv -\frac{\partial \ln P}{\partial C_k}. \quad (2)$$

This is the capitalization effect in the terminology of Kuminoff and Pope (2014).² The framework

² κ_k is the marginal effect of one additional incident at the property’s current exposure. The linear specification

assumes that it equals the product of three terms,

$$\kappa_k = \underbrace{w_k}_{\text{WTP to avoid one expected incident}} \times \underbrace{S_k}_{\text{information arrival}} \times \underbrace{b_k(S_k)}_{\text{expected incidents per observed incident}}, \quad (3)$$

where w_k captures the location-specific value of avoiding expected exposure to offense k . An offense may impose substantial costs on its victims yet carry little location-specific valuation if those costs are not associated with occupying or owning property at that location.

Written in units, w_k is a fractional price change per expected future incident, S_k is the share of realized incidents that reach the buyer, and $b_k(S_k)$ is expected future incidents per observed incident, so the three terms multiply to the log-price discount per realized incident estimated empirically:

$$\underbrace{\frac{\Delta \ln P}{\text{expected incident}}}_{w_k} \times \underbrace{\frac{\text{incidents observed}}{\text{incidents realized}}}_{S_k} \times \underbrace{\frac{\text{expected incidents}}{\text{observed incident}}}_{b_k(S_k)} = \frac{\Delta \ln P}{\text{realized incident}}. \quad (4)$$

Consider a buyer who would pay 1 percent of a property's value to avoid one expected future burglary, so $w_k = 0.01$. If one realized burglary reaches the buyer ($S_k = 1$) and raises expected future burglaries over the horizon from 0.5 to 2.5, the belief update is $b_k(S_k) = 2$ and, since the incident is observed ($S_k = 1$), the implied discount is $\kappa_k = w_k b_k(S_k) = 0.01 \times 2 = 0.02$, or 2 percent per realized burglary. The framework expresses κ_k as a positive magnitude. The estimate δ_k in the empirical sections is a signed price change, so a 2 percent discount appears as $\delta_k = -2$. A value of $b_k(S_k)$ above one simply means one realized burglary leads the buyer to expect more than one additional future burglary; it is not a probability. Had the incident not reached the buyer, or reached the buyer without changing expected exposure, this channel would generate no price response.

A zero observed discount can therefore arise through three distinct channels: willingness to pay to reduce expected exposure may be zero ($w_k = 0$); information about the realized incident may not reach the participant ($S_k = 0$); or the incident may be fully observed but leave expected future exposure unchanged ($b_k(S_k) = 0$). The last case is particularly important. A buyer may place a high value on avoiding future homicide and learn that a homicide occurred nearby, yet view the incident as isolated and therefore not revise the expected risk of future homicide. The price response is then zero despite high willingness to pay and full information. Thus, full information ensures that the incident is known, but not that it changes beliefs about future exposure; a zero capitalization estimate therefore need not imply low willingness to pay.

Equation (3) is an accounting decomposition, not a structural model. Equation (3) assumes that the buyer's entire valuation change is reflected in the sale price. With the seller's reservation value fixed, this corresponds to $\theta = 1$ in the bargaining illustration in Section 6.4, which relaxes

in Section 4 estimates a regression-weighted average of this effect across counts, with weights set by the residual variation in exposure; Table OA6 reports how it varies with the count.

this assumption. Prior hedonic estimates of crime report the price response to a realized incident and interpret it as a willingness to pay. With $S_k = 1$ and $b_k = \rho_k$, the discount per realized incident is $w_k \rho_k$: willingness to pay per expected future incident, scaled by how many such incidents one realized incident implies. It equals w_k only when $\rho_k = 1$. The decomposition here keeps the two apart and asks what the data say about each term. Kuminoff and Pope (2014) note a further wedge even then, re-sorting of heterogeneous buyers and sellers, so a capitalization effect recovers marginal willingness to pay only absent that sorting (Rosen, 1974; Bishop and Murphy, 2011). The tests in Section 5.2 use only that the three terms are separable.

Prediction 1: Observability and Relevance. An incident is capitalized only if it reaches the buyer and bears on what the buyer is purchasing ($S_k > 0$ and $w_k > 0$). Holding the offense fixed, incidents that are more likely to be observed should carry larger discounts than incidents that go unseen. Across offenses, only those whose threat stays with the location should price; an offense whose harm travels with the household should not, however observable it is. I test the first condition by splitting incidents of the same offense by where they occurred and whether they are visible from outside the dwelling, and the second by classifying offenses by what the harm attaches to.

Prediction 2: Recurrence. The belief update $b_k(S_k)$ is subjective and is not observed directly. I therefore ask whether capitalization tracks how well each offense predicts future crime. Holding valuation and information arrival fixed, stronger predictive content should generate a larger discount; across offenses these other components may differ, so recurrence alone need not determine the ranking of discounts. I use a forecasting regression of whether current crime predicts recurrence to measure what an incident objectively implies about future crime, not what buyers believe it implies. If discounts do not rise with recurrence, recurrence alone does not determine which offenses are priced.

Prediction 3: Price Formation. The framework describes a change in buyers' valuation but does not say where in the transaction that change appears. If sellers anticipate the effect on buyers' willingness to pay, asking prices should adjust. If they do not, the discount can still emerge after listing, through a lower sale-to-list ratio, a longer spell on the market, or a greater chance the listing is withdrawn. I use these margins to trace where capitalization enters the transaction.

3 Data

The analysis combines three sources: residential listings from the Chicago Multiple Listing Service, incident-level crime reports from the Chicago Police Department, and parcel-level measures of neighborhood conditions. This section describes each source and the construction of the estimation sample.

Chicago is appropriate for this analysis because it provides variation along both dimensions the design requires. First, the volume and breadth of crime incidents allow effects to be estimated separately across the 22 offense categories reported here: the data contain 10,766 homicides alongside more than 1.3 million thefts over the sample period. Second, crime varies substantially at fine spatial scales: roughly 55 percent of the variation in doorstep crime exposure occurs within tract-months, against 14 percent for crime in the surrounding 0.20–0.30 mile ring. The doorstep varies within neighborhoods; the surrounding area largely does not.

Housing transactions. The housing data are the universe of Chicago Multiple Listing Service (MLS) closed transactions in single-family, condominium, and residential-income properties. I keep transactions closing between 2007 and 2025 with a recorded close price between \$10,000 and \$10,000,000, and parcels whose geometry is stable across their sale history, a constant parcel count, so that the near–far rings measure the same ground at each sale and are not contaminated by parcels that split or consolidate. The resulting sample is 530,395 sales on 337,461 parcels, of which 136,866 transact at least twice and so identify the within-property comparison. Single-family homes make up 88 percent of these repeat sales, two-to-four-unit buildings 11 percent, and condominiums 1 percent. Table OA1 reports the construction. Each record carries the property identifier, close date, and close price and the full listing history: original and revised list prices, days on market, and whether the listing ended in sale or withdrawal. That history is what lets me trace where a discount forms, from the asking price through negotiation to the close.

Lease listings. The rental analysis uses the parallel universe of MLS lease listings, to which I apply the same property-type, location, and geometry filters as the sale sample: single-family, condominium, and residential-income units in Chicago, on parcels of constant parcel count, listed 2007–2025 with a monthly asking rent between \$500 and \$10,000. This yields 191,155 leased listings. Leases rarely record a closing rent (only 0.1 percent do), so for rentals the price outcome is the *asking* rent, timed to the list date, and the design otherwise mirrors the sale side, including the friction margins (days on market and withdrawal). Running the identical near–far comparison on leases, a different sample and a different price, asks whether the same offense is capitalized differently by what the transaction conveys: occupancy without ownership or resale.

Property and listing characteristics. Table A1 summarizes both samples. The median sale is a three-bedroom, two-bathroom property that closes at \$279,000; the median lease is a smaller two-bedroom unit at \$1,950 per month. Physical characteristics (living area, bedrooms, bathrooms, and year built) are taken from the standardized listing fields and are recorded for the large majority of transactions; two-thirds of sale listings record at least one reduction in the asking price before sale.

Crime incidents. Incident-level crime data come from the Chicago Police Department’s public “Crimes, 2001 to present” file. Over 2007–2025, 5,657,404 offenses were reported citywide, of which

98.8 percent carry coordinates for the location of the incident. Figure OF1 maps their density across the city. Incident locations are released at block-face resolution: the published point is shifted from the actual location for partial redaction but falls on the same block, so distance between a parcel and an incident is measured with error of up to a block face. For homicide the file records one row per victim rather than per incident, so homicide counts, and the per-incident homicide estimates below, are per victim record.³ Coordinate coverage varies systematically by offense: from 78.8 percent for embezzlement and 89–97 percent for sex offenses and for offenses without a fixed incident location (fraud, forgery, bad checks), to 97.9 percent or above for every offense that capitalizes into housing, and 99.6 percent or above for all but narcotics (homicide 100.0, burglary 99.6, vandalism 99.6). Attrition therefore falls largely on the offenses that return nulls, not on those that price, with the exception of sex offenses, which have one of the lowest coverage rates yet price in rents. I estimate separate effects for 27 of the 29 offense groups, omitting human trafficking and nonforcible sex offenses (222 incidents combined) as too sparse.⁴ Of these 27, I report 22 in the main analysis. I set aside the residual “all other offenses” category, which names no specific offense, and four offenses, kidnapping, embezzlement, extortion, and child pornography, that are too sparse to identify: their standard errors run from roughly 0.6 to 1.3 percentage points, too wide to rule out either a null or a large discount. The twenty-two reported offenses appear at every window in Appendix Table A4. For each listing I count incidents of each offense type within concentric rings of the parcel centroid over pre-transaction windows of one to twelve months, using a ball-tree spatial join,⁵ and time the counts to the listing’s close date (to its list date on the lease side). Table 1 reports, for each offense, its citywide count, geocoding rate, and near exposure, together with the raw cross-sectional price gap between listings with and without a nearby incident. These raw gaps are large and, for homicide and narcotics, exceed 40 percent, but they are heavily confounded: near-crime blocks differ from far blocks on many dimensions, and theft, the most common offense, even carries a *positive* gap because it concentrates in high-price commercial districts. The confounding also shows up in observables: Table OA2 splits the sample by whether any incident occurs within 0.05 miles in the three or six months before sale, and listings near recent crime are cheaper, smaller, and slower to sell. My within-parcel, tract $\times$ month design is intended to address such confounding.

³To protect victim privacy, the Chicago Police Department releases each incident’s address “at the block level only” and does not identify the specific location (City of Chicago Data Portal, *Crimes – 2001 to Present*); the recorded point is placed on the correct block, so it can differ from the true location by up to a block face. Chicago blocks are about 330 feet east–west and 660 feet north–south, against a near-ring radius of about 264 feet, so the shift can move an incident into or out of the near ring; it adds measurement error to the near count, which is expected to attenuate δ toward zero.

⁴The pornography/obscene-material group is predominantly child pornography rather than adult material: about two-thirds of the Chicago Police incidents so classified are child pornography (IUCR 1582) or related offenses involving children, and roughly one-quarter is adult obscene material (obscene telephone calls are classified separately, under all other offenses). I therefore label this group “child pornography/obscenity” throughout. It is sparse, returns nulls in every market, and its estimates appear only in the appendix.

⁵Distances are great-circle (haversine) distances between each incident’s coordinates and the parcel’s latitude–longitude centroid (the Cook County Assessor coordinate). A ball-tree is a spatial index that sorts the geocoded incidents by great-circle distance so that all incidents within a given radius of a parcel can be retrieved efficiently; the resulting ring counts are exact, identical to computing every parcel–incident distance directly.

Table 1: Crime Incidence and Near Exposure

Offense	Incidents (city)	Geocoded (%)	Mean near count	% any near	Raw price gap (%)
Theft	1,364,912	97.8	5.56	83.2	13.7
Assault (non-domestic)	739,961	99.7	1.92	56.3	-20.9
Assault (domestic)	670,887	99.7	1.71	55.5	-35.6
Criminal damage/vandalism	632,166	99.6	1.88	67.1	-18.2
Narcotics	439,526	97.9	0.83	25.3	-45.5
Other offenses	360,851	99.0	1.09	49.5	-24.1
Burglary	296,724	99.6	0.97	47.1	-15.5
Motor vehicle theft	292,290	99.6	0.87	46.5	-9.9
Robbery	213,364	99.8	0.55	32.3	-12.2
Trespass	138,862	99.7	0.41	23.8	-1.3
Fraud	136,957	97.5	0.71	33.4	18.0
Weapons	114,818	99.6	0.23	14.4	-36.1
Sex offenses	63,549	89.2	0.18	14.6	-9.8
Disorderly conduct	39,056	99.7	0.09	7.4	-24.2
Forgery	32,711	97.1	0.11	8.9	15.9
Prostitution	31,173	99.6	0.07	2.2	-14.9
Family (nonviolent)	29,131	99.0	0.07	5.9	-35.2
Homicide	10,766	100.0	0.02	2.0	-42.9
Arson	9,488	99.7	0.02	2.1	-35.8
Bad checks	9,424	97.3	0.03	3.0	12.8
Gambling	8,105	99.4	0.02	1.2	-72.6
Liquor violations	7,941	99.5	0.03	2.2	2.5
Kidnapping	5,921	99.5	0.01	1.4	-34.0
Extortion	3,281	98.7	0.01	1.1	4.4
Child pornography/obscenity	2,677	89.8	0.01	0.6	-4.3
Stolen property	1,586	98.6	0.00	0.4	1.3
Embezzlement	1,055	78.8	0.00	0.4	45.4

Notes: Offense groups ordered by citywide incident count (Chicago Police Department, 2007–2025). Near exposure: 0–0.05 miles, twelve months before sale ($N = 530,395$). Raw gap: cross-sectional log-price difference (percent) between listings with and without a near incident; descriptive only.

Neighborhood conditions. To guard against the discount proxying for physical decline or disinvestment rather than crime, I assemble five predetermined measures of neighborhood condition at the parcel: tree canopy, active business licenses, 311 service requests, vacant buildings, and building permits.⁶ Each enters with the same near-ring and far-annulus structure as crime, differenced within parcel and tract $\times$ month, but as a cumulative stock of events recorded more than twelve months before the close, that is, before every crime-exposure window used, so it is predetermined relative to the crime and cannot be an outcome of it. Permits, licenses, 311 requests, and vacant buildings are time-varying, counted as of that pre-crime cutoff for each listing; tree canopy is read from the land-cover raster two years before the sale. Foreclosures lower nearby prices and raise nearby crime (Campbell et al., 2011; Ellen et al., 2013; Anenberg and Kung, 2014), so I control for the predetermined vacancy stock near and far.

4 Empirical Strategy

4.1 Design and Identification

The design is a spatial difference-in-differences with two-way fixed effects and continuous treatment. Crime exposure within a narrow ring around the property constitutes the near treatment, while exposure in a wider annulus around the same property provides the spatial comparison. Identification combines three comparisons: within parcel across transactions, within tract and month, and near relative to farther crime exposure (Figure 1). Parcel fixed effects absorb permanent differences across properties, and tract-by-month fixed effects absorb contemporaneous shocks shared by the surrounding neighborhood. This division of variation is borne out in the data: only 14 percent of far-ring crime exposure varies within tract-months, so the surrounding annulus is almost entirely a neighborhood-level quantity that the fixed effects absorb nearly in full, whereas a majority of doorstep exposure survives within tract-months (Section 3). The near-relative-to-far contrast is therefore identified from within-neighborhood differences in exposure. The parameter of interest is the differential price response,

$$\delta = \beta_{\text{near}} - \beta_{\text{far}}.$$

The estimate δ_k corresponds to $-\kappa_k$ in Equation (3) under the linear approximation and the far-ring restriction: it is a regression-weighted average of the per-incident effect across counts.

