

# Online Appendix

## When Does Crime Enter House Prices? Evidence from Chicago

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## A Data and Measurement

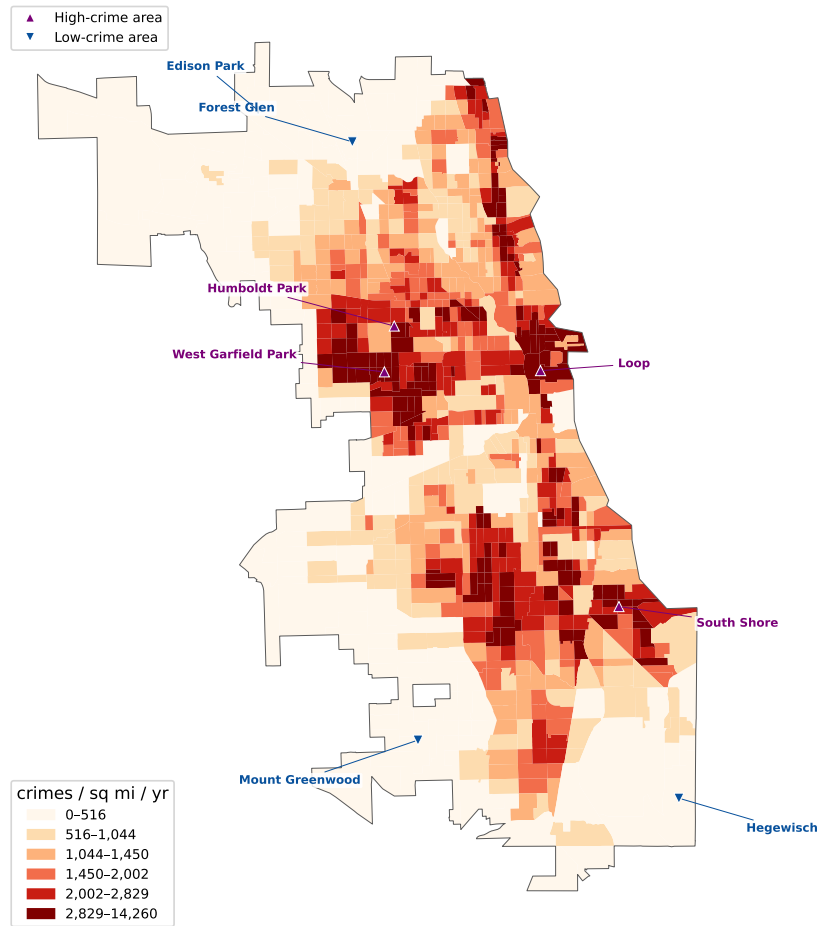


Figure OF1: Density of reported crime incidents, Chicago, 2007–2025.

*Notes:* Census-tract density of all reported Chicago Police Department incidents, 2007–2025, in incidents per square mile per year, binned into sextiles. Darker shades denote higher crime density.

Table OA1: Sample Construction

|                                                | Transactions | Parcels |
|------------------------------------------------|--------------|---------|
| MLS sale listings (Chicago, SF/CO/RI)          | 1,243,680    |         |
| sold (closed transaction)                      | 562,770      |         |
| closed 2007–2025                               | 546,494      |         |
| price \$10,000–\$10,000,000                    | 543,535      | 341,896 |
| constant parcel count (analysis sample)        | 530,395      | 337,461 |
| of which repeat-sale parcels ( $\geq 2$ )      | 329,800      | 136,866 |
| identified by parcel + tract $\times$ month FE | 242,815      |         |

*Notes:* Chicago Multiple Listing Service closed transactions in single-family, condominium, and residential-income properties. Within-property identification comes from the 136,866 parcels that transact at least twice. Adding tract $\times$ month fixed effects to the parcel fixed effects drops transactions that are the only sale in their tract $\times$ month cell (and the singletons this cascade creates), leaving the 242,815 transactions that identify the two-way fixed-effect model. Crime-specific specifications are smaller still (the near–far regressor also requires within-parcel variation in exposure), and every sale-price regression uses the 242,815-sale repeat-sale estimating sample unless a table states otherwise.

Table OA2: Listings With vs Without Nearby Crime

|                       | 3-month window               |                                | 6-month window               |                                |
|-----------------------|------------------------------|--------------------------------|------------------------------|--------------------------------|
|                       | No crime<br>( $N = 97,348$ ) | Any crime<br>( $N = 433,017$ ) | No crime<br>( $N = 47,022$ ) | Any crime<br>( $N = 483,343$ ) |
| Close price (\$)      | 381,096                      | 363,512                        | 370,137                      | 366,409                        |
| Log close price       | 12.54                        | 12.42                          | 12.53                        | 12.43                          |
| Days on market        | 86                           | 98                             | 83                           | 97                             |
| Any list-price cut    | 0.68                         | 0.67                           | 0.69                         | 0.67                           |
| Bedrooms              | 3.1                          | 3.0                            | 3.0                          | 3.0                            |
| Bathrooms             | 2.3                          | 2.1                            | 2.3                          | 2.1                            |
| Living area (sq. ft.) | 1,282                        | 1,149                          | 1,302                        | 1,161                          |
| Total rooms           | 7.3                          | 7.0                            | 7.2                          | 7.1                            |
| Year built            | 1954                         | 1954                           | 1955                         | 1954                           |

*Notes:* Sale analysis sample ( $N = 530,395$ ; the 530,365 with an exposure classification are split) by whether any of the 27 offense groups records an incident within 0.05 miles in the 3 or 6 months before sale. Listings near recent crime are cheaper, smaller, and slower to sell; the raw price gap (Table 1) is confounded by these and unobserved differences, motivating the within-parcel, tract $\times$ month design.