⁶Tree canopy is percent canopy cover from the U.S. Geological Survey National Land Cover Database (NLCD) Tree Canopy Cover product (30 m resolution). Business licenses, 311 service requests, and building permits are geocoded records from the City of Chicago Data Portal (<https://data.cityofchicago.org>). Before the City’s unified 311 system launched in December 2018, the portal releases requests in category-specific historical files; the 311 control combines those files for 2011–2018 (graffiti removal, sanitation code complaints, vacant and abandoned buildings, and abandoned vehicles) with requests of all types from the unified system for 2019–2024, so its composition changes in 2019. Vacant and abandoned buildings are “Vacant and Abandoned Buildings Reported” records from the same portal; because such complaints also enter the City’s unified 311 stream from 2019 onward, this measure partly overlaps the total-311 control over the recent period.

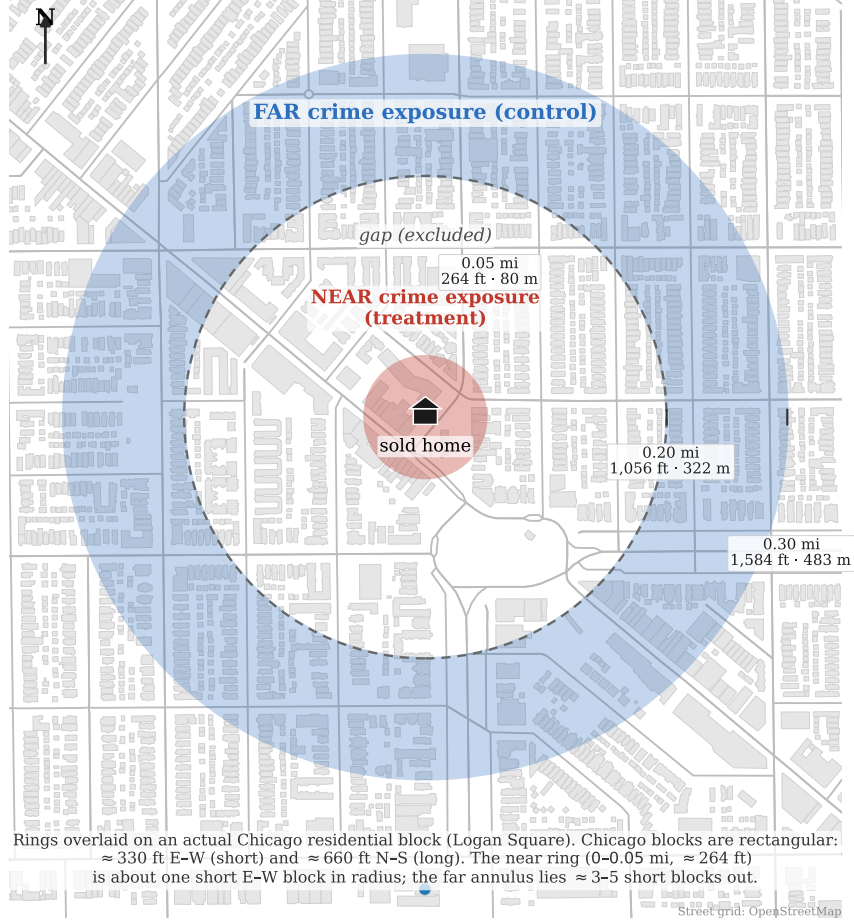


Figure 1: The near–far design. Crime within the near disk (0–0.05 miles, about 260 feet, roughly one short east–west Chicago block) is the treatment; crime in the far annulus (0.20–0.30 miles, about three to five short blocks out) is the spatial comparison, separated from the doorstep by a gap so the two rings are non-adjacent. The near–far difference $\delta = \beta_{\text{near}} - \beta_{\text{far}}$ differences out conditions common to the surrounding neighborhood. Near disk in red, far annulus in blue; incidents in the gap and beyond the far ring (grey) are excluded.

For property i transacting at date t , let Near_{it} be the count of incidents of a given offense within 0.05 miles of the parcel in the m months before the sale, and Far_{it} the count in the 0.20–0.30 mile annulus over the same window, the same comparison ring as Tang and Le (2023); Appendix Table A2 reports alternative far rings. Stale counts SNear_{it} and SFar_{it} , near- and far-ring incidents in the part of a twelve-month lookback that falls *beyond* the m -month window, and therefore non-overlapping with Near_{it} and Far_{it} , absorb slower-moving local conditions; they are empty when $m = 12$. The window length m is not fixed a priori; I estimate across $m = 1, \dots, 12$ months and examine the choice in Section 4.2. The estimating equation is

$$\ln P_{it} = \beta_{\text{near}} \text{Near}_{it} + \beta_{\text{far}} \text{Far}_{it} + \gamma_n \text{SNear}_{it} + \gamma_f \text{SFar}_{it} + \mu_i + \lambda_{c(i)t} + \varepsilon_{it}, \quad (5)$$

where μ_i is a parcel fixed effect and $\lambda_{c(i)t}$ a tract $\times$ month fixed effect.

Identification requires that, after accounting for parcel and tract-by-month fixed effects and

the included controls, the difference in crime exposure between the near ring and the surrounding annulus is unrelated to other factors that affect the sale price. This does not require near and far rings to be alike in levels, or unconditional parallel trends across high- and low-crime properties. Parcel fixed effects absorb time-invariant property differences, and tract-by-month fixed effects absorb shocks shared within a tract and month. What remains is block-level variation across a property’s own transactions. Formally, let $\mathbf{X}_{it} = (\text{Near}_{it}, \text{Far}_{it})'$. Identification requires

$$E\left[\tilde{\mathbf{X}}_{it} \tilde{\varepsilon}_{it}\right] = \mathbf{0}, \quad (6)$$

where tildes denote residualization with respect to the fixed effects and controls in Equation (5). Under this condition β_{near} and β_{far} are identified, and so is $\delta = \beta_{\text{near}} - \beta_{\text{far}}$.

Two threats to this assumption, and the interpretation of δ , are addressed directly.

1. *No shocks correlated with the near–far difference.* The concern is an event that raises crime on the block relative to the surrounding annulus and lowers the price for a reason of its own, such as a building abandoned next door. A shock shared by the block and the annulus is differenced out; one confined to the block is not. Such an event is a confounder: it moves both the near–far crime difference and the price, so the estimate would attribute its price effect to crime. Five predetermined neighborhood measures (building permits, vacant buildings, business licenses, tree canopy, and 311 service requests), each entered as its own near–far difference and dated before the crime window, proxy for the block-specific events that can be observed (Section 5.6, Appendix Table A7).
2. *No reverse causality or mean reversion.* Crime exposure is measured before the sale, so the price cannot mechanically produce it. But a property that sells at a temporarily depressed price may sit on a block where crime was already rising, so the estimate would pick up the block’s trajectory rather than the incident. Crime that occurs after the closing cannot affect a price already set, so a post-sale placebo that loads would indicate this kind of confounding; a null is consistent with the assumption without establishing it. Every estimate has a mirror placebo that substitutes post-sale for pre-sale crime.

The interpretation requires that the far ring is beyond the effect’s reach. δ compares marginal responses to near and far exposure and equals the doorstep effect only under the far-ring restriction, that crime a fifth to three tenths of a mile out has no doorstep effect of its own. Crime at that distance is the benchmark for what would have happened on the block anyway; if an incident there still moved the price, the contrast would recover a relative effect rather than the full doorstep effect. The spatial decay results are consistent with the restriction: the discount is gone by the adjacent block.

The design does not assume that crime is randomly distributed across the city, across neighborhoods, or over time; only that the residual near–far variation is unrelated to unobserved price shocks. Section 5.6 reports each check.

The parcel fixed effect restricts identification to the 136,866 parcels that transact more than once (329,800 sales). The tract-by-month fixed effect then drops any transaction that is the only sale in its cell, together with the singletons this creates, leaving the 242,815 transactions that identify the two-way model and form the sample in the results tables (Table OA1).

Enforcement intensity affects recorded crime directly (Levitt, 1998) and can bias the estimate in either direction. Where enforcement deters crime and improves the block, detection may still raise recorded counts, so a block that is getting safer shows more incidents and the discount is attenuated. Where enforcement is aimed at blocks that are already deteriorating, recorded counts rise alongside falling prices and the discount is inflated. Tract-by-month fixed effects absorb enforcement changes shared within each tract and month.⁷ Uneven deployment within tracts, including enforcement targeted at particular addresses, remains a concern. This matters for offenses recorded through enforcement rather than victim complaint, which here means narcotics, prostitution, gambling, and liquor violations, and I describe those measures as enforcement-recorded offenses rather than as a census of crime.

4.2 Choice of Spatial Bands and Exposure Windows

The rings and windows in (5) are chosen from the data rather than imposed. I estimate the price effect as a smooth function of distance and time since incident by a tensor-product spline⁸ (Wood, 2017; Korolev, 2026). The distance profile is significantly negative at the doorstep and returns to zero by the adjacent-block annulus, so the near ring sits in the steep part of the profile and the far ring beyond its reach (Figure 2; Online Appendix J). Because administrative units are not the area over which a household experiences crime, aggregating to them would make coefficients shift with the unit chosen, the modifiable areal unit problem (MAUP), so I report estimates across nine near-far ring definitions (Appendix Table A2) and all one-to-twelve-month windows. Tables that report a single number per offense use the central window of the range over which that offense’s discount is significant at five percent (pooled sample, headline 0–0.05 versus 0.20–0.30 mile ring), taking the lower of the two central windows when that range has even length, and offenses with no significant window are shown at the median window, six months. Summary tables use the central window among those with significant discounts; Table 2 reports estimates across all twelve windows. Significant-window counts are descriptive; the windows are nested, not independent.

Section 5.6 reports four robustness checks. A post-transaction placebo addresses reverse causality and mean reversion; predetermined near-far measures of neighborhood conditions address hyper-

⁷A Chicago police beat averages about 0.8 square miles and spans roughly three census tracts; the 791 tracts in the estimation area average about 0.29 square miles. Beats nest within sectors and sectors within the city’s roughly two dozen districts. Beat and tract boundaries cross rather than nest, so beat-level deployment is absorbed only in part.

⁸A tensor-product spline models a function of two variables as a weighted sum of products of one-dimensional basis functions, here a basis in distance from the parcel and a basis in months since the incident. Each product term lets the profile in one dimension vary with the other, so the distance profile can differ across lags without imposing a parametric form. The basis sizes are $c(6, 12)$, six in distance and twelve in lag, and the smoothness penalty is chosen by restricted maximum likelihood (Wood, 2017).

local confounding; a full-universe model of sale propensity and a complementary selection correction assess endogenous transaction selection; and a mass-point diagnostic following Caetano (2015) and Caetano et al. (2021) addresses selection into zero crime exposure. The full set of diagnostics is reported in the robustness section. The comparisons that follow use incident location and recurrence to distinguish among the mechanisms in the conceptual framework.

The parameter of interest is the effect of a recent incident on the property’s block, relative to the surrounding neighborhood in the same month. The spatial scale is not imposed: the distance profile in Figure 2, estimated without rings, locates the response at the doorstep, and the rings follow it. The estimates do not capture the long-run co-movement of crime and prices across neighborhoods.

5 Results

5.1 Sale Prices

Appendix Table A4 reports estimates for every offense at all twelve windows.

Table 2 reports sale-price estimates for all twenty-two offenses. Across the twelve windows, the estimates fall into four broad groups: *priced*, *suggestive*, *precise nulls*, and *inconclusive*. Table 2 is ordered accordingly, with classification criteria reported in the table notes.

Homicide, burglary, narcotics, and vandalism are priced. Disorderly conduct and sex offenses are suggestive. Twelve offenses are precise nulls: their intervals rule out a one-percent discount in at least nine of twelve windows, and for larceny, motor vehicle theft, and non-domestic assault they also rule out a discount as large as burglary’s (0.24 percent per incident); the estimates for bad checks and family offenses are positive. Weapons, arson, gambling, and stolen property are inconclusive.

Table 2: Sale Prices

Offense	Identifying parcels	Sig. discount windows	Min δ (s.e.) [mo]	Max δ (s.e.) [mo]	Peak δ (s.e.) [mo]
<i>Priced</i>					
Homicide	5,701	2/12	-5.86* (3.35) [1]	-1.24 (1.43) [8]	-3.15** (1.38) [11]
Narcotics	53,844	5/12	-0.93 (0.75) [1]	-0.18 (0.19) [6]	-0.38** (0.18) [8]
Burglary	88,094	6/12	-0.34** (0.14) [6]	-0.18* (0.09) [12]	-0.34** (0.14) [6]
Vandalism	104,661	2/12	-0.17** (0.08) [6]	0.26 (0.22) [1]	-0.17** (0.08) [6]
<i>Suggestive</i>					
Disorderly conduct	19,550	1/12	-1.33* (0.78) [2]	-0.46 (0.54) [4]	-0.65** (0.29) [12]
Sex offenses	36,393	1/12	-0.47** (0.23) [12]	0.11 (0.35) [3]	-0.47** (0.23) [12]
<i>Precise nulls</i>					
Larceny/theft	115,548	0/12	0.01 (0.02) [12]	0.08*** (0.03) [1]	—
Motor vehicle theft	85,781	0/12	-0.02 (0.09) [12]	0.51* (0.30) [1]	—
Assault (non-dom.)	91,783	0/12	-0.05 (0.06) [7]	0.01 (0.11) [3]	—
Bad checks	8,919	0/12	0.66* (0.35) [11]	2.20** (1.03) [1]	—
Family (non-violent)	15,928	0/12	1.00* (0.51) [12]	2.16*** (0.82) [5]	—
Fraud	67,030	0/12	-0.36 (0.23) [1]	-0.04 (0.09) [6]	—
Assault (domestic)	90,500	0/12	-0.19* (0.10) [12]	0.02 (0.15) [5]	—
Trespass	54,025	0/12	-0.15 (0.14) [6]	0.18 (0.22) [3]	—
Robbery	65,876	0/12	-0.06 (0.14) [9]	0.30 (0.42) [1]	—
Forgery	23,614	0/12	-0.07 (0.59) [1]	0.47 (0.45) [2]	—
Liquor law	6,393	0/12	0.08 (1.20) [1]	1.34 (0.91) [2]	—
Prostitution	6,326	0/12	-1.00 (0.82) [1]	0.08 (0.12) [12]	—
<i>Inconclusive</i>					
Weapons	32,294	0/12	-0.62 (0.47) [5]	0.11 (0.58) [3]	—
Arson	6,116	0/12	-0.20 (1.06) [11]	1.27 (3.58) [1]	—
Gambling	3,125	0/12	-4.77 (4.33) [3]	1.99 (8.32) [1]	—
Stolen property	1,262	0/12	-2.23 (2.63) [2]	0.81 (1.20) [8]	—

Notes. $\delta = (\beta_{\text{near}} - \beta_{\text{far}}) \times 100$: percent change in sale price per incident within 0–0.05 miles, relative to 0.20–0.30 miles. Parcel and tract $\times$ year-month fixed effects; standard errors clustered by tract; $N = 242,815$ repeat sales, 2007–2025. Estimated at each cumulative window, one to twelve months. Sig. windows: negative and significant at 5%. Range: most negative and most positive estimates (standard error; window in brackets). Peak: window of the most negative significant estimate. Identifying parcels: exposure varies within parcel, counted at the twelve-month window. Priced: a significant discount at two or more windows, with adjacent windows agreeing in sign. Suggestive: a significant discount at only one window, marginally significant at adjacent windows. Precise nulls: no window shows a significant discount, and the 95 percent intervals rule out a one-percent discount in at least nine of twelve windows; for larceny, motor vehicle theft, non-domestic assault, bad checks, and family offenses they also rule out a discount as large as burglary’s (0.24 percent), for the last two because their estimates are positive. Inconclusive: no window shows a significant discount, but the intervals cannot rule out a one-percent discount in most windows. Kidnapping, embezzlement, extortion, child pornography, and other offenses have too few incidents to estimate.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Fraud, forgery, and bad checks impose direct monetary losses, yet none carries a detectable discount in the pooled estimates (Table OA3); embezzlement has too few incidents to estimate. Bad checks instead carry a positive and significant estimate (1.09, s.e. 0.53), positive at five percent in ten of twelve windows (Appendix Table A4). Splitting each offense’s incidents by recorded location (residential, street, and commercial) leaves the picture unchanged (Table OA13). Bad checks have no detectable effect in any location category, and forgery’s only significant estimate is positive and occurs at commercial addresses. Fraud is associated with a residential discount of 0.40 percent (s.e. 0.15) that survives the post-sale placebo. This estimate is one of sixty-six location-specific

comparisons, does not survive a Bonferroni correction, and appears only at the longest exposure windows, so I treat it as suggestive.