Table OA3: White-Collar Offenses and the Financial-Magnitude Account

| Offense    | Identifying<br>parcels | Pooled<br>2007–25 | Crisis<br>2009–13 | Recovery<br>2014–25 |
|------------|------------------------|-------------------|-------------------|---------------------|
| Fraud      | 38,856                 | −0.07<br>(0.10)   | −0.17<br>(0.25)   | 0.00<br>(0.12)      |
| Forgery    | 22,229                 | 0.23<br>(0.18)    | 0.06<br>(0.71)    | 0.18<br>(0.17)      |
| Bad checks | 4,226                  | 1.09**<br>(0.53)  | 2.40**<br>(1.06)  | 0.32<br>(0.68)      |

*Notes.* White-collar offenses, estimated on the same specification as Table 2 ( $\delta = (\beta_{\text{near}} - \beta_{\text{far}}) \times 100$ ; near 0–0.05 against 0.20–0.30 miles; parcel and tract  $\times$  year-month fixed effects, clustered by tract;  $N = 242,815$ ). The financial-magnitude account predicts that offenses inflicting large monetary losses capitalize into prices. Embezzlement has too few incidents to estimate. Each offense is shown at the central window of the range over which its discount is significant (or the median window, six months, if none is significant), so the window differs across offenses; identifying parcels are counted at that window; standard errors in parentheses.

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



Table OA4: Prior Estimates of Sale Prices

| Study                                     | Setting                | Outcome                                          | Offense                                  | Spatial unit of crime                | Estimate as reported                                                                                                                                                                         | This paper, same offense                                   |
|-------------------------------------------|------------------------|--------------------------------------------------|------------------------------------------|--------------------------------------|----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|------------------------------------------------------------|
| <i>Sale prices, aggregate crime rates</i> |                        |                                                  |                                          |                                      |                                                                                                                                                                                              |                                                            |
| Thaler (1978)                             | Rochester, NY          | Value                                            | Property crime                           | Census tract                         | $\approx 3\%$ per SD <sup>†</sup>                                                                                                                                                            | Burglary $-0.24$ ; theft $0.01$                            |
| Gibbons (2004)                            | London                 | Value                                            | Criminal damage                          | 250 m radius                         | $\approx 1\%$ per 0.1 SD                                                                                                                                                                     | Vandalism $-0.17$                                          |
| Gibbons (2004)                            | London                 | Value                                            | Burglary                                 | 250 m radius                         | Null                                                                                                                                                                                         | Burglary $-0.24$                                           |
| Schwartz, Susin & Voicu (2003)            | New York City          | Value                                            | Violent (murder, rape, robbery, assault) | Precinct (74)                        | Elasticity $0.13$ ; $\approx$ one third of the post-1994 boom                                                                                                                                | Homicide $-3.0$ ; robbery, assault $\approx 0$             |
| Schwartz, Susin & Voicu (2003)            | New York City          | Value                                            | Property (burglary, larceny, auto theft) | Precinct (74)                        | Negative, insignificant                                                                                                                                                                      | Burglary $-0.24$ ; theft, MVT $\approx 0$                  |
| Ihlanfeldt & Mayock (2010)                | Miami-Dade             | Value                                            | Violent                                  | Tract                                | Elasticity $-0.2$ to $-0.6$ (mean $-0.238$ )                                                                                                                                                 | Homicide $-3.0$ ; robbery, assault $\approx 0$             |
| Ihlanfeldt & Mayock (2010)                | Miami-Dade             | Value                                            | Property                                 | Tract                                | Small or null                                                                                                                                                                                | Burglary $-0.24$ ; MVT $-0.02$                             |
| Bishop & Murphy (2011)                    | San Francisco Bay Area | Value                                            | Violent                                  | Tract                                | WTP $\approx \$472/\text{yr}$ per 10% less                                                                                                                                                   | Homicide $-3.0$                                            |
| <i>Sale prices, point-located events</i>  |                        |                                                  |                                          |                                      |                                                                                                                                                                                              |                                                            |
| Linden & Rockoff (2008)                   | Mecklenburg Co., NC    | Value                                            | Sex-offender arrival                     | 0.1 mile                             | $\approx -4\%$                                                                                                                                                                               | Sex offenses $-0.47$ (suggestive)                          |
| Pope (2008)                               | Hillsborough Co., FL   | Value                                            | Sex-offender arrival                     | 0.1 mile                             | $-2.3\%$                                                                                                                                                                                     | Sex offenses $-0.47$ (suggestive)                          |
| Congdon-Hohman (2013)                     | Akron, OH              | Value                                            | Methamphetamine-lab discovery            | 0.1 to 0.5 mile                      | $-10\%$ to $-19\%$                                                                                                                                                                           | Narcotics $-0.29$                                          |
| <i>Rents</i>                              |                        |                                                  |                                          |                                      |                                                                                                                                                                                              |                                                            |
| Rizzo (1979)                              | Chicago                | Rents and values                                 | Total crime                              | Community area                       | Value elasticity $\approx -1.67^{\dagger}$ ; rent elasticity $-0.24^{\dagger}$                                                                                                               | Rents: sex $-0.22$ , prostitution $-0.15$ , liquor $-0.30$ |
| Koirala, Chakraborti & Pradhan (2023)     | North Carolina cities  | Rents and wages                                  | Violent and property rates               | City                                 | $\approx \$52/\text{month}$ net compensation                                                                                                                                                 | As above                                                   |
| Donovan et al. (2024)                     | New Zealand            | Rents and wages                                  | Victimizations per 1,000                 | Urban area (134)                     | Rent elasticity $-0.15$ to $-0.44$                                                                                                                                                           | As above                                                   |
| Klimova & Lee (2014)                      | Sydney                 | Rents and values                                 | Murder                                   | 0.1–0.3 mile                         | Rents: no effect; values $-3.9\%$ within 0.2 mile                                                                                                                                            | Homicide: sale $-3.0$ , rent too imprecise to sign         |
| Carrillo and Yezer (2021)                 | Individual units       | Rents and values                                 | Homicide, violent                        | 0.25 mile                            | Not directly comparable                                                                                                                                                                      | Homicide: sale $-3.0$ ; rent too imprecise to sign         |
| <i>This paper</i>                         |                        |                                                  |                                          |                                      |                                                                                                                                                                                              |                                                            |
|                                           | Chicago                | Sale price, asking rent, and transaction margins | 22 offense categories                    | 0.05 mile vs. 0.20–0.30 mile annulus | Homicide $-3.0$ , narcotics $-0.29$ , burglary $-0.24$ , vandalism $-0.17$ (priced); disorderly conduct $-0.65$ , sex offenses $-0.47$ (suggestive); remaining offenses small or $\approx 0$ |                                                            |