Spatial decay. For burglary and narcotics, the estimated discount falls sharply beyond the near ring. Setting the near ring to each successive annulus while holding the far ring at 0.20–0.30 miles, the doorstep discount largely collapses by the 0.05–0.10 annulus and stays near zero through 0.15–0.20, the ring just inside the far control, with the rate of decay varying by offense across the intervening rings (Appendix Table A2). Two offenses show discounts beyond the doorstep. Burglary’s discount remains significant when the near disk is widened to 0–0.10 miles (-0.14 , s.e. 0.07 ; significant in 5 of 12 windows, with no significant post-sale placebo), but the 0.05–0.10 annulus alone is not significant, so the wider-disk estimate reflects the doorstep effect averaged over a larger area. Homicide is significant in the 0.10–0.15 mile annulus in 7 of 12 windows (months three to nine; -1.16 , s.e. 0.64 , at its ten-month central window), with no significant post-sale placebo, but not in the 0.05–0.10 annulus, so its effect extends a few blocks without a smooth decline in distance (Figure OF9). Crime at 0.20–0.30 carries no doorstep effect, so the far ring is untreated. Because $\delta = \beta_{\text{near}} - \beta_{\text{far}}$, the far-ring effect enters the design as a normalization, and any residual far-ring discount would make the estimate understate rather than overstate the doorstep effect.

Spline estimates. A tensor-product spline in distance and lag, estimated as a fixed-effects signal regression following Korolev (2026), traces the same decay (Figure 2): for the dense property offenses, burglary and narcotics, the effect is significantly negative at the doorstep and gone by the adjacent-block annulus; vandalism is a precise near-zero, consistent with its small per-incident discount (Table 2); and homicide’s profile is imprecise: its band is an order of magnitude wider and includes zero at the doorstep, since homicide is too sparse to support a stable two-dimensional surface (Online Appendix J). In the lag direction, the doorstep effect is significantly negative over months 2.5–5.5 for burglary and 2.5–4.5 for vandalism and declines only slowly for narcotics.

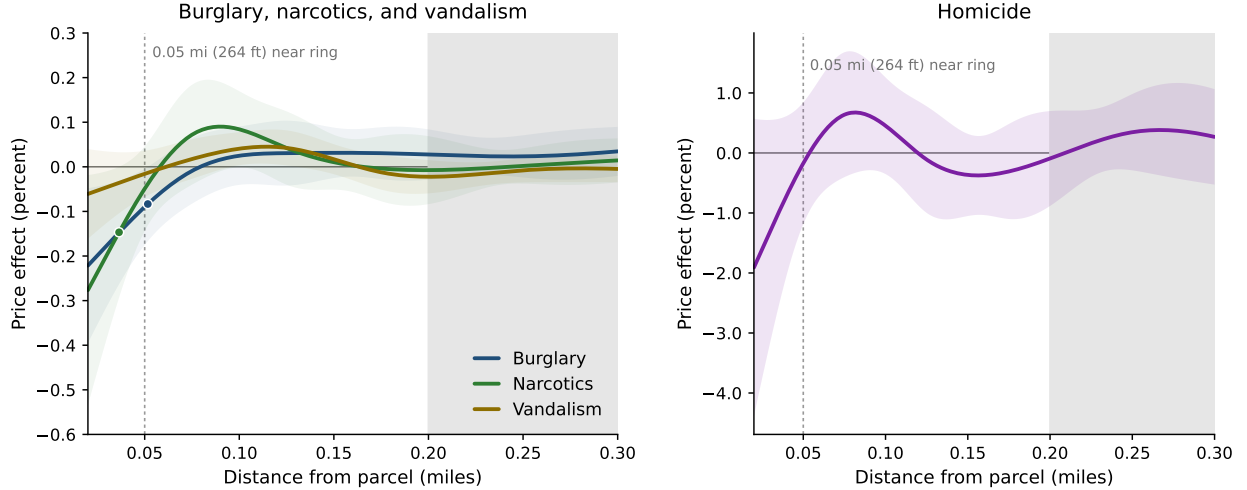


Figure 2: Price response by distance from the parcel. Distance profiles from a tensor-product spline in distance and lag, estimated as a fixed-effects signal regression in `mgcv` following Korolev (2026), fit on 0–0.30 miles and centered on their mean, averaged over the one-to-twelve-month windows; MLS close-date sample, 2007–2025, with 95 percent cluster-robust bands. *Left*: burglary, narcotics, and vandalism. *Right*: homicide, whose band is wider because incidents are rare and includes zero at the doorstep. The dotted line marks the 0.05-mile near-ring edge; the shaded band is the 0.20–0.30 mile comparison ring. The profile locates where the response is negative and where it is flat; its level is not the near–far discount in Table 2. Estimation details in Online Appendix J.

Time horizon of the effects. Estimated jointly, exposure two to six months before the sale and exposure eight to eleven months before carry different weights across offenses. Vandalism loads on the recent block and not on the late one, and the difference is significant ($p = 0.008$); narcotics loads on the late block, with the difference significant at ten percent ($p = 0.09$); for homicide and burglary the point estimates load late and early respectively, but the two blocks are not statistically distinguishable ($p = 0.12$ and 0.20 ; Figure OF2 and Table OA7).

The design removes most of the raw price gap near crime, and the remaining structure matters in a way that differs by offense. Table OA5 estimates the near–far contrast on the repeat-sales sample with progressively more structure. Tract-by-month effects alone reduce the raw per-incident homicide gap from about thirteen percent to about two, so most of the naive association is neighborhood and period. For the property offenses, adding observed characteristics and then parcel fixed effects shrinks the discount further, from about 0.7 to 0.2 percent for burglary and from 1.2 to 0.2 for vandalism, so a cross-sectional hedonic on these data overstates the property-crime discounts. Homicide moves the other way, from 2.3 to 3.0 percent, so the same hedonic understates it. Parcel fixed effects without tract-by-month effects give the wrong sign for homicide and double the narcotics discount, which is why the design needs both: the parcel effect removes what is permanent about the property and the tract-by-month effect removes what is moving in the neighborhood.

Wider near rings produce additional discounts that also appear in post-sale placebos. At 0–0.10 miles several offenses that show no detected discount at the doorstep produce apparent sale-price discounts for which post-sale crime also predicts the price: domestic assault, for instance, discounts by about a quarter percent in the 2009–2013 foreclosure-crisis era, while crime occurring

after the sale predicts the same discount in seven of twelve windows. That is neighborhood-level contamination, not capitalization, and the tighter doorstep ring excludes it.

5.2 Mechanisms

Sixteen of twenty-two offenses carry no detectable discount, and two more (disorderly conduct and sex offenses) are significant only at a single long-horizon window. Section 2 gives three conditions for an incident to be priced; Section 5.2.1 varies observability within an offense, Section 5.2.2 varies what the incident harms, and Section 5.2.3 asks whether discounts track recurrence among the priced offenses.

5.2.1 Observability: What reaches the buyer

Prediction 1 says that, holding the offense fixed, incidents more likely to be observed carry larger discounts. Split by incident location, homicide and narcotics load on street incidents and burglary and vandalism on residential ones (Table OA12); the within-residential split below holds relevance fixed and varies only what is visible from the street.

Within-Property Visibility Within residential-address incidents I split the dwelling interior (inside a residence or apartment) from the frontage (garage, porch, yard, driveway, entryway, and the porch/hallway and common-area codes, which make up 39 percent of frontage-coded incidents across all offenses; Table OA15), each geocoded to the block face (Table 3). Burglary frontage is -0.74 (s.e. 0.33), significant in 7 of 12 windows; burglary interior is -0.14 (s.e. 0.16), not significant at any window; equality is not rejected at this window ($p = 0.109$) and is rejected at five percent only at twelve months ($p = 0.030$), the frontage subsample rests on about 45,000 incidents, and the post-sale placebo is null (-0.20 , $p = 0.45$). Vandalism is the reverse: frontage -0.14 (s.e. 0.33), not significant at the central window, and interior -0.73 (s.e. 0.25), significant in 9 of 12 windows (Table OA18).⁹ Larceny is null on both sides ($p = 0.686$), the check that the split is not manufacturing differences (Table OA18).

Splitting incidents instead by where they were recorded (residential, street, or commercial) gives a related pattern (Tables OA12 and OA14). Vandalism at residential addresses is associated with a discount (-0.56 , significant at one percent), while vandalism in public space is not (-0.01), and the two differ significantly in seven consecutive windows, although residential and street vandalism forecast future doorstep crime at similar rates (Table OA16). Homicide’s discount is concentrated in incidents on the street, although its residential and street estimates are not statistically distinguishable.

⁹CPD masks each incident to the hundred-block, so an interior record cannot be separated between the sold parcel and a neighbor’s. For burglary this matters little, since the loss is the stolen contents and any entry damage is minor; for vandalism an interior record is damage to the property itself, which lowers its value as repair cost rather than through information. I report the vandalism cells but exclude vandalism from this within-offense observability comparison.

Table 3: Two Within-Offense Tests: Observability and Relevance

	Exposure window (months before sale)											
	1	2	3	4	5	6	7	8	9	10	11	12
<i>Panel A. Observability: burglary, residential incidents</i>												
Frontage (visible)	-0.37 (0.77)	-0.45 (0.59)	-0.54 (0.49)	-0.68 (0.46)	-0.54 (0.41)	-0.74** (0.36)	-0.76** (0.35)	-0.74** (0.33)	-0.62** (0.29)	-0.70** (0.27)	-0.71*** (0.25)	-0.73*** (0.24)
Interior (hidden)	-0.17 (0.47)	-0.27 (0.30)	-0.12 (0.26)	-0.11 (0.22)	-0.20 (0.20)	-0.30 (0.19)	-0.22 (0.18)	-0.14 (0.16)	-0.18 (0.15)	-0.19 (0.13)	-0.20 (0.13)	-0.13 (0.13)
Frontage = interior (p)	0.824	0.796	0.457	0.258	0.462	0.299	0.183	0.109	0.182	0.098	0.081	0.030
<i>Panel B. Relevance: vandalism, frontage incidents, by target</i>												
Damage to property	0.85 (1.12)	1.06 (0.83)	0.80 (0.63)	0.22 (0.58)	0.03 (0.51)	-0.49 (0.47)	-0.89** (0.45)	-0.95** (0.43)	-0.91** (0.41)	-1.03*** (0.39)	-0.92** (0.36)	-0.89*** (0.34)
Damage to vehicle	0.70 (0.93)	0.89 (0.67)	0.50 (0.56)	0.33 (0.53)	0.20 (0.48)	0.21 (0.45)	0.05 (0.42)	-0.07 (0.40)	-0.13 (0.40)	-0.08 (0.38)	-0.09 (0.37)	0.02 (0.34)
Property = vehicle (p)	0.916	0.874	0.724	0.896	0.818	0.298	0.143	0.142	0.168	0.078	0.102	0.057

Notes. δ as in Table 2, at each window; standard errors below. Panel A splits residential burglary into frontage (garage, porch, yard, driveway, entryway, porch/hallway and common areas; Table OA15) and interior incidents. Panel B splits frontage vandalism by target. Last row of each panel: Wald p for equal effects. Post-sale placebos for the frontage and interior components are significant in at most one of twelve windows; the pooled residential burglary coefficient, by contrast, is matched window-for-window by post-sale crime (Online Appendix C); both fixed-effect designs in Online Appendix D.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Homicide is the most-covered offense in the data, about 27.7 news stories per incident in Media-Cloud,¹⁰ roughly fifteen times the next-highest offense, and the largest article volume in GDELT.¹¹ This is coverage of the crime *category*, not of the particular nearby incident: narcotics, for instance, is capitalized despite far lower category coverage.

When the near count is entered as indicators for one, two, three, and four-or-more incidents (the main design with the near count binned), a single incident is not significantly negative at any of the twelve windows for any priced offense. The four-or-more estimate is -1.85 (s.e. 0.76) at burglary’s central window and significant in 8 of 12 windows; vandalism’s is -0.92 (s.e. 0.45), significant in 3 of 12; narcotics is significant at one window and homicide at none, and the four-or-more cell holds 8,040 listings for narcotics and 14 for homicide (Table OA6).

5.2.2 Relevance: What the incident threatens

Prediction 1 also concerns whether the harm attaches to the location. When each offense is classified as anchored to the address or travelling with the household (Section 2, Table OA19), all four priced offenses are anchored to the location, and of the eleven offenses that travel with the household only sex offenses show a suggestive discount (Fisher exact test, $p = 0.09$). Because offenses differ in severity, frequency, and visibility as well, this pattern is consistent with relevance but does not isolate it. Within vandalism, restricting to frontage incidents (visibility held roughly fixed) and splitting by target, damage to property is significant in 6 of 12 windows but not at the central window (-0.49 , s.e. 0.47), while damage to a vehicle is significant in none (0.21, s.e. 0.45), although the two are not distinguishable at five percent at any window; both post-sale placebos are null, and the two sub-codes forecast their own recurrence at the same small rate, so the contrast holds visibility and the recurrence benchmark fixed and varies what is harmed (Online Appendix D).¹²

5.2.3 Recurrence: Whether prices track the forecast

Prediction 2 asks whether discounts track how well an offense forecasts its own recurrence. Nearby crime is informative about future crime: in a forecasting regression without fixed effects, far-ring history significantly forecasts future doorstep crime, at five percent, for twenty-one of the twenty-two offenses at the six-month horizon (all but stolen property) and for all twenty-two at twelve months, priced or not, though the far ring explains little of what the doorstep’s own past leaves unexplained (partial R^2 at most 0.09 for any offense at either horizon). The priced offenses are not those the far ring forecasts best: fraud, weapons, domestic assault, and motor-vehicle theft forecast doorstep crime at least as well as burglary and vandalism, and homicide, which carries the

¹⁰Media Cloud, an open-source platform for large-scale quantitative news analysis (<https://mediacloud.org>); the measure is the count of news articles mentioning an offense category over the sample period, divided by the number of recorded incidents in that category.

¹¹The GDELT Project, a global open database that monitors and indexes news coverage worldwide (<https://www.gdeltproject.org>).

¹²Because the police records mask each incident to the hundred-block, a frontage damage-to-property record can be the sold parcel itself, so this contrast carries an own-property component.

largest discount, is among the least forecastable (Table A5). If the discount tracked that predictive content, offenses that better predict their own recurrence would carry larger discounts. They do not. Narcotics is the most self-predicting offense (own-recurrence coefficient $\rho = 0.40$, Table OA10) yet carries a discount far below homicide’s; homicide’s own-recurrence coefficient is small and negative ($\rho = -0.026$, s.e. 0.012) and it carries the largest. Homicide’s weak recurrence is consistent with Carrillo and Yezer (2021), who find homicide far less persistent from year to year than other violent crime. The recurrence ratio $|\delta|/\rho$ is in window-specific units: ρ is a within-parcel persistence coefficient estimated over the offense’s exposure horizon (roughly six to eleven months). The ratio is about 0.7 for narcotics, 3.6 for burglary, and 1.8 for vandalism, and is undefined for homicide, whose recurrence coefficient is negative. The burglary and narcotics ratios are individually bounded away from zero (Fieller 95%), and vandalism’s interval only narrowly excludes zero, and the intervals are wide and overlapping, so the ordering across offenses is suggestive rather than identified. A joint test does not reject equal ratios across burglary, narcotics, and vandalism ($p = 0.25$). Using ρ without parcel fixed effects, which overstates recurrence, the ratios are about 0.4 for narcotics, 0.7 for burglary, and 0.4 for vandalism, so burglary remains the largest but narcotics and vandalism are no longer distinguishable. Discounts do not rise with measured recurrence. That recurrence is real but small and offense-specific is corroborated by Caetano and Maheshri (2018), who find that several Dallas offenses (robbery, burglary, auto theft, and light crime) raise their own future incidence by roughly five to fifteen percent, fading within about a month, with no escalation across offense types.

5.3 Price Formation

Prediction 3 asks where in the transaction the discount appears. I trace the effect of crime through the housing transaction. Boggess et al. (2013) find that higher neighborhood crime is followed by higher rates of housing transactions, with sales inhibited only where crime and vacancy are already high, but their data cannot say which margin adjusts. I examine asking prices, time on market, withdrawal, and the sale-to-list ratio, which lets me distinguish effects incorporated when the seller sets the asking price from those that emerge later in the transaction.

Table 4: The Pricing Chain: From Ask to Sold

Offense	Ask %	Sale-to-list %	Price cut pp	Days on market %	Withdrawal pp	Close %
Homicide	0.38 (0.57)	0.53 (0.45)	8.24**† (3.30)	14.54** (6.85)	2.24** (0.96)	-3.02** (1.38)
Narcotics	0.21 (0.17)	-0.16** (0.07)	0.43 (0.35)	-0.78 (0.98)	0.04 (0.04)	-0.29** (0.14)
Burglary	-0.15 (0.12)	-0.01 (0.05)	-0.33 (0.33)	0.23 (1.25)	-0.10 (0.10)	-0.24** (0.12)
Vandalism	0.05 (0.07)	0.00 (0.03)	-0.12 (0.21)	-0.10 (0.69)	0.00 (0.05)	-0.17** (0.08)
Disorderly conduct†	-0.43** (0.21)	-0.28** (0.12)	2.55*** (0.95)	-1.97 (3.86)	0.10 (0.23)	-0.65** (0.29)
Parcel FE	Yes	Yes	Yes	×era	Yes	Yes
Tract × month FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes. Near-far estimates as in Table 2, pooled 2007–2025. Columns: log asking price, log sale-to-list ratio, mid-listing price cut (percentage points), log days on market, withdrawal (percentage points), and log closing price. Closing price at each offense’s central window (homicide 10, burglary 8, narcotics 10, vandalism 6 months); listing margins at the median significant window. Days on market uses parcel × era fixed effects; withdrawal uses all listings.

†Spell-level comparison of the final to the original ask, which cannot order the crime and the cut; an event-level hazard on listing change histories (from 2015) does not detect a during-listing cut (Table OA23).

†Disorderly conduct is a suggestive offense (Table 2), shown because it is the only offense capitalized at the ask.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 follows each priced offense along the full sequence from the opening ask to the closed sale (ask, sale-to-list ratio, mid-listing price cut, days on market, withdrawal, and close), pooled over the sample, with the era-resolved version in Online Appendix E. The ask shows no significant response for any priced offense; disorderly conduct, a suggestive offense, is the only one capitalized at the ask; for burglary the ask is imprecise rather than unresponsive (-0.15 , s.e. 0.12 , a confidence interval that includes the -0.24 close discount), so I cannot rule out a modest ask response. The rest of the adjustment happens after listing, and which margin carries it depends on the offense. For homicide, three clocks must be kept apart. On the list clock, pre-listing homicide does not detectably move the average asking price but is associated with longer time on market and a higher probability of withdrawal: the listing is harder to sell. On the close clock, pre-sale homicide lowers the closing price by roughly three percent, a discount that appears at the close rather than in the posted price; it is significant at the ten- and eleven-month exposure windows, while the month-one estimate is larger in magnitude but too imprecise to distinguish. The sale-to-list margin within the close is imprecisely estimated. Crime *during* the live listing, by contrast, does not move the mid-listing price cut: in an event-level discrete-time hazard that dates each cut from the listing change history and measures crime in the preceding month, the prior-month homicide effect on the probability of a cut is 0.24 percentage points (not significant), with a null lead-crime placebo (Table OA23). An earlier spell-level comparison of the final to the original ask had suggested a

during-listing markdown, but it cannot order the crime and the cut, and the event-level hazard does not detect it. The hazard uses listing change histories that begin in 2015, so it covers the recovery and COVID years, whereas Table 4 pools 2007–2025. Narcotics closes below list through the sale-to-list ratio. Disorderly conduct is capitalized at the ask. Burglary and vandalism carry closing-price discounts, but the pooled listing estimates do not establish where these discounts emerge (Online Appendix E).

Agent experience Agents’ incentives and experience affect what reaches the price (Rutherford et al., 2005; Hendel et al., 2009; Han and Hong, 2011; Barwick and Pathak, 2015; Gilbukh and Goldsmith-Pinkham, 2024). The listing agent sets the ask and the buyer’s agent negotiates the close, so I estimate the listing-agent experience gradient on the ask and the buyer-agent gradient on the closing price (Table 5). The homicide listing-agent gradient on the ask is negative and significant in seven of twelve windows; for homicide, a buyer-agent gradient on the closing price is present within agent (-1.93 per standard deviation, significant in six of twelve windows; Appendix Table A6), while narcotics loads on the ask (significant in seven of twelve windows). For these offenses the discount at mean experience is not statistically significant on the smaller agent-matched sample; the response is concentrated among experienced agents. The listing-ask gradient is negative for homicide, narcotics, and vandalism in nearly every exposure window and under both parcel and parcel-plus-agent fixed effects (Appendix Table A6); for narcotics it attenuates once first-time-in-tract agents are allowed to differ, and for vandalism it is concentrated at that first-listing margin (Table OA24). With total experience (all prior listings) and its interaction with exposure in the same equation, homicide’s local gradient remains for both the listing agent (-0.79 , s.e. 0.35) and the buyer’s agent (-1.99 , s.e. 0.86), while its total-experience gradient is not significant on either side; for narcotics, the listing-agent gradient loads on total experience (-0.21 , s.e. 0.05, significant in all twelve windows), and the local gradient is zero (Table OA25). Because agents are not randomly assigned to listings, these estimates are associations rather than causal effects.

Table 5: Agent Local Experience: Ask (Listing) and Close (Buyer)

Offense	Gradient, min (s.e.)	max (s.e.)	Sig. windows
<i>Panel A. Listing agent, asking price (ln list)</i>			
Homicide	−0.97*** (0.34)	−0.03 (0.52)	7/12
Burglary	−0.02 (0.03)	0.06 (0.05)	0/12
Narcotics	−0.11** (0.05)	−0.05 (0.08)	7/12
Vandalism	−0.06** (0.02)	−0.02 (0.07)	3/12
<i>Panel B. Buyer agent, closing price (ln close)</i>			
Homicide	−6.27** (2.60)	−0.95 (0.88)	6/12
Burglary	0.02 (0.15)	0.53** (0.27)	0/12
Narcotics	−0.06 (0.47)	0.38* (0.22)	0/12
Vandalism	−0.04 (0.14)	0.14** (0.06)	0/12

Notes. Near–far effect interacted with agent experience, $E = \ln(1 + \text{prior same-agent same-tract listings})$. Panel A: listing agent, asking price. Panel B: buyer’s agent, closing price. Parcel, tract $\times$ month, and agent fixed effects; clustered by tract. Gradient: change in the near–far effect per log unit of E (s.d. 1.20) (negative: experienced agents price crime more), shown as its range across the twelve windows and the number of significantly negative windows. Appendix Table A6 reports the same specification per standard deviation of E at each offense’s central window.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Two offenses that move no sale price, robbery and weapons, nonetheless raise the probability of withdrawal (Table OA21), estimated at the 0.10-mile ring and, for robbery, in the crisis era only. Sex offenses similarly lengthen time on market by about four percent, one to two days against the median of forty-two, without a robust sale-price discount (sex offenses are significant at a single window).

The quantity margin Price is not the only margin on which the market can absorb a local shock. I therefore estimate the effect of each offense on the probability that a parcel is listed and on the probability that it sells, and test the difference (Table OA26). Ten of the twenty-two offenses show a positive gap: the listing response exceeds the completed-sale response. The gap is a listing-minus-sale difference, not a direct withdrawal estimate. The gap set contains every offense that is priced, and the offenses that are null on price but show a gap, most reliably domestic assault and weapons offenses, show a quantity response without a detectable price response. Theft and motor vehicle theft are null on both margins.

5.4 Rental Prices

Lease listings run through the identical near–far design (Table 6). The lease sample is MLS-listed units, which are formal rentals and skew toward condominiums (Table A1), so the rental estimates describe that segment and do not speak to informal or lower-income rental markets. Three offenses lower the asking rent. Burglary lowers the asking rent under parcel fixed effects, and sex offenses and liquor violations lower it under parcel-by-era fixed effects but not under parcel fixed effects; all three

survive the post-date placebo. Narcotics leaves rents unchanged under parcel fixed effects, where its post-date placebo is clean, though the placebo fails under parcel-by-era fixed effects; homicide is estimated too imprecisely in the lease sample to sign. Both instead appear in the leasing frictions of Panel B, where narcotics raises the probability a listing returns to market (robust across all twenty-two specifications, with a null future-crime placebo; Figure OF3). The full offense battery is in Table OA27.

The rental estimates describe what landlords post: an asking rent is posted rather than negotiated, and the lease sample is smaller and skews toward condominiums, so sampling error is larger and informal and lower-income rentals are absent (Table A1).

Table 6: The Rental Market

Offense	PIN			PIN×era		
	Min (%)	Max (%)	Sig. (plac.)	Min (%)	Max (%)	Sig. (plac.)
<i>A. Asking rent (percent)</i>						
<i>Priced (asking rent lower; identifying FE in note)</i>						
Burglary	−0.34*** (0.12)	−0.07 (0.06)	6/12 (0)	−0.30** (0.12)	−0.04 (0.07)	1/12 (0)
Sex offenses	−0.29* (0.15)	−0.21* (0.13)	1/12 (0)	−0.32 (0.27)	−0.16 (0.14)	7/12 (0)
Liquor law	−0.23 (0.26)	−0.01 (0.13)	0/12 (0)	−0.30*** (0.07)	0.09 (0.49)	4/12 (0)
<i>Precise nulls (rent effect near zero, tightly estimated)</i>						
Narcotics [†]	0.01 (0.06)	0.10 (0.08)	0/12 (0)	0.02 (0.17)	0.14 (0.09)	0/12 (4)
Vandalism	−0.06 (0.06)	0.05 (0.07)	0/12 (0)	−0.03 (0.04)	0.09 (0.07)	0/12 (1)
Prostitution	−0.16 (0.10)	0.00 (0.17)	0/12 (1)	−0.33*** (0.12)	−0.03 (0.14)	1/12 (0)
<i>Imprecise / underpowered</i>						
Homicide	−0.55 (0.61)	1.03 (1.20)	0/12 (0)	−0.59 (0.59)	1.16 (1.22)	0/12 (0)
<i>B. Leasing frictions</i>						
Narcotics, back-on-market (pp)	0.18** (0.08)	0.97*** (0.34)	11/12 (0)	0.27*** (0.08)	1.28*** (0.40)	11/12 (0)
Homicide, time on market (%)	1.01 (4.65)	12.19 (11.00)	0/12 (0)	1.54 (3.98)	19.63* (11.70)	4/12 (0)
Burglary, withdrawn (pp) [†]	−1.06** (0.46)	−0.02 (0.16)	3/12 (0)	−1.12** (0.53)	−0.08 (0.15)	5/12 (2)

Notes. Near–far estimates on lease listings, 2007–2025, tract × listing-month fixed effects, clustered by tract (118,849 listings with parcel fixed effects; 98,813 with parcel × era). For each specification: range across the twelve windows and the number significant at 5%; in parentheses, windows with a significant post-listing placebo; †, placebo fails in two or more. Panel A: log asking rent. Panel B: return to market and withdrawal (percentage points), time on market (percent).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The rental market has the same agent structure. Interacting the near–far lease effect with the letting agent’s local experience (Table OA28), experienced agents lease narcotics-exposed units faster (days on market fall by about 0.6 percent per standard deviation of experience) and list them slightly lower, while the average rental discount stays small. Tenant-agent experience is unrelated to asking rents or days on market in any window.

5.5 Heterogeneity

The capitalization of crime varies across macroeconomic eras (Table 7). Burglary and vandalism price during the 2009–2013 foreclosure crisis and fade in the recovery, though the difference across eras is not precisely estimated; homicide prices in the 2014–2025 recovery and is null in the crisis; narcotics is negative in each era but roughly flat and significant only in the pooled estimate (crisis

−0.33, s.e. 0.23; recovery −0.52, s.e. 0.31). The era differences are not formally tested. The crisis estimates for rare offenses rest on few within-crisis repeat sales and are imprecise, as their standard errors show.