*Notes.* Estimates are in the units of the original study and are not on a common scale. “This paper, same offense” gives the near–far discount  $\delta$  (percent per doorstep incident) at each offense’s central window, for the offense closest to the one the prior study measures; for composite measures, the components are listed. Distance bands for Pope (2008) and Congdon-Hohman (2013) are inferred from the design. <sup>†</sup>Reported in a secondary source.

## B Additional Sale-Price Estimates

Table OA5: The Near–Far Discount Across Fixed-Effect Specifications

| Offense         | (0)<br>Raw          | (1)<br>Tract×mo.   | (2)<br>+ Char.     | (3)<br>Parcel FE   | (4)<br>Parcel FE<br>+ tract×mo. |
|-----------------|---------------------|--------------------|--------------------|--------------------|---------------------------------|
| Homicide        | −12.81***<br>(3.71) | −2.29<br>(1.43)    | −2.63**<br>(1.07)  | 0.24<br>(1.63)     | −3.02**<br>(1.38)               |
| Burglary        | −3.50***<br>(0.50)  | −0.71***<br>(0.27) | −0.32**<br>(0.15)  | −0.90***<br>(0.21) | −0.24**<br>(0.12)               |
| Narcotics       | −1.88***<br>(0.47)  | −0.54***<br>(0.18) | −0.56***<br>(0.13) | −1.09***<br>(0.29) | −0.33***<br>(0.11)              |
| Vandalism       | −3.45***<br>(0.56)  | −1.24***<br>(0.22) | −0.49***<br>(0.13) | −0.33**<br>(0.14)  | −0.16**<br>(0.08)               |
| Tract×month FE  | No                  | Yes                | Yes                | No                 | Yes                             |
| Characteristics | No                  | No                 | Yes                | No                 | No                              |
| Parcel FE       | No                  | No                 | No                 | Yes                | Yes                             |

*Notes.* Each cell is a separate near–far regression of  $\ln(\text{sale price})$  on doorstep (0–0.05 mi) and far (0.20–0.30 mi) incident counts;  $\delta = (\beta_{\text{near}} - \beta_{\text{far}}) \times 100$  is the percent price change per doorstep incident, standard error below. Columns: (0) no fixed effects; (1) tract×year-month effects; (2) adds observed listing characteristics (bedrooms, bathrooms, ln living and lot area, age, units, stories, property type, new construction); (3) parcel fixed effects in place of characteristics; (4) parcel and tract×year-month fixed effects, the headline specification. All five columns use the *same* rows, the column-(4) estimating sample ( $N = 242,815$  repeat-sale transactions); columns (1)–(4) also include the stale-crime persistence terms. Each offense is at its pre-set canonical window (homicide 10, burglary 8, narcotics 12, vandalism 7 months), which differs from the central window in Table 2 for narcotics (10) and vandalism (6). Clustered by tract.

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

Table OA6: Dose-Response for All Estimable Offenses

| Offense   | 1                                 | 2                  | 3               | 4+                | 1                    | 2      | 3      | 4+     |
|-----------|-----------------------------------|--------------------|-----------------|-------------------|----------------------|--------|--------|--------|
|           | $\delta$ : near incidents (vs. 0) |                    |                 |                   | listings identifying |        |        |        |
| Homicide  | −2.22<br>(1.43)                   | −20.17*<br>(10.72) | 8.86<br>(27.74) | 9.24<br>(6.06)    | 2,749                | 145    | 15     | 14     |
| Burglary  | 0.48*<br>(0.25)                   | −0.51<br>(0.36)    | −0.68<br>(0.59) | −1.85**<br>(0.76) | 55,036               | 20,475 | 7,903  | 6,598  |
| Narcotics | −0.52<br>(0.34)                   | 0.59<br>(0.66)     | −1.74<br>(1.10) | −2.38**<br>(1.12) | 30,589               | 9,048  | 3,928  | 8,040  |
| Vandalism | 0.15<br>(0.22)                    | −0.47<br>(0.31)    | −0.29<br>(0.44) | −0.92**<br>(0.45) | 63,985               | 31,189 | 15,485 | 16,236 |

*Notes.* Price effect (%) of 1, 2, 3, and 4+ near (0–0.05 mi) incidents relative to zero, for all 22 estimable offenses. Parcel and tract × month fixed effects, pooled 2007–2025, clustered by tract; each offense at its headline window (priced offenses) or a 12-month window otherwise. The final four columns (“listings”) give the number of sale listings identifying each count (not parcels); “—” denotes no listings at that count.