Table 7: Crime Capitalization by Era

Offense	Identifying parcels	Pooled 2007–25	Crisis 2009–13	Recovery 2014–25
<i>Priced</i>				
Homicide	4,950	−3.02** (1.38)	−0.07 (4.73)	−4.12*** (1.47)
Narcotics	49,513	−0.29** (0.14)	−0.33 (0.23)	−0.52* (0.31)
Burglary	76,323	−0.24** (0.12)	−0.49* (0.25)	−0.18 (0.14)
Vandalism	88,765	−0.17** (0.08)	−0.54** (0.25)	−0.11 (0.10)
<i>Suggestive</i>				
Disorderly conduct	19,550	−0.65** (0.29)	−0.87* (0.51)	−0.59* (0.35)
Sex offenses	36,393	−0.47** (0.23)	−0.20 (0.72)	−0.36 (0.23)
<i>Precise nulls</i>				
Larceny/theft	105,610	0.02 (0.01)	0.07 (0.06)	0.02** (0.01)
Motor vehicle theft	70,464	0.09 (0.12)	0.00 (0.38)	0.08 (0.12)
Assault (non-dom.)	89,931	−0.03 (0.04)	−0.07 (0.10)	−0.04 (0.05)
Family (non-violent)	12,632	1.49** (0.60)	0.99 (1.08)	2.05** (0.86)
Assault (domestic)	77,543	−0.03 (0.12)	0.18 (0.28)	−0.02 (0.14)
Trespass	9,395	−0.09 (0.38)	−0.49 (0.93)	0.05 (0.39)
Robbery	46,664	0.02 (0.18)	−0.58 (0.45)	0.06 (0.19)
Liquor law	3,935	0.61 (0.43)	0.55 (0.93)	0.23 (0.39)
Parcel FE		Yes	Yes	Yes
Tract × year-month FE		Yes	Yes	Yes
Crime × era interaction		No	Yes	Yes

Notes. δ as in Table 2. Crisis (2009–2013) and recovery (2014–2025) effects come from an era-interacted model with 2007–2008 as the base; the pooled column is the full-sample effect and need not lie between them. Each offense is shown at its own central identifying window (the window differs across offenses, for example trespass at one month), held fixed across the three columns; identifying parcels (exposure varies within parcel) are counted at that window, whereas Table 2 counts them at twelve months. The crisis and recovery columns report per-era near-far levels, not deviations from the pooled column. Era differences are not formally tested.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The discounts also vary with neighborhood socioeconomic status. Interacting the near-far term

with predetermined tract characteristics (standardized over the sale sample, so $z = 0$ is the average sale), all four priced discounts are larger in lower-income and less-educated neighborhoods, and the burglary and narcotics discounts are also larger in higher-poverty tracts (Table OA29). The homicide discount shrinks by about 2.28 percentage points per standard deviation of tract income (largest in the lowest-income tracts and near zero a standard deviation above the mean); burglary, narcotics, and vandalism show the same gradient at smaller magnitudes. The socioeconomic moderators are strongly correlated and point the same way, so they are best read as a single income gradient rather than as separate findings; the full grid across all offenses and moderators is in Table OA29. The same gradient appears for tract share Black, but net of the tract’s predetermined crime level it is marginal for homicide and burglary and significant for narcotics and vandalism; only narcotics also shows a significant gradient in dollars, and share Hispanic shows no gradient at five percent for the priced offenses (Table OA29). A racial-segregation (divergence) index sharpens the pattern. Vandalism is the exception: its discount is larger in tracts with a higher share Black and in more segregated tracts (about -0.60 percent per standard deviation of the index), each net of the tract’s crime level, consistent with visible disorder being priced more heavily where neighborhood stigma is already high. The composition gradient is otherwise largely the income gradient, and the design does not say why narcotics and vandalism stand out.

5.6 Robustness

Reverse Causality and Mean Reversion Prices could drive crime rather than the reverse, or repeat-sales properties transacting at temporarily depressed prices could appreciate mechanically. I test both using a post-date placebo that replaces pre-transaction crime with crime occurring after the transaction, which cannot affect a price already set. The priced offenses pass this placebo at their headline windows: homicide, narcotics, and vandalism show no significant post-sale placebo at their headline windows, and burglary’s headline eight-month estimate likewise shows no detectable post-sale placebo effect, but post-sale crime is significant at two and six months, and six months is where burglary’s pre-sale estimate is largest (-0.34 , s.e. 0.14), so the placebo does not fully separate burglary’s peak discount from post-sale crime.

Sample Selection Because the repeat-sales estimator observes only properties that transact, crime could affect which properties enter the estimation sample. I test this directly in the full parcel universe (the risk set is all residential parcels, whether or not they ever sell) using a monthly model of sale propensity. Nearby crime has no significant effect on the monthly probability of sale: the estimated effect is statistically indistinguishable from zero ($p \approx 0.4$) and negligible in magnitude against a sale rate of about 0.3 percent per month, from the 3.8 percent annual rate in Table OA26. The annual quantity margin (Table OA26) does show small listing-minus-sale gaps for several offenses, so crime moves the *listing* decision more than the completed sale; the monthly sale-propensity test here bears specifically on selection into the repeat-sales estimation sample. Entering an arm’s-length-versus-distressed interaction, the estimated discounts are if anything

larger in magnitude among distressed sales, not muted; but the distressed subsample is thin near crime, so those estimates are imprecise and the two rarely differ significantly (Table OA30). The only significant difference is narcotics, whose distressed discount is the larger of the two. Because a sale is classified as distressed on the basis of its REO, short-sale, or foreclosure status, which is itself associated with lower prices, this split describes where the discount appears rather than testing a forced-seller against a demand-side mechanism (Campbell et al., 2011; Ellen et al., 2013; Anenberg and Kung, 2014); homicide’s distressed estimate (-8.99 , s.e. 4.74) is larger but imprecise, and its difference from the arm’s-length estimate is not significant ($p = 0.13$). The collider-free check on a foreclosure-driven confound is instead the predetermined vacancy control described below, which leaves the discounts essentially unchanged. A complementary Heckman selection correction tells the same story: although the inverse-Mills term is significant, the correction leaves the crime coefficients largely unchanged (Online Appendix L). Overall, these diagnostics provide little evidence that the threats to identification account for the estimated discounts.

Reporting Endogeneity Recorded incidents combine underlying crime with the probability that crime is reported or detected. Several features limit this concern. The largest discount occurs for homicide, an offense comparatively less vulnerable to underreporting, while heavily reported offenses produce no detected discount, robbery among them, and theft is a precise zero; differential reporting therefore does not naturally explain the cross-offense pattern. Identification also comes from a near–far contrast within tract $\times$ month, limiting confounding from broader differences in reporting propensity.

Selection into Zero Exposure Properties with no nearby crime could differ systematically from exposed properties along unobserved dimensions. I test for such selection using the mass-point dummy test of Caetano (2015); Caetano et al. (2021) for the three priced offenses with sufficient support at zero: narcotics, vandalism, and burglary. For narcotics and vandalism a large discontinuity in the raw data disappears under the headline parcel and tract $\times$ month fixed effects, and explicitly controlling for the boundary leaves the narcotics discount unchanged and, if anything, strengthens the vandalism discount. For burglary the boundary term is itself significant (-0.978 , s.e. 0.317), so the linear discount is identified partly from the contrast between exposed and unexposed parcels. The discontinuity appears only once parcel fixed effects are included, so it does not reflect burglars targeting more valuable houses, a concern Gibbons (2004) raises for London. The jump does not reflect spillover from the adjacent ring: controlling for burglary at 0.05–0.10 miles leaves it unchanged (-0.99 , s.e. 0.32). Because the test jointly detects failures of exogeneity, functional form, and parallel trends, its rejection does not establish the direction of any bias, and I treat the linear count for burglary as a maintained approximation; adding the boundary dummy moves δ from -0.244 (s.e. 0.117) to -0.552 (s.e. 0.168) (Online Appendix M). At a 0–0.10 mile near ring, where 27 percent of sales have no nearby burglary and the boundary test does not reject (-0.06 , s.e. 0.27), burglary’s discount remains significant (-0.14 , s.e. 0.07 ; Appendix Table A2), with no significant post-sale placebo at any window.

Predetermined Neighborhood Controls The estimated discounts are not explained by pre-existing neighborhood investment or decline. I add five neighborhood controls, building permits (investment), vacant buildings (disinvestment), business licenses (commercial activity), tree canopy (amenity), and total 311 service requests (disorder), each entered as its own near-far difference alongside crime. Each varies across a parcel’s sales and therefore retains identifying variation with parcel fixed effects. All five are predetermined relative to the twelve-month crime window: tree canopy is measured two years before the sale, while permits, vacant buildings, business licenses, and 311 requests are measured as cumulative stocks of events recorded more than twelve months before the close. Predetermined measures are used deliberately: contemporaneous neighborhood conditions may themselves respond to local crime, so conditioning on them could amount to controlling for an outcome of the exposure. These controls instead capture pre-existing neighborhood conditions rather than potential consequences of the crime itself (Appendix Table A7).

Point estimates change little (Appendix Table A7): a nearby homicide still lowers sale prices by about three percent, and the property-crime estimates move by hundredths of a percent. Precision falls for two offenses: with all five controls together, burglary (-0.22 , s.e. 0.12) and narcotics (-0.26 , s.e. 0.15) are significant only at ten percent. The priced effects therefore do not appear to reflect the measured physical or economic conditions of the block that are correlated with local crime.

Spatial Standard Errors The baseline standard errors are clustered by census tract (Cameron and Miller, 2015), allowing arbitrary correlation among parcels within a tract but not across tract boundaries. To allow for spatial correlation that crosses those boundaries, I also report Conley (1999) standard errors using a Bartlett kernel with cutoffs of 0.5 and 1.0 km. The 1.0 km cutoff exceeds twice the 0.30-mile outer radius used to construct crime exposure, so parcels with overlapping exposure areas can have correlated residuals. Tract clustering and Conley inference make different assumptions: the former allows unrestricted within-tract correlation regardless of distance, while the latter allows correlation across tracts but downweights it with distance and sets it to zero beyond the cutoff. This check reports both the era-salient and the pooled discount for each priced offense. Conley standard errors at 0.5 and 1.0 km, reported in Table OA31, are below the clustered standard errors for every priced offense; inference and the significance stars use the clustered ones. Clustering instead by Chicago’s 77 community areas, a coarser grouping, leaves homicide and burglary significant at five percent ($p = 0.03$ and 0.04); vandalism is significant at ten percent ($p = 0.06$), and narcotics is not ($p = 0.12$) (Table OA31).

Functional Form How nearby crime enters the regression matters differently across offenses (Table OA33). Homicide, rare enough that a single incident is effectively the treatment, is significant whether exposure enters as a raw count, a presence indicator, or in inverse hyperbolic sine (-3.0 to -3.6 percent throughout). The property-crime discounts, by contrast, are significant at five percent only under the raw count: narcotics is significant at ten percent under the presence indicator (-0.78 , s.e. 0.43) and the inverse hyperbolic sine (-0.60 , s.e. 0.31), and burglary and vandalism are not significant under either. A presence dummy weights one incident like four, and the inverse

hyperbolic sine compresses high counts, so both dilute an effect that the dose-response (Table OA6) shows is driven by accumulated exposure rather than an isolated incident. I therefore adopt the raw count as a maintained approximation for the property offenses; homicide is significant under every form.

Across all twenty-two offenses (Table OA33), the choice of functional form does not manufacture the priced discounts; some offenses that do not price in the headline count specification do turn significant under the inverse-hyperbolic-sine or presence transforms, which rescale or redefine exposure. Two offenses outside the priced set are significant under all three forms: disorderly conduct, negative (-0.65 , -1.00 , and -0.81 , the last at ten percent), and bad checks, positive (0.66 , 1.06 , and 1.07). Homicide is significant at five percent under every form and narcotics at ten percent under the presence and inverse-hyperbolic-sine forms; burglary and vandalism are significant under the raw count maintained above but not under the presence or inverse-hyperbolic-sine transforms.

Cross-Crime Confounding A discount attributed to one offense could in principle be capitalizing a spatially correlated neighbor. Homicide, burglary, and narcotics are near-orthogonal to the other offenses in the near ring, and entering all twenty-two offenses’ near-far terms jointly, at a common twelve-month window, leaves each of their discounts nearly identical to its separate estimate (Online Appendix K; Table OA34). Vandalism is the exception: it correlates moderately with the high-volume nuisance offenses it co-occurs with, and its joint estimate is -0.02 . At the common twelve-month window, however, vandalism’s separate estimate is already small and insignificant (-0.05 , s.e. 0.06), so the joint specification does not test its six-month discount, and part of that discount may reflect the co-occurring offenses. Disorderly conduct, by contrast, retains a discount in the joint specification (-0.58 , significant at five percent).

Specification curves: sale prices Figure 3 reports the sale-price specification curve for vandalism (narcotics, homicide, and burglary appear in Appendix Figures A1–A3); each curve sorts the doorstep near-far discount $\delta = (\beta_{\text{near}} - \beta_{\text{far}}) \times 100$ across 63 specifications. The grid varies the exposure window from six to twelve months, the fixed-effect design (parcel + tract $\times$ month, or parcel $\times$ era + tract $\times$ month), the comparison ring (0.20 – 0.30 , 0.25 – 0.35 , 0.30 – 0.40 mile), the functional form (linear count, presence indicator, inverse hyperbolic sine), and the predetermined controls (none, all five jointly, or one at a time). The window begins at six months because shorter windows fall inside the listing and under-contract period, where measured pre-sale exposure is sparse and partly contemporaneous with the sale itself. Nearly all specifications are negative for the four priced offenses; the exceptions are the presence-indicator estimates for burglary (0.54 , s.e. 0.98) and vandalism (about 0.46), both imprecise. The discount is significant at five percent in 33 of 63 specifications for vandalism, 29 for narcotics, 26 for burglary, and 15 for homicide, whose point estimates range from about -3.6 percent to about zero with wide intervals. The post-sale placebo is significant in none of the 63 for homicide and vandalism, one for narcotics (the presence indicator at ten months), and four for burglary (the linear count at six months under each

of the three far rings, and the inverse hyperbolic sine at eight months).¹³ The full curve over all one-to-twelve-month exposure windows (103 specifications) is reported in Figures OF5–OF8.

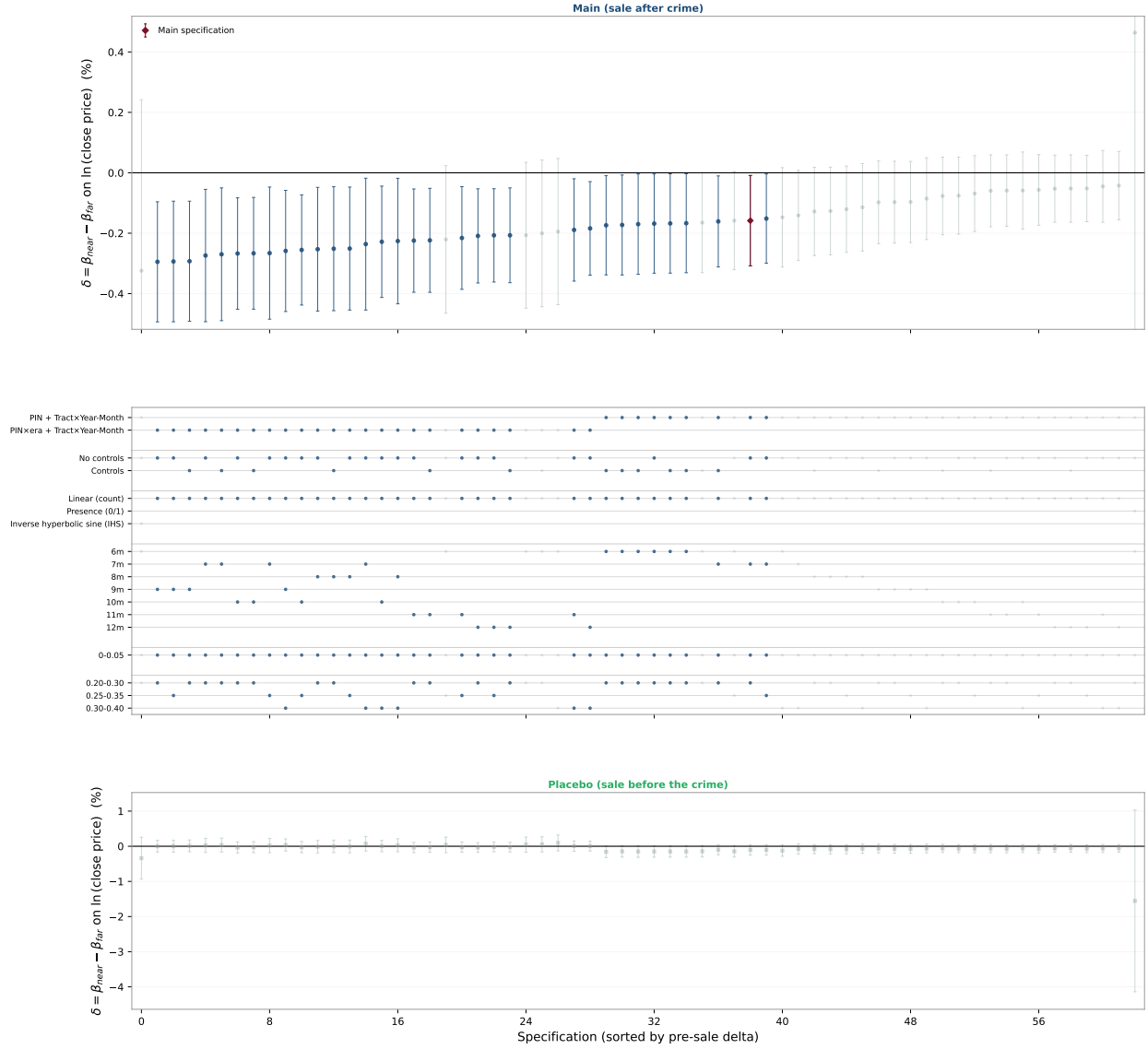


Figure 3: Vandalism: Sale-Price Discount Across Specifications

Notes. Common to this figure and Appendix Figures A1–A3; specifications as described in Section 5.6. Top panel: δ (%) with 95 percent intervals, sorted from most to least negative; navy marks estimates significant and negative at five percent, and maroon marks the main specification (Table 2). Middle panel: the specification behind each estimate. Bottom panel: the post-sale placebo, which replaces pre-sale crime with crime after the sale. All years pooled (2007–2025); standard errors clustered by tract. Vandalism: 33/63 significant at five percent; placebo 0/63.

A falsification test that moves the treatment band outward to 0.05–0.20 leaves narcotics, burglary, and vandalism significant in at most 1 of 48 specifications (Figure OF9), indicating effects concentrated at the doorstep. Homicide behaves differently. It is significant in 11 of 48 off-doorstep specifications, all in the 0.10–0.15 band, with a median estimate (−2.72) essentially identical to

¹³The specification curve shows how stable the estimates are across modeling choices. Because the specifications share data and most choices, they are not independent, so the number of significant estimates is not a formal test.

the doorstep (-2.78),¹⁴ while the intervening 0.05–0.10 band is null. Homicide is therefore robust at the doorstep but, unlike the other three offenses, not localized to it. This is consistent with Section 5.2.1, where homicide is priced through public incidents rather than the dwelling, a pattern that is neighborhood-scale rather than confined to the doorstep. Because homicide reaches beyond the doorstep, it likely raises exposure in the 0.20–0.30 mile comparison ring as well; since $\delta = \beta_{\text{near}} - \beta_{\text{far}}$ nets that ring out, the reported homicide discount is if anything a lower bound on the doorstep effect rather than an overstatement. Significantly negative placebo estimates off the doorstep (3 of 48 for narcotics, 5 of 48 for burglary and vandalism) modestly exceed the 5 percent expected by chance and are concentrated in the wider bands, consistent with the contamination this falsification test is designed to detect rather than with a doorstep effect.

Specification curves: rents Across the twenty-two specifications that span the eleven exposure windows from two to twelve months and the two fixed-effect designs, the narcotics estimate on the probability a lease listing returns to the market is positive and significant in all twenty-two, with no significant placebo (Figure OF3). The sex-offense estimate on the asking rent is significant in eight of twenty-two, with no significant placebo (Figure OF4).

6 Discussion

6.1 Why most offenses are not priced

Sixteen of twenty-two offenses have no detectable sale-price discount, and two more are significant only at a single window. Neither frequency nor severity alone orders the discounts. Homicide is among the rarest offenses and carries the largest discount. Larceny, the most common offense, and motor vehicle theft are precise nulls, as are robbery and both assaults, among the most serious (Tables OA8 and OA9 split assault and the other offenses by domestic status). The framework expresses the discount per incident as $\kappa_k = w_k \times S_k \times b_k(S_k)$: the value of avoiding one expected future incident at the address, the probability that the buyer learns of the incident, and the resulting change in expected future incidents. Relevance concerns whether exposure of this kind at this address matters to the buyer; updating concerns whether an incident changes expected exposure. A buyer may expect more car thefts on the block after learning of one without valuing the house less.

The forecasting regressions in Section 5.2.3 measure predictive relationships in the crime data, not how buyers form expectations. They provide a benchmark for predictive content, although buyers may have different information or evaluate exposure over a different horizon. Price responses do not follow this benchmark in two respects: crime on surrounding blocks predicts subsequent crime at the property but carries no detectable discount, and the offenses with the strongest predictive

¹⁴The -2.78 and -2.72 medians are taken over the significant estimates in the band-varying close specification grid (window $\times$ fixed effects $\times$ far ring), the object common to both bands; the larger doorstep significant count (15) additionally varies the predetermined controls and the functional form.

relationships are not those that are priced. These patterns are consistent with buyers responding to incidents they learn about rather than fully incorporating the predictive information in recorded crime, as models of salience and costly attention would suggest (Bordalo et al., 2012; Gabaix, 2014).

A zero discount can arise from any of the framework’s three terms being zero; an undetected discount can also reflect limited precision. The transaction data do not distinguish these possibilities. Each comparison examines a different implication of the framework without holding the other components fixed. The vandalism comparison helps distinguish relevance from measured recurrence: damage to property is associated with discounts in more exposure windows than damage to vehicles, although the two sub-codes have similar measured recurrence. This weakens an explanation based on that recurrence measure, but does not establish that buyers perceive the two incidents as equally predictive. The comparison holds the frontage location category fixed, although the two targets may still differ in how visible the damage is. Related evidence across decisions suggests that relevance depends on the decision being made: Fe and Sanfelice (2022) find that incidents inside residences have no detectable effect on visits to Chicago businesses, while public incidents do.

Within burglary, frontage incidents are associated with discounts, while incidents inside the dwelling have no detectable effect; the difference is statistically significant only at the longest exposure windows (Table 3). This pattern is consistent with more observable incidents having larger price effects. It may also help explain differences from Gibbons (2004), who finds that criminal damage lowers London property prices while burglary does not, and interprets vandalism as a visible signal of disorder. His burglary measure combines incidents within 250 meters, potentially pooling locations with different observability and extending beyond the immediate block where the effects here are concentrated.

None of these comparisons measures information arrival directly. The data therefore cannot determine whether buyers never learn of an incident, regard it as irrelevant, or leave their expectations unchanged. Changes in what online real estate platforms display about nearby crime, discussed in Section 7, would test this channel directly.

6.2 Why asking prices respond little

The framework holds the seller’s reservation value fixed when an incident occurs. The absence of a detectable average asking-price response to homicide, alongside a roughly three-percent closing-price discount, is consistent with this assumption, although an asking price does not directly measure a reservation value (Table 4). The framework does not explain this rigidity. Two possible accounts are worth separating.

The first is reference dependence. Sellers may anchor on their purchase price or a previous valuation and resist reducing the ask. Genesove and Mayer (2001) show that owners facing prospective nominal losses set higher asking prices, sell more slowly, and obtain higher prices conditional on selling.

The second concerns how asking prices are set. Comparative market analyses use recent nearby sales, which may predate an incident and therefore contain little information about its effect.

Reliance on these comparables could leave the initial ask unchanged even if the eventual sale price adjusts. The more negative asking-price responses to homicide and narcotics among more experienced listing agents are consistent with agents incorporating information beyond recent sales. They do not, however, establish that reliance on comparables explains the average asking-price response.

These accounts are not mutually exclusive, and the data do not separate them.

A related question is whether discounts reflect changes in sellers' bargaining positions rather than buyers' valuations. For burglary, narcotics, and vandalism, discounts do not differ detectably between soft and tight markets ($p > 0.4$); the homicide estimates are too imprecise to support this comparison. These results provide little evidence of variation with measured market tightness, but do not rule out bargaining effects. Online Appendix N also reports contract-timing and distressed-sale tests, which have limited statistical power.

The rental data record asking rents rather than negotiated rents, so a muted asking-rent response need not imply that tenants place little value on safety.

6.3 Who carries the information

The agent results examine how information may reach prices through market intermediaries. For homicide, the closing-price response is more negative when the buyer's agent has more experience in the same census tract. This gradient remains after controlling for total transactions, while total experience has no detectable gradient. The listing-agent gradient also remains at the central window after controlling for total experience, although it is significant in only one of twelve windows. Together, these associations are consistent with local knowledge contributing to the closing-price response.

For narcotics, the listing-agent gradient in asking prices reflects total experience rather than experience in the tract. This distinction points toward general pricing expertise as a possible explanation, rather than knowledge specific to the block. More broadly, these results relate to evidence that agents' information advantages can affect transaction outcomes (Levitt and Syverson, 2008). Because agents are not randomly assigned to listings, the gradients do not identify causal effects of experience or knowledge.

News coverage is another possible source of information. Homicide receives the most coverage and has the largest discount, which is consistent with wider information reach contributing to its price effect. At the incident level, however, Klimova and Lee (2014) find that murders with more media coverage have no additional effect on Sydney house prices. Across offenses, however, the measure of press attention does not explain which are priced (Section 5.2.1). This leaves room for information to travel through local contacts and agents, although the comparison does not establish the relative importance of these sources.

6.4 Welfare and policy

The estimates measure equilibrium price responses among completed transactions. Completed sales may also involve a selected group of buyers. If nearby crime deters the households most averse to it, the buyers who remain may value avoiding crime less than those who withdraw from consideration. The estimates therefore need not represent average household willingness to pay. Translating them into willingness to pay also requires assumptions about information and price formation. A discount may reflect lower buyer valuations, a weaker seller bargaining position, or both.

If the negotiated price splits the gains from trade, $P = V_s + \theta(V_b - V_s)$ with seller share $\theta \in [0, 1]$, an incident that lowers the buyer's and seller's valuations by $\Delta_b \geq \Delta_s \geq 0$ lowers the price by $\theta \Delta_b + (1 - \theta) \Delta_s$, which lies between $\theta \Delta_b$ and Δ_b ; Equation (3) is the case $\theta = 1$. A decline in the buyer's valuation larger than the initial gains from trade eliminates agreement, one possible interpretation of increased withdrawals, although a withdrawn listing does not identify why agreement failed.

The transaction data do not separate these adjustments, so a price response cannot be read directly as willingness to pay to avoid crime. Transaction relevance may also operate through resale: purchasing a home exposes the owner to future buyers' assessments as well as to the value of current occupancy.

Two implications follow. First, sale prices alone omit changes in time on market and withdrawal. These outcomes reveal additional transaction responses without establishing their net welfare cost. Longer marketing periods may entail carrying costs and additional search effort. Withdrawal may represent a forgone move, postponement, relisting, or a switch to renting; its welfare implications depend on what follows.

Second, ranking offenses by price effects need not rank them by social cost. The price estimates do not align with the victim-cost estimates of McCollister et al. (2010), and some offenses affect listing outcomes without producing detectable sale-price discounts. If buyers do not learn about harmful incidents, price-based evaluations may miss benefits of crime reduction. The analysis does not quantify those omitted benefits.

Information disclosure presents a related possibility. If observability contributes to capitalization, crime maps or disclosure rules may affect property values without changing underlying crime. This is an implication of the proposed mechanism, which the design does not isolate, rather than an estimated policy effect.

6.5 Scope

For offenses recorded through enforcement rather than victim complaints, price responses may reflect policing as well as underlying crime. Police killings have documented effects on nearby minority students' academic performance and psychological well-being (Ang, 2021), and stop-and-frisk surges lower minority youths' test scores (Legewie and Fagan, 2019). Enforcement intensity also affects recorded offense counts (Levitt, 1998). If buyers regard concentrated drug or vice enforcement as a disamenity, estimates based on enforcement-recorded incidents may capture that

response. The design does not separate it from the response to crime. Among the four priced offenses, narcotics is the category most directly affected by this concern; homicide, burglary, and vandalism are not primarily generated by proactive enforcement.

The parameter is the marginal effect of one additional incident within roughly one block, on average: a regression-weighted average of the per-incident slope across counts, which provides a common unit for comparisons across settings. Its geographic meaning nevertheless depends on local conditions. The near and far rings are calibrated to Chicago’s street geometry; applying the design elsewhere would require reconsidering the distances rather than importing them unchanged.

Whether the magnitudes transfer is an empirical question. Responses may depend on urban density, street structure, and how readily prospective buyers learn about local incidents. They also vary with neighborhood income and across eras within Chicago (Section 5.5). Applying an estimate elsewhere therefore requires attention to the local income distribution and housing-market conditions, as well as the offense and geographic scale.

7 Conclusion

Safety is a leading consideration for homebuyers, yet most types of crime do not measurably lower house prices. Using repeat sales of the same property and comparing crime on its block with crime a few blocks away in the same neighborhood and month, I find sale-price discounts for only four of twenty-two offense categories. Neither severity nor measured recurrence alone explains which offenses are priced.

Three findings help interpret this pattern. First, price effects are more localized than the predictive relationships in the crime data: crime a few blocks away predicts subsequent crime at the property, yet detectable price effects are confined to the immediate block for property offenses and extend only a few blocks for homicide. Second, the estimates vary with where an incident occurs, a dimension the framework links to observability: frontage burglaries, which are more exposed to view, are associated with price discounts, while burglaries inside the dwelling have no detectable effect; the difference between the two grows with the exposure window and is significant at twelve months. Third, the response extends beyond the sale price: homicide lengthens time on market and raises the probability that a listing is withdrawn. Asking prices respond more negatively to some nearby crimes when listing agents are more experienced.

These patterns are consistent with price responses depending on whether buyers learn of an incident, regard it as relevant to the property, and revise their expectations of future exposure. An absent discount may therefore reflect a lack of awareness, awareness without any change in expected exposure, or a discount too small to detect. Under the proposed conceptual framework, observed price discounts need not equal willingness to pay to avoid crime. Property values need not rank offenses by the harm they impose, and price-based valuations of crime reduction may understate the benefits of reducing crime that buyers rarely observe.

Future research could test how access to crime information affects the price effects of crime.

Online real estate platforms began showing neighborhood crime maps in 2011, when Trulia launched them for fifty metropolitan areas, and removed crime data from home listings in late 2021 and 2022 (Realtor.com in December 2021, Trulia in 2022). If capitalization depends on whether buyers learn of an incident, the doorstep discount should rise when crime becomes visible on home listings and fall when it is removed, with no change in crime itself, and the change should be largest for offenses the maps displayed. Zhang et al. (2022) propose a related design using England’s public crime maps. More generally, the framework could guide research on other local risks, such as flooding, to distinguish changes in underlying harm from changes in what market participants know.

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A Appendix

This appendix reports seven supporting tables and the sale-price specification curves for narcotics, homicide, and burglary. Unless a note says otherwise, each reports $\delta = (\beta_{\text{near}} - \beta_{\text{far}}) \times 100$, the percent sale-price change per incident within 0–0.05 miles of the property against 0.20–0.30 miles, with parcel and tract $\times$ year-month fixed effects and standard errors clustered by census tract; $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. All other supporting material is in the Online Appendix.