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

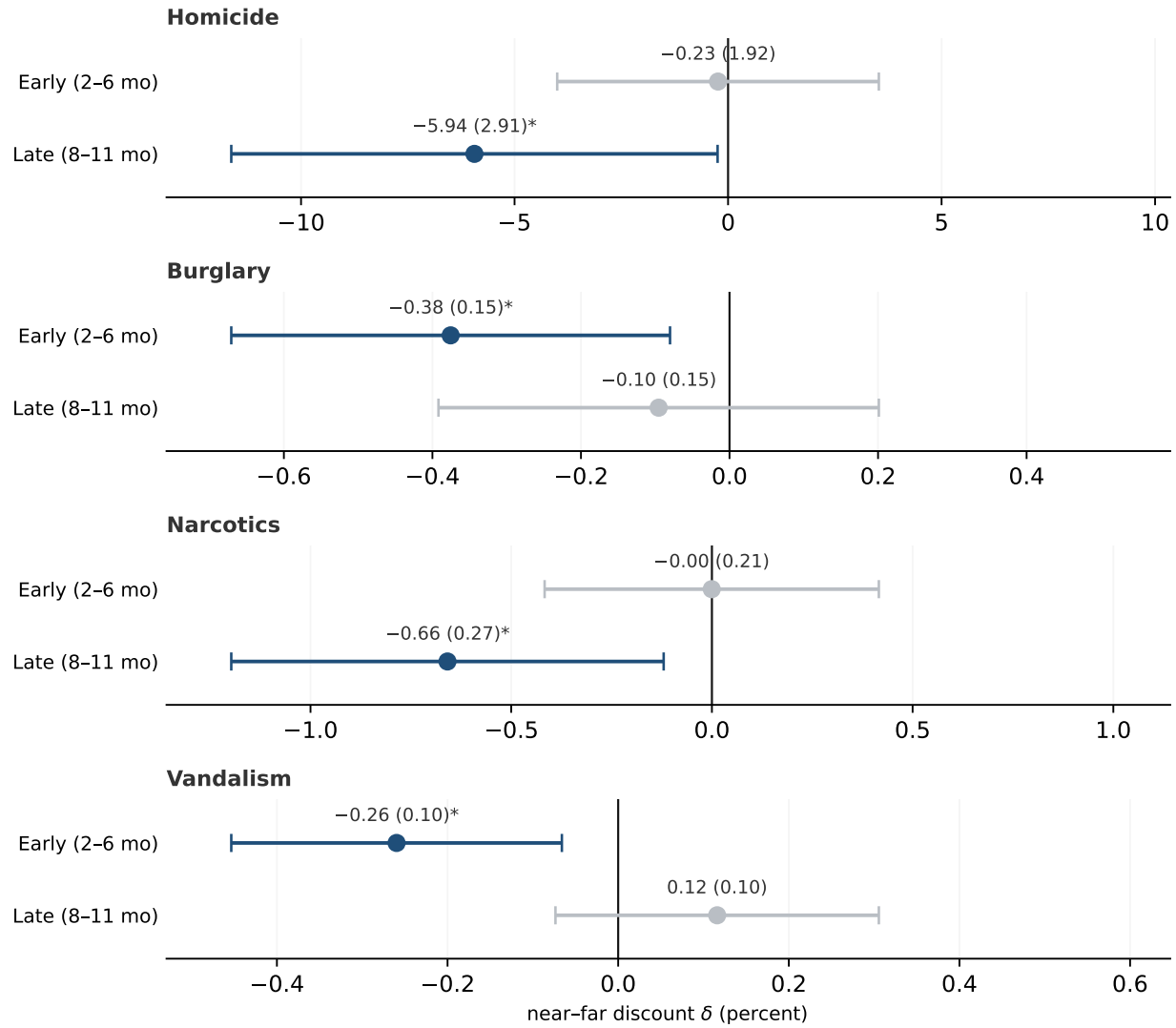


Figure OF2: Early versus late exposure, estimated jointly. For each priced offense, the doorstep near-far discount  $\delta$  (%) from one regression that enters an early block (incidents two to six months before the sale) and a late block (eight to eleven months before) as separate counts, each with its far-ring counterpart and separate controls for the omitted months (one, seven, twelve). The two points in each panel are the early-block and late-block estimates (significant at five percent in colour, otherwise grey), shown with 95% confidence intervals and the estimate and standard error beside each point; each panel has its own horizontal scale. The Wald test that the early and late coefficients are equal gives  $p = 0.121$  (homicide),  $0.200$  (burglary),  $0.090$  (narcotics), and  $0.008$  (vandalism), so the early and late estimates are individually distinguishable only for vandalism. Parcel and tract $\times$ year-month fixed effects, clustered by census tract,  $N = 242,815$ .

Table OA7: When the Discount Loads: Non-Cumulative Exposure

| Offense   | Earliest sig. month | Late block (8–11 mo) | Loads          |
|-----------|---------------------|----------------------|----------------|
| Homicide  | m1: $-5.86^*$       | $-5.95^{**}$ (2.91)  | late (8–11 mo) |
| Burglary  | m6: $-0.85^{***}$   | $-0.09$ (0.15)       | early          |
| Narcotics | m8: $-2.09^{***}$   | $-0.64^{**}$ (0.27)  | late (8–11 mo) |
| Vandalism | m3: $-0.44^{**}$    | $0.12$ (0.10)        | early          |

*Notes.* Non-cumulative decomposition of the doorstep discount by the lag at which crime occurs. “Earliest sig. month” is the first single month (crime in that month alone,  $\delta = \beta_{\text{near},w} - \beta_{\text{near},w-1}$ ) at which the discount is significant; “Late block” is crime in the disjoint 8–11 month span entered as a single regressor (standard error in parentheses). Same specification as Table 2: parcel and tract  $\times$  year-month fixed effects, clustered by tract,  $N = 242,815$ . Estimated jointly, the early and late blocks are statistically distinguishable only for vandalism (Figure OF2); see Section 5.1.

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

Table OA8: Sale-Price Capitalization by Domestic Status, All Offenses

| Window<br>(months) | Dom<br>$\delta_D$ | Non-dom<br>$\delta_N$ | Wald<br>$H_0: \delta_D = \delta_N$ |
|--------------------|-------------------|-----------------------|------------------------------------|
| 1                  | 0.20<br>(0.33)    | 0.05<br>(0.03)        | 0.634                              |
| 2                  | 0.05<br>(0.23)    | 0.03<br>(0.02)        | 0.933                              |
| 3                  | 0.00<br>(0.18)    | 0.03<br>(0.02)        | 0.885                              |
| 4                  | 0.03<br>(0.15)    | 0.01<br>(0.01)        | 0.904                              |
| 5                  | 0.06<br>(0.13)    | 0.00<br>(0.01)        | 0.646                              |
| 6                  | 0.01<br>(0.11)    | -0.01<br>(0.01)       | 0.910                              |
| 7                  | -0.02<br>(0.10)   | 0.00<br>(0.01)        | 0.882                              |
| 8                  | -0.06<br>(0.10)   | 0.00<br>(0.01)        | 0.547                              |
| 9                  | -0.14<br>(0.09)   | 0.00<br>(0.01)        | 0.143                              |
| 10                 | -0.13<br>(0.09)   | 0.00<br>(0.01)        | 0.145                              |
| 11                 | -0.17*<br>(0.09)  | 0.00<br>(0.01)        | 0.062*                             |
| 12                 | -0.17**<br>(0.08) | -0.01<br>(0.01)       | 0.046**                            |

*Notes.* Near-far price effect  $\delta = (\beta_{\text{near}} - \beta_{\text{far}}) \times 100$ , the percent sale-price change per doorstep incident (crime within 0–0.05 miles against 0.20–0.30 miles), at each cumulative exposure window from one to twelve months. Column  $\delta_D$  is the domestic-related arm (per the Illinois Domestic Violence Act) and  $\delta_N$  the non-domestic arm, both included in the same regression; the last column is the  $p$ -value of the Wald test that the two are equal. Incidents are pooled across the twenty-two named offense groups and split by the CPD domestic flag (1,003,504 domestic vs 4,357,006 non-domestic geocoded incidents, 2007–2025). Headline specification: parcel and tract  $\times$  year-month fixed effects, clustered by census tract;  $N = 242,815$  repeat-sale transactions, pooled 2007–2025. Standard errors in parentheses. Incident counts come from the geocoded 2007–2025 records with the domestic flag and differ by about 1.4 percent from the incident counts in Table 1, which use the exposure-construction pool.

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

Table OA9: Sale-Price Capitalization of Domestic versus Non-Domestic Assault

| Window<br>(months) | Dom<br>$\delta_D$ | Non-dom<br>$\delta_N$ | Wald<br>$H_0: \delta_D = \delta_N$ |
|--------------------|-------------------|-----------------------|------------------------------------|
| 1                  | 0.03<br>(0.37)    | -0.03<br>(0.20)       | 0.896                              |
| 2                  | 0.01<br>(0.26)    | -0.01<br>(0.14)       | 0.964                              |
| 3                  | -0.02<br>(0.21)   | 0.03<br>(0.11)        | 0.851                              |
| 4                  | 0.02<br>(0.17)    | -0.02<br>(0.09)       | 0.834                              |
| 5                  | 0.03<br>(0.15)    | -0.03<br>(0.08)       | 0.748                              |
| 6                  | 0.02<br>(0.13)    | -0.03<br>(0.07)       | 0.756                              |
| 7                  | -0.02<br>(0.12)   | -0.03<br>(0.07)       | 0.946                              |
| 8                  | -0.08<br>(0.12)   | -0.02<br>(0.06)       | 0.638                              |
| 9                  | -0.16<br>(0.11)   | -0.01<br>(0.05)       | 0.242                              |
| 10                 | -0.13<br>(0.10)   | 0.01<br>(0.05)        | 0.261                              |
| 11                 | -0.17<br>(0.10)   | -0.02<br>(0.04)       | 0.195                              |
| 12                 | -0.18*<br>(0.10)  | -0.03<br>(0.04)       | 0.174                              |

*Notes.* Near-far price effect  $\delta = (\beta_{\text{near}} - \beta_{\text{far}}) \times 100$ , the percent sale-price change per doorstep incident (crime within 0–0.05 miles against 0.20–0.30 miles), at each cumulative exposure window from one to twelve months. Domestic and non-domestic assault (split by the Chicago Police Department’s domestic flag, based on the Illinois Domestic Violence Act) are separate near-far exposures in the same regression: column  $\delta_D$  is the domestic assault arm,  $\delta_N$  the non-domestic assault arm, and the last column the  $p$ -value of the Wald test that the two are equal. Parcel and tract  $\times$  year-month fixed effects, clustered by census tract;  $N = 242,815$  repeat-sale transactions, pooled 2007–2025. Standard errors in parentheses.