Table A1: Summary Statistics

	Mean	SD	p10	Median	p90	Obs.
<i>A. Sale listings</i> ($N = 530,395$)						
Close price (\$)	366,777	382,539	74,000	279,000	700,000	530,395
Log close price	12.44	0.92	11.21	12.54	13.46	530,395
Original list price (\$)	384,828	410,804	85,000	289,900	725,000	529,956
Days on market	96	147	5	42	248	529,995
Any list-price cut	0.68	0.47	0.00	1.00	1.00	530,395
Bedrooms	3.0	1.7	1.0	3.0	5.0	517,982
Bathrooms	2.1	1.1	1.0	2.0	3.0	530,395
Living area (sq. ft.)	1,175	1,115	0	1,100	2,483	454,018
Total rooms	7.1	3.2	4.0	6.0	11.0	517,753
Year built	1954	41	1902	1954	2007	453,343
Garage spaces	1.6	0.6	1.0	2.0	2.0	351,747
Fireplaces	0.9	0.7	0.0	1.0	2.0	206,694
<i>B. Lease listings</i> ($N = 191,155$)						
Monthly rent (\$)	2,208	1,086	1,250	1,950	3,400	191,155
Log monthly rent	7.61	0.41	7.13	7.58	8.13	191,155
Days on market	43	57	6	25	102	191,122
Bedrooms	1.8	1.0	1.0	2.0	3.0	191,031
Bathrooms	1.6	0.8	1.0	1.0	2.0	191,155
Living area (sq. ft.)	883	675	0	900	1,691	191,041
Total rooms	4.7	1.4	3.0	5.0	6.0	191,032
Year built	1972	37	1912	1982	2008	150,337

Notes: Chicago MLS single-family, condominium, and residential-income listings, 2007–2025, on parcels with a constant parcel count. Panel A: closed sales (\$10,000–\$10,000,000). Panel B: leases (monthly rent \$500–\$10,000). Any list-price cut: at least one reduction before sale.

Table A2: The Near–Far Discount Across Nine Ring Definitions

Near vs. far ring (miles)	Homicide	Burglary	Narcotics	Vandalism
0–0.05 vs. 0.20–0.30 (<i>headline</i>)	-3.02 (1.38)	-0.24 (0.12)	-0.29 (0.14)	-0.17 (0.08)
0–0.05 vs. 0.25–0.35	-2.75 (1.39)	-0.24 (0.12)	-0.32 (0.14)	-0.16 (0.08)
0–0.05 vs. 0.30–0.40	-2.66 (1.35)	-0.27 (0.12)	-0.32 (0.14)	-0.15 (0.08)
0–0.10 vs. 0.20–0.30	-0.42 (0.67)	-0.14 (0.07)	0.01 (0.06)	-0.06 (0.05)
0–0.15 vs. 0.20–0.30	-0.90 (0.54)	-0.05 (0.05)	0.05 (0.04)	0.01 (0.03)
0–0.20 vs. 0.20–0.30	-0.74 (0.46)	-0.03 (0.04)	0.02 (0.03)	-0.02 (0.03)
0.05–0.10 vs. 0.20–0.30	0.55 (0.78)	-0.09 (0.07)	0.06 (0.07)	-0.02 (0.05)
0.10–0.15 vs. 0.20–0.30	-1.16 (0.64)	0.04 (0.06)	0.06 (0.05)	0.07 (0.04)
0.15–0.20 vs. 0.20–0.30	-0.45 (0.58)	-0.00 (0.05)	-0.02 (0.04)	-0.05 (0.04)

Notes: Each row is one near/far ring pair: the headline (row 1), the far ring moved out (rows 2–3), the near disk widened (rows 4–6), and an outer annulus in place of the doorstep (rows 7–9). Windows: homicide 10, burglary 8, narcotics 10, vandalism 6 months. $N = 242,815$.

Table A3: Sale-Price Capitalization, All Twenty-Two Estimable Offenses

Offense	Identifying parcels	Sig. discount windows	Min δ (s.e.) [mo]	Max δ (s.e.) [mo]
<i>Priced</i>				
Homicide	5,701	2/12	-5.86* (3.35) [1]	-1.24 (1.43) [8]
Narcotics	53,844	5/12	-0.93 (0.75) [1]	-0.18 (0.19) [6]
Burglary	88,094	6/12	-0.34** (0.14) [6]	-0.18* (0.09) [12]
Vandalism	104,661	2/12	-0.17** (0.08) [6]	0.26 (0.22) [1]
<i>Suggestive</i>				
Disorderly conduct	19,550	1/12	-1.33* (0.78) [2]	-0.46 (0.54) [4]
Sex offenses	36,393	1/12	-0.47** (0.23) [12]	0.11 (0.35) [3]
<i>Precise nulls</i>				
Theft	115,548	0/12	0.01 (0.02) [12]	0.08*** (0.03) [1]
Motor vehicle theft	85,781	0/12	-0.02 (0.09) [12]	0.51* (0.30) [1]
Assault (non-dom.)	91,783	0/12	-0.05 (0.06) [7]	0.01 (0.11) [3]
Bad checks	8,919	0/12	0.66* (0.35) [11]	2.20** (1.03) [1]
Family (nonviol.)	15,928	0/12	1.00* (0.51) [12]	2.16*** (0.82) [5]
Fraud	67,030	0/12	-0.36 (0.23) [1]	-0.04 (0.09) [6]
Assault (domestic)	90,500	0/12	-0.19* (0.10) [12]	0.02 (0.15) [5]
Trespass	54,025	0/12	-0.15 (0.14) [6]	0.18 (0.22) [3]
Robbery	65,876	0/12	-0.06 (0.14) [9]	0.30 (0.42) [1]
Forgery	23,614	0/12	-0.07 (0.59) [1]	0.47 (0.45) [2]
Liquor	6,393	0/12	0.08 (1.20) [1]	1.34 (0.91) [2]
Prostitution	6,326	0/12	-1.00 (0.82) [1]	0.08 (0.12) [12]
<i>Inconclusive</i>				
Weapons	32,294	0/12	-0.62 (0.47) [5]	0.11 (0.58) [3]
Arson	6,116	0/12	-0.20 (1.06) [11]	1.27 (3.58) [1]
Gambling	3,125	0/12	-4.77 (4.33) [3]	1.99 (8.32) [1]
Stolen property	1,262	0/12	-2.23 (2.63) [2]	0.81 (1.20) [8]

Notes. All twenty-two estimable offenses under the specification of Table 2; kidnapping, embezzlement, extortion, child pornography, and other offenses have too few incidents to estimate and are excluded; columns as there; groups as in Table 2. Sig. discount windows counts windows with a significantly negative estimate at five percent, so positive significant estimates (bad checks, family offenses) count as zero.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Sale-Price Capitalization at All Twelve Exposure Windows

Offense	Exposure window (cumulative months)											
	1	2	3	4	5	6	7	8	9	10	11	12
<i>Priced</i>												
Homicide	-5.86*	-2.23	-1.45	-2.04	-1.28	-1.30	-1.59	-1.24	-2.54*	-3.02**	-3.15**	-2.49*
	(3.35)	(2.60)	(2.46)	(2.05)	(1.82)	(1.77)	(1.53)	(1.43)	(1.36)	(1.38)	(1.38)	(1.36)
Narcotics	-0.93	-0.37	-0.35	-0.32	-0.28	-0.18	-0.20	-0.38**	-0.31**	-0.29**	-0.35***	-0.33***
	(0.75)	(0.42)	(0.38)	(0.27)	(0.23)	(0.19)	(0.17)	(0.18)	(0.15)	(0.14)	(0.13)	(0.11)
Burglary	-0.20	-0.23	-0.20	-0.21	-0.25*	-0.34**	-0.30**	-0.24**	-0.22**	-0.22**	-0.22**	-0.18*
	(0.31)	(0.20)	(0.17)	(0.16)	(0.15)	(0.14)	(0.12)	(0.12)	(0.11)	(0.10)	(0.10)	(0.09)
Vandalism	0.26	0.13	-0.06	-0.11	-0.16*	-0.17**	-0.16**	-0.13*	-0.10	-0.08	-0.06	-0.05
	(0.22)	(0.15)	(0.12)	(0.10)	(0.09)	(0.08)	(0.08)	(0.07)	(0.07)	(0.07)	(0.06)	(0.06)
<i>Suggestive</i>												
Disorderly conduct	-1.22	-1.33*	-0.65	-0.46	-0.71	-0.66	-0.58	-0.50	-0.63*	-0.60*	-0.57*	-0.65**
	(1.03)	(0.78)	(0.63)	(0.54)	(0.48)	(0.44)	(0.38)	(0.35)	(0.33)	(0.32)	(0.29)	(0.29)
Sex offenses	-0.08	0.10	0.11	-0.02	-0.29	-0.31	-0.25	-0.20	-0.26	-0.39	-0.43*	-0.47**
	(0.64)	(0.42)	(0.35)	(0.32)	(0.31)	(0.27)	(0.26)	(0.25)	(0.25)	(0.25)	(0.24)	(0.23)
<i>Precise nulls</i>												
Larceny/theft	0.08***	0.04*	0.04*	0.03**	0.03*	0.02	0.02	0.02	0.01	0.02	0.01	0.01
	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)
Motor vehicle theft	0.51*	0.28	0.17	0.10	0.05	0.04	0.09	0.09	0.05	0.06	0.07	-0.02
	(0.30)	(0.22)	(0.18)	(0.16)	(0.14)	(0.12)	(0.12)	(0.11)	(0.10)	(0.10)	(0.09)	(0.09)
Assault (non-dom.)	-0.05	-0.02	0.01	-0.03	-0.04	-0.04	-0.05	-0.03	-0.02	-0.01	-0.03	-0.04
	(0.20)	(0.14)	(0.11)	(0.09)	(0.08)	(0.07)	(0.06)	(0.06)	(0.05)	(0.05)	(0.04)	(0.04)
Bad checks	2.20**	2.00***	1.36**	1.38**	1.09**	1.18***	1.08**	0.97**	0.77*	0.72**	0.66*	0.66**
	(1.03)	(0.74)	(0.61)	(0.57)	(0.53)	(0.45)	(0.43)	(0.41)	(0.39)	(0.37)	(0.35)	(0.33)
Family (non-violent)	1.99	1.07	1.16	1.68*	2.16***	1.77**	1.89***	1.68***	1.49**	1.28**	1.07*	1.00*
	(1.87)	(1.33)	(1.09)	(0.92)	(0.82)	(0.75)	(0.66)	(0.62)	(0.60)	(0.57)	(0.55)	(0.51)
Fraud	-0.36	-0.29*	-0.11	-0.07	-0.06	-0.04	-0.05	-0.05	-0.05	-0.08	-0.11	-0.11
	(0.23)	(0.17)	(0.12)	(0.10)	(0.10)	(0.09)	(0.10)	(0.09)	(0.09)	(0.08)	(0.08)	(0.08)
Assault (domestic)	0.02	0.00	-0.02	0.02	0.02	0.01	-0.03	-0.09	-0.17	-0.13	-0.18*	-0.19*
	(0.37)	(0.26)	(0.21)	(0.17)	(0.15)	(0.13)	(0.12)	(0.12)	(0.11)	(0.10)	(0.10)	(0.10)
Trespass	-0.09	0.16	0.18	0.00	-0.10	-0.15	-0.14	-0.12	-0.06	-0.05	-0.09	-0.14
	(0.38)	(0.29)	(0.22)	(0.18)	(0.15)	(0.14)	(0.13)	(0.12)	(0.11)	(0.10)	(0.10)	(0.10)
Robbery	0.30	0.15	0.09	0.08	0.04	0.02	-0.02	-0.03	-0.06	-0.01	0.01	-0.03
	(0.42)	(0.30)	(0.24)	(0.22)	(0.19)	(0.18)	(0.16)	(0.15)	(0.14)	(0.13)	(0.13)	(0.13)
Forgery	-0.07	0.47	0.12	0.15	0.05	0.38	0.44*	0.39*	0.30	0.30*	0.23	0.21
	(0.59)	(0.45)	(0.38)	(0.32)	(0.28)	(0.24)	(0.22)	(0.20)	(0.19)	(0.18)	(0.18)	(0.17)
Liquor law	0.08	1.34	0.71	0.72	0.47	0.39	0.61	0.65	0.64*	0.51	0.36	0.36
	(1.20)	(0.91)	(0.81)	(0.60)	(0.52)	(0.48)	(0.43)	(0.40)	(0.38)	(0.36)	(0.35)	(0.35)
Prostitution	-1.00	-0.43	-0.23	-0.23	-0.18	-0.28	-0.14	-0.04	0.01	0.01	0.05	0.08
	(0.82)	(0.51)	(0.48)	(0.39)	(0.35)	(0.33)	(0.30)	(0.27)	(0.22)	(0.18)	(0.15)	(0.12)
<i>Inconclusive</i>												
Weapons	-0.47	0.07	0.11	-0.51	-0.62	-0.39	-0.18	-0.12	-0.17	0.04	0.09	0.10
	(0.92)	(0.71)	(0.58)	(0.48)	(0.47)	(0.44)	(0.40)	(0.37)	(0.35)	(0.31)	(0.30)	(0.28)
Arson	1.27	0.32	0.27	0.10	-0.13	0.07	1.07	0.57	-0.15	-0.13	-0.20	0.07
	(3.58)	(2.75)	(2.33)	(2.01)	(1.78)	(1.63)	(1.49)	(1.31)	(1.22)	(1.15)	(1.06)	(1.03)
Gambling	1.99	1.69	-4.77	-1.39	-1.35	-0.76	-1.17	-3.04	-2.84	-2.91*	-1.79	-1.94
	(8.32)	(5.78)	(4.33)	(3.99)	(3.39)	(3.12)	(2.74)	(2.43)	(2.10)	(1.75)	(1.77)	(1.75)
Stolen property	-2.23	-2.23	-1.78	-2.02	-1.14	-0.67	0.26	0.81	0.08	0.19	-0.25	-0.43
	(4.06)	(2.63)	(2.18)	(1.95)	(1.83)	(1.75)	(1.59)	(1.20)	(1.06)	(1.02)	(1.05)	(1.05)

Notes. δ at every cumulative window from one to twelve months, under the specification of Table 2; offense groups as there. Covers the twenty-two estimable categories, including the white-collar offenses, prostitution, gambling, and stolen property. Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Rational Forecast: The Discount Does Not Track Forecastable Risk

Offense	0–6 months		0–12 months	
	Joint F	Partial R^2	Joint F	Partial R^2
Homicide [†]	127.3***	0.004	237.0***	0.009
Burglary [†]	1580.8***	0.047	1871.1***	0.062
Narcotics [†]	411.1***	0.038	251.5***	0.037
Vandalism [†]	711.6***	0.045	627.5***	0.054
Theft	6.8***	0.035	9.1***	0.017
Motor-vehicle theft	789.0***	0.047	721.1***	0.052
Robbery	357.2***	0.045	365.8***	0.060
Assault (non-dom.)	103.6***	0.030	99.3***	0.025
Assault (domestic)	696.5***	0.062	497.1***	0.058
Arson	80.3***	0.001	204.3***	0.002
Bad checks	27.4***	0.002	34.0***	0.005
Disorderly conduct	129.5***	0.012	132.0***	0.020
Family offenses	109.4***	0.002	148.8***	0.004
Forgery	82.3***	0.012	91.1***	0.018
Fraud	102.1***	0.087	75.4***	0.075
Gambling	170.2***	0.013	261.6***	0.029
Liquor	30.3***	0.001	34.5***	0.003
Prostitution	18.0***	0.007	10.9***	0.007
Sex offenses	75.8***	0.008	45.2***	0.014
Stolen property	3.2*	0.000	5.8**	0.000
Trespass	22.4***	0.019	20.2***	0.022
Weapons	251.0***	0.054	348.9***	0.067

Notes. Estimated without fixed effects. Outcome: crime within 0.05 miles after the sale. Predictor: crime 0.20–0.30 miles away before the sale, controlling for the doorstep’s own prior crime. Windows: 0–6 and 0–12 months. F tests that the far-ring term is zero; partial R^2 is the share of the remaining variation the far ring explains. A predictive benchmark, not a causal estimate.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. † priced offense.