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

Table OA10: Crime Recurrence and Capitalization

| Offense                | Recurrence $\rho$<br>(parcel + tract FE) | $\rho$ , no<br>parcel FE | $\delta$<br>(Table 2) [mo] |
|------------------------|------------------------------------------|--------------------------|----------------------------|
| Homicide <sup>†</sup>  | −0.026**<br>(0.012)                      | 0.070***<br>(0.006)      | −3.02** [10]<br>(1.38)     |
| Burglary <sup>†</sup>  | 0.067***<br>(0.009)                      | 0.362***<br>(0.011)      | −0.24** [8]<br>(0.12)      |
| Narcotics <sup>†</sup> | 0.397***<br>(0.038)                      | 0.685***<br>(0.013)      | −0.29** [10]<br>(0.14)     |
| Vandalism <sup>†</sup> | 0.093***<br>(0.018)                      | 0.389***<br>(0.013)      | −0.17** [6]<br>(0.08)      |
| Theft                  | 0.237<br>(0.158)                         | —                        | 0.01 [12]<br>(0.02)        |
| Motor-vehicle theft    | 0.140***<br>(0.031)                      | —                        | −0.02 [12]<br>(0.09)       |
| Robbery                | 0.038***<br>(0.012)                      | —                        | −0.03 [12]<br>(0.13)       |
| Assault (non-domestic) | 0.280***<br>(0.029)                      | —                        | −0.04 [12]<br>(0.04)       |
| Assault (domestic)     | 0.118***<br>(0.012)                      | —                        | −0.19* [12]<br>(0.10)      |
| All crime              | 0.314***<br>(0.113)                      | —                        | —                          |

*Notes.* Own-recurrence  $\rho$  of doorstep crime: the near-ring (0–0.05 mi) incident count over the window *after* the sale regressed on the count over the matching window *before*, standard errors clustered by tract in parentheses;  $N = 242,815$ . For the four priced offenses  $\rho$  is measured over the same exposure window as  $\delta$  (homicide and narcotics ten months, burglary eight, vandalism six; shown in the  $\delta$  bracket); the remaining offenses are measured over six months. The first  $\rho$  column uses parcel and tract  $\times$  sale-month fixed effects; the second omits parcel fixed effects and is shown for the priced offenses. Parcel fixed effects bias  $\rho$  toward zero in short panels, whereas the no-fixed-effect estimate includes permanent level differences across properties and overstates recurrence, so the two bracket the within-parcel persistence; for homicide the within-parcel estimate is slightly negative while the no-fixed-effect estimate is positive.  $\rho > 0$  means the offense recurs at the same property.  $\delta$  is each offense’s near–far close-price discount at its headline window (Table 2; window length in brackets), unpriced offenses at the twelve-month reference window. <sup>†</sup>Priced offenses.

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

**Hedonic estimates on all sales.** The headline design uses repeat sales, which identifies from twice-sold homes. As an alternative specification on the full sold sample, I estimate a cross-sectional hedonic near–far regression that replaces the parcel fixed effect with observed characteristics (bedrooms, bathrooms, living area, lot size, age, stories, property type, new construction, and units) together with tract $\times$ month fixed effects (Table OA11). The four priced discounts keep their sign and significance (−1.9 percent for homicide, −0.5 to −0.6 for burglary, narcotics, and vandalism), and theft remains a precise zero, so the four priced discounts do not depend on the repeat-sales sample. Because observed characteristics absorb less than the parcel fixed effect, the hedonic estimates are larger than the repeat-sales estimates for the three property offenses (burglary −0.52

against  $-0.24$ , narcotics  $-0.55$  against  $-0.33$ , vandalism  $-0.61$  against  $-0.16$ ) and smaller only for homicide ( $-1.90$  against  $-3.02$ ). Several offenses that are null under repeat sales become significantly negative here (robbery  $-0.77$ , domestic assault  $-0.72$ , non-domestic assault  $-0.52$ , and motor-vehicle theft  $-0.46$ ); these estimates reflect cross-parcel differences that the parcel fixed effect removes.

Table OA11: Robustness: Hedonic Estimates on All Sales

| Offense                               | $\delta$ (%)  | (s.e.) | Window |
|---------------------------------------|---------------|--------|--------|
| <i>Priced offenses</i>                |               |        |        |
| Homicide                              | $-1.90^{***}$ | (0.72) | 10     |
| Burglary                              | $-0.52^{***}$ | (0.13) | 8      |
| Narcotics                             | $-0.55^{***}$ | (0.09) | 12     |
| Vandalism                             | $-0.61^{***}$ | (0.11) | 7      |
| <i>No discount under repeat sales</i> |               |        |        |
| Theft                                 | 0.01          | (0.07) | 12     |
| Motor-vehicle theft                   | $-0.46^{**}$  | (0.20) | 12     |
| Robbery                               | $-0.77^{***}$ | (0.19) | 12     |
| Assault (non-dom.)                    | $-0.52^{***}$ | (0.09) | 12     |
| Assault (dom.)                        | $-0.72^{***}$ | (0.10) | 12     |

*Notes.* Cross-sectional hedonic near-far regression on all sold listings ( $N = 490,563$ ), replacing the parcel fixed effect with observed characteristics (bedrooms, bathrooms, ln living area, ln lot size, age, stories, property type, new construction, and units, with missing indicators) plus tract $\times$ month fixed effects, clustered by tract. Regressors are the 0–0.05 and 0.20–0.30 mi counts in the  $w$  months before sale and the same-ring counts in months  $(w+1)$ –12. Each cell reports  $\delta = (\beta_{\text{near}} - \beta_{\text{far}}) \times 100$  at the offense’s pre-set canonical window, which differs from the central significant window in Table 2 (narcotics at twelve months rather than ten, vandalism at seven rather than six).

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

## C Incident Location Classification

Several tests split an offense’s incidents by *where* they occur. I classify each incident using the Chicago Police Department’s `location_description` field into three mutually exclusive categories. *Commercial* covers places of business: banks, currency exchanges, and ATMs; retail, grocery, and drug stores; restaurants, bars, and hotels; gas stations; commercial, medical, and dental offices; factories and warehouses; and entertainment venues. *Residential* covers the dwelling interior and its curtilage: residences, apartments, and houses, together with garages, porches, yards, hallways, stairwells, basements, and public-housing units. *Street* covers the public way and other outdoor, encounterable space: streets, sidewalks, alleys, and gangways; parking and vacant lots; highways, parks, and waterfronts; and vehicles and transit. Incidents whose location maps to none of these, blank or ambiguous entries, are dropped, roughly one in eight. The street category is the encounterable bucket, because it is the space a passer-by can observe during a visit to the block.

This classification underlies the incident-location split (Table OA12).

Applying the same split to the four priced offenses (Table OA12) localizes the discount differently across them. A component significant on its own is not necessarily distinguishable from the others, so I report the Wald tests that the residential component equals the street and commercial components. Homicide’s discount loads on the street (public-space) component, the only component individually significant, though its sparse residential estimate is too imprecise to reject equality; narcotics is likewise significant only in the street component. Vandalism’s residential component is individually significant across most windows, and its residential and street estimates differ ( $p = 0.018$ ), with a post-sale placebo indistinguishable from zero. Burglary’s near–far discount does not localize cleanly in this coarse residential–street–commercial split: neither its pooled residential nor its street component is distinguishable from the other, and the pooled residential coefficient is matched window-for-window by post-sale crime. The finer within-residential split in Table 3 is sharper: burglary’s visible-*frontage* component is large and significant ( $-0.74$ , s.e.  $0.33$ , significant in 7 of 12 windows) with a clean placebo ( $-0.20$ ,  $p = 0.45$ ), while the hidden interior is null ( $-0.14$ , s.e.  $0.16$ ), so the pooled residential placebo failure traces to the interior subcategory, not to the frontage incidents that carry the effect. Several additional offenses show negative residential coefficients in isolation, but those patterns are mirrored by post-sale crime and disappear when the pre-sale effect is compared directly with its placebo; the priced set is therefore not an artifact of the binary  $p < .05$  placebo screen. Table OA13 reports the split for every estimable offense (the priced-four summary is Table OA12).

Table OA12: Where the Discount Comes From: Private versus Public Incidents

| Offense   | Private (residential) | Public (street)    | $p$ : priv.=pub. | Windows sig. | $N$     |
|-----------|-----------------------|--------------------|------------------|--------------|---------|
| Homicide  | −0.41<br>(2.39)       | −4.59***<br>(1.68) | 0.137            | 1/12         | 242,815 |
| Burglary  | −0.25*<br>(0.14)      | −0.93<br>(0.65)    | 0.289            | 0/12         | 242,815 |
| Narcotics | −0.28<br>(0.65)       | −0.38**<br>(0.19)  | 0.897            | 0/12         | 242,815 |
| Vandalism | −0.56***<br>(0.20)    | −0.01<br>(0.11)    | 0.018**          | 7/12         | 242,815 |

*Notes.* Near–far close-price effect (%) of each priced offense’s doorstep count, split by whether the incident was recorded at a *residential* address (private: the dwelling and its garage, porch, yard, hallways, and stairwells) or in *public space* (street and other public way), both estimated jointly in one regression; commercial incidents keep their own category in the three-category version (Appendix Table OA13). Pooled 2007–2025, parcel and tract×month fixed effects, near 0–0.05 vs. far 0.20–0.30 mi; standard errors clustered by tract in parentheses. Estimates at each offense’s central window (homicide 10, burglary 8, narcotics 10, vandalism 6 months); “Windows sig.” counts, of the twelve exposure windows, how many reject private=public at five percent.  $N = 242,815$ .

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

Table OA13: Incident-Location Split, All Estimable Offenses

| Offense             | Residential        | Street             | Commercial        | $p$<br>res=str | $p$<br>str=com | $p$<br>all eq. | $N$     |
|---------------------|--------------------|--------------------|-------------------|----------------|----------------|----------------|---------|
| Homicide            | −0.41<br>(2.39)    | −4.59***<br>(1.68) | −0.77<br>(2.76)   | 0.137          | 0.247          | 0.269          | 242,815 |
| Burglary            | −0.25*<br>(0.14)   | −0.93<br>(0.65)    | 0.00<br>(0.16)    | 0.289          | 0.151          | 0.243          | 242,815 |
| Narcotics           | −0.28<br>(0.65)    | −0.38**<br>(0.19)  | −0.10<br>(0.91)   | 0.897          | 0.766          | 0.951          | 242,815 |
| Vandalism           | −0.56***<br>(0.20) | −0.01<br>(0.11)    | −0.23<br>(0.20)   | 0.018**        | 0.338          | 0.054*         | 242,815 |
| Theft               | −0.03<br>(0.07)    | 0.04***<br>(0.01)  | 0.00<br>(0.02)    | 0.326          | 0.040**        | 0.105          | 242,815 |
| Motor-vehicle theft | 0.86**<br>(0.42)   | −0.04<br>(0.09)    | −0.62<br>(0.56)   | 0.042**        | 0.308          | 0.069*         | 242,815 |
| Robbery             | 0.47<br>(0.75)     | −0.06<br>(0.15)    | −0.06<br>(0.21)   | 0.480          | 0.994          | 0.777          | 242,815 |
| Assault (non-dom.)  | −0.28*<br>(0.16)   | 0.01<br>(0.08)     | 0.03<br>(0.07)    | 0.110          | 0.849          | 0.219          | 242,815 |
| Assault (dom.)      | −0.26**<br>(0.12)  | 0.04<br>(0.24)     | 0.10<br>(0.35)    | 0.296          | 0.892          | 0.453          | 242,815 |
| Disorderly conduct  | −1.58*<br>(0.84)   | −0.21<br>(0.41)    | −0.85<br>(1.09)   | 0.145          | 0.625          | 0.340          | 242,815 |
| Trespass            | −0.43**<br>(0.19)  | −0.24<br>(0.21)    | 0.09<br>(0.14)    | 0.478          | 0.210          | 0.071*         | 242,815 |
| Prostitution        | 0.80<br>(1.54)     | 0.02<br>(0.16)     | 0.27<br>(0.18)    | 0.612          | 0.294          | 0.551          | 242,815 |
| Fraud               | −0.40***<br>(0.15) | 0.24<br>(0.24)     | 0.02<br>(0.08)    | 0.010**        | 0.324          | 0.007***       | 242,815 |
| Sex offenses        | −0.64**<br>(0.32)  | −0.05<br>(0.47)    | −0.71<br>(0.47)   | 0.284          | 0.305          | 0.498          | 242,815 |
| Forgery             | −0.39<br>(0.43)    | 0.39<br>(0.41)     | 0.95***<br>(0.29) | 0.155          | 0.289          | 0.044**        | 242,815 |
| Weapons             | −1.10<br>(1.21)    | 0.19<br>(0.28)     | 1.54<br>(1.37)    | 0.311          | 0.338          | 0.376          | 242,815 |
| Arson               | −1.90<br>(1.96)    | 2.21<br>(1.40)     | 2.52<br>(1.95)    | 0.072*         | 0.897          | 0.164          | 242,815 |
| Stolen property     | −0.82<br>(3.81)    | −1.58<br>(1.60)    | 1.93<br>(1.51)    | 0.858          | 0.093*         | 0.231          | 242,815 |
| Liquor              | 3.24<br>(2.94)     | 0.60<br>(1.14)     | 0.31<br>(0.36)    | 0.408          | 0.812          | 0.601          | 242,815 |
| Gambling            | −5.64<br>(9.72)    | −1.69<br>(2.03)    | −1.95<br>(4.27)   | 0.706          | 0.957          | 0.931          | 242,815 |
| Bad checks          | 0.76<br>(0.62)     | 2.40<br>(3.14)     | 0.41<br>(0.44)    | 0.605          | 0.533          | 0.768          | 242,815 |
| Family offenses     | 1.00<br>(0.68)     | 0.78<br>(1.26)     | 2.36<br>(1.68)    | 0.879          | 0.484          | 0.742          | 242,815 |