Table A6: Agent Experience Gradient on Price

Offense	Discount (mean agent)	Experience gradient (per SD)		Sig. windows (noFE/FE)
		no agent FE	agent FE	
<i>A. Asking price (listing agent)</i>				
Narcotics	-0.01 (0.04)	-0.15*** (0.05)	-0.09** (0.04)	9/7
Vandalism	0.02 (0.03)	-0.11*** (0.03)	-0.07** (0.03)	8/3
Homicide	0.18 (0.59)	-0.82 (0.63)	-1.01** (0.49)	0/7
Burglary	-0.05 (0.10)	-0.04 (0.11)	0.02 (0.08)	0/0
<i>B. Closing price (buyer's agent)</i>				
Homicide	-0.94 (0.79)	-0.01 (1.70)	-1.93** (0.89)	0/6
Narcotics	-0.09 (0.12)	-0.52** (0.25)	0.13 (0.15)	3/0
Vandalism	-0.13 (0.11)	-0.04 (0.17)	0.10 (0.15)	0/0
Burglary	-0.07 (0.08)	0.25* (0.15)	0.12 (0.13)	0/0

Notes. Gradient per standard deviation of agent experience ($E = \ln(1 + \text{prior same-agent same-tract listings})$, s.d. 1.20), each offense at its central window. Panel A: listing agent, asking price. Panel B: buyer's agent, closing price. Discount: near-far effect at mean experience. Gradients are estimated without and with agent fixed effects; Sig. windows counts significant windows of twelve, without/with. Sample: listings with an agent identifier.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Priced Discounts with Predetermined Neighborhood Controls

	Homicide	Burglary	Narcotics	Vandalism
δ , baseline (no neighborhood control)	-3.02** (1.38)	-0.24** (0.12)	-0.29** (0.14)	-0.17** (0.08)
<i>Adding each predetermined control alone (near & far, stock to 12 months before close):</i>				
+ Building permits (investment)	-3.03** (1.38)	-0.24** (0.12)	-0.29** (0.14)	-0.17** (0.08)
+ Vacant buildings (disinvestment)	-3.06** (1.40)	-0.23** (0.12)	-0.25* (0.15)	-0.17* (0.08)
+ Business licenses (commercial)	-2.95** (1.39)	-0.24** (0.12)	-0.30** (0.14)	-0.17** (0.08)
+ Tree canopy (amenity)	-3.02** (1.38)	-0.24** (0.12)	-0.29** (0.14)	-0.17** (0.08)
+ 311 service requests, all types (disorder)	-3.00** (1.38)	-0.24** (0.12)	-0.30** (0.14)	-0.17** (0.08)
δ , all five controls together	-2.97** (1.40)	-0.22* (0.12)	-0.26* (0.15)	-0.17** (0.08)

Notes. δ at each offense's central window. Row 1 is the baseline; the middle rows add one neighborhood control at a time; the last row adds all five. Each control is a near-far cumulative stock of events recorded more than twelve months before the close (tree canopy, two years before the sale); 311 data begin in 2011 and change composition in 2019 (Section 3).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

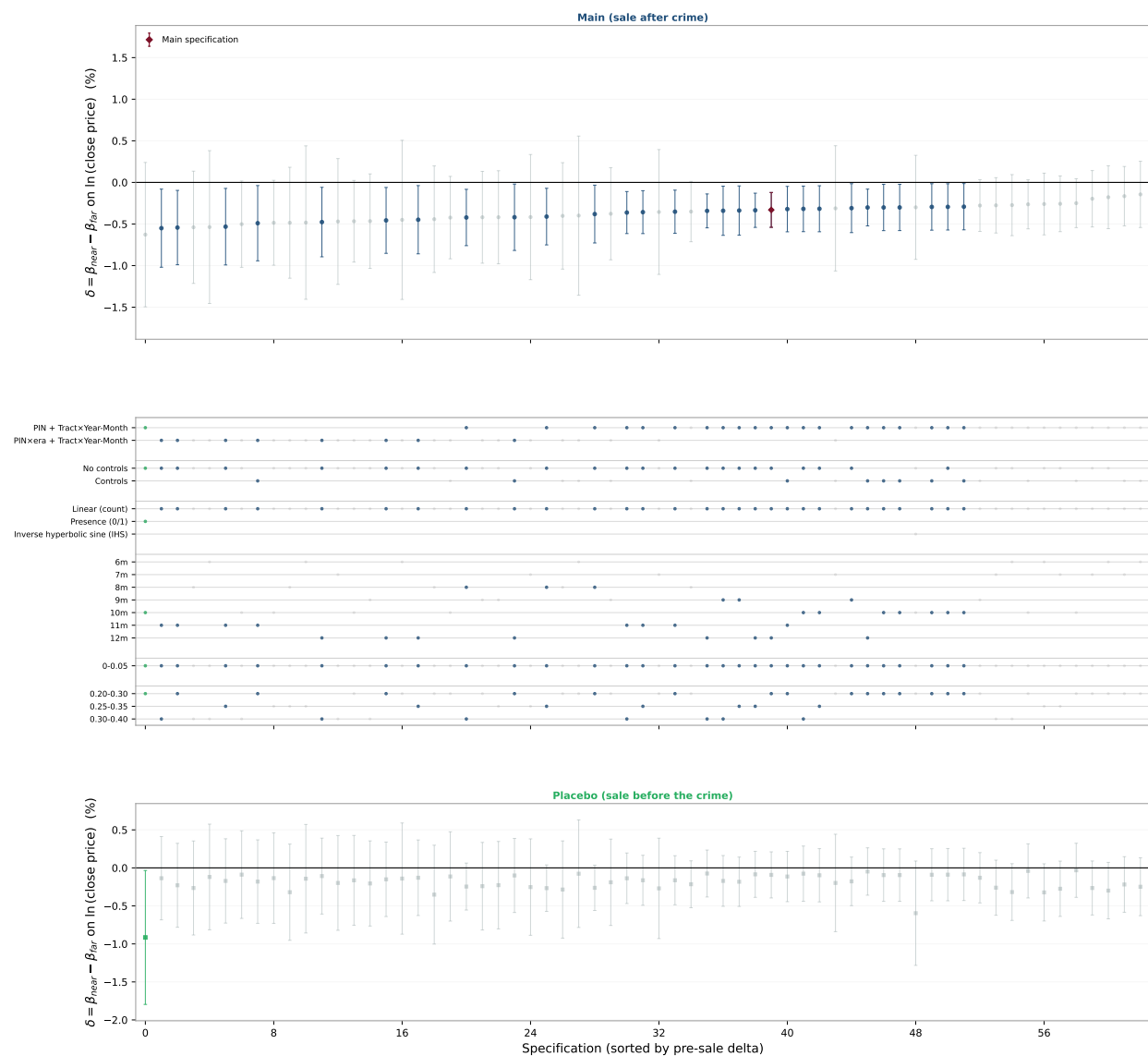


Figure A1: Narcotics: Sale-Price Discount Across Specifications

Notes. As in Figure 3, for narcotics. Main: 29/63 significant at five percent; placebo 1/63 (the presence indicator at ten months).

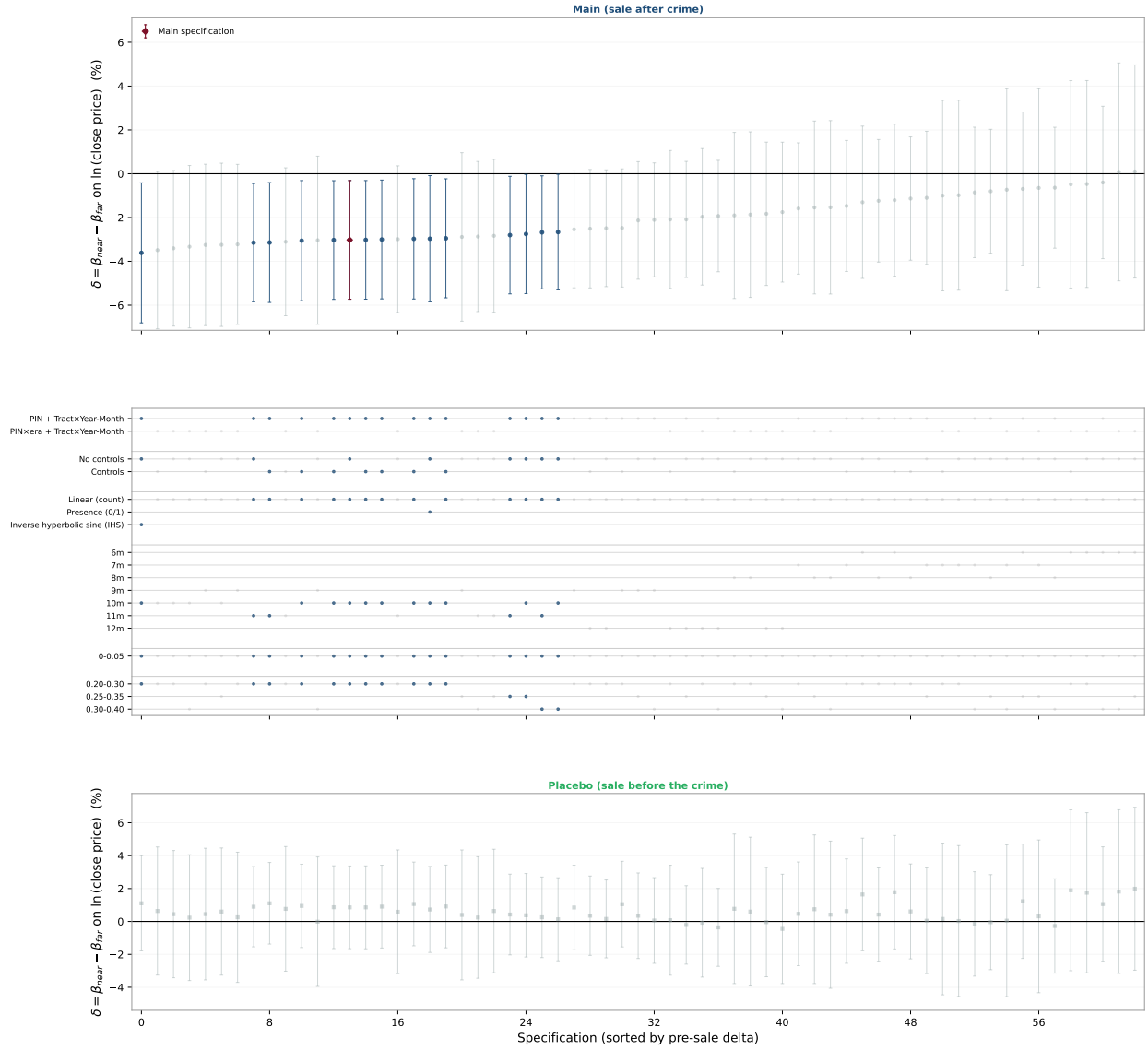


Figure A2: Homicide: Sale-Price Discount Across Specifications

Notes. As in Figure 3, for homicide. Main: 15/63 significant at 5%; placebo: 0/63. Homicide's intervals are an order of magnitude wider because incidents are rare, and the significant specifications cluster at the ten-to-eleven-month window; point estimates range from about -3.6 percent to about zero.

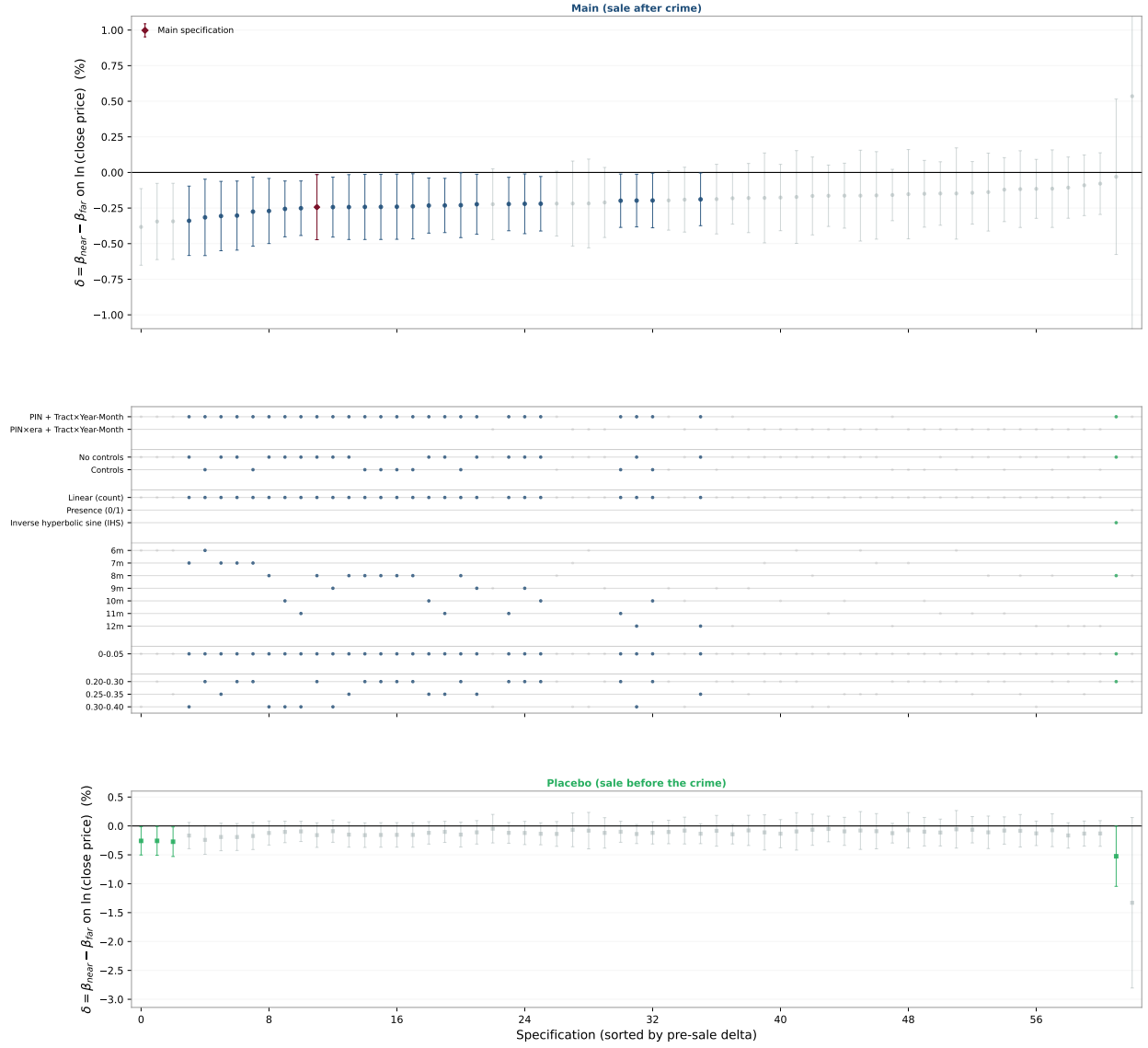


Figure A3: Burglary: Sale-Price Discount Across Specifications

Notes. As in Figure 3, for burglary. Main: 26/63 significant at 5%, all in the discount direction; placebo: 4/63 (the linear count at six months under each of the three far rings, and the inverse hyperbolic sine at eight months).

Supplementary Material

See the *Online Appendix*.