*Notes.* Three-category incident-location split (residential / street / commercial) for all 22 estimable offenses, at each priced offense's central window (homicide 10, burglary 8, narcotics 10, vandalism 6) and a 12-month window otherwise; entered jointly, pooled 2007–2025, parcel and tract  $\times$  month fixed effects, standard errors clustered by tract. The last three columns are Wald  $p$ -values that residential equals street, that street equals commercial, and that all three are equal. This three-category detail underlies the private/public summary in Table OA12.  $N = 242,815$ .

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

Because the reported estimates fix each offense at a single canonical window, Table OA14 reports

the cross-location Wald tests at every exposure window, so the location heterogeneity can be read against the full window range rather than a selected one. The residential-versus-street difference for vandalism is significant across seven consecutive windows; homicide crosses conventional significance only at the twelve-month window, where its residential estimate is wrongly signed; burglary's only significant contrasts are residential-versus-commercial at ten and eleven months, driven by a residential component that fails the post-sale placebo; and narcotics never rejects. The vandalism result is therefore not a selected-window artifact, while the street concentration for homicide and narcotics is descriptive rather than a demonstrated cross-location difference.

Table OA14: Cross-Location Wald Tests by Exposure Window, Priced Offenses

| Window                                          | Homicide     | Narcotics    | Vandalism       | Burglary     |
|-------------------------------------------------|--------------|--------------|-----------------|--------------|
| <i>Panel A. Residential = street/public (p)</i> |              |              |                 |              |
| 1                                               | 0.227        | 0.447        | 0.495           | 0.216        |
| 2                                               | 0.099*       | 0.647        | 0.270           | 0.520        |
| 3                                               | 0.526        | 0.520        | 0.238           | 0.916        |
| 4                                               | 0.624        | 0.948        | 0.182           | 0.712        |
| 5                                               | 0.678        | 0.647        | 0.090*          | 0.925        |
| 6                                               | 0.410        | 0.843        | 0.018**         | 0.810        |
| 7                                               | 0.297        | 0.853        | <b>0.002***</b> | 0.425        |
| 8                                               | 0.136        | 0.679        | 0.001***        | <b>0.289</b> |
| 9                                               | 0.106        | 0.851        | 0.002***        | 0.340        |
| 10                                              | <b>0.137</b> | 0.897        | 0.001***        | 0.317        |
| 11                                              | 0.080*       | 0.879        | 0.002***        | 0.340        |
| 12                                              | 0.046**      | <b>0.677</b> | 0.000***        | 0.287        |
| <i>Panel B. Residential = commercial (p)</i>    |              |              |                 |              |
| 1                                               | 0.380        | 0.148        | 0.605           | 0.847        |
| 2                                               | 0.147        | 0.689        | 0.383           | 0.273        |
| 3                                               | 0.498        | 0.411        | 0.417           | 0.545        |
| 4                                               | 0.689        | 0.865        | 0.551           | 0.227        |
| 5                                               | 0.475        | 0.625        | 0.475           | 0.294        |
| 6                                               | 0.420        | 0.826        | 0.258           | 0.158        |
| 7                                               | 0.500        | 0.960        | <b>0.233</b>    | 0.362        |
| 8                                               | 0.345        | 0.845        | 0.252           | <b>0.251</b> |
| 9                                               | 0.817        | 0.975        | 0.234           | 0.121        |
| 10                                              | <b>0.916</b> | 0.871        | 0.113           | 0.034**      |
| 11                                              | 0.717        | 0.861        | 0.154           | 0.032**      |
| 12                                              | 0.379        | <b>0.550</b> | 0.142           | 0.104        |

*Notes.* Wald  $p$ -values that the residential near-far component equals the street/public component (Panel A) and the commercial component (Panel B), at every exposure window 1–12 months before the sale, for the four priced offenses. Pooled 2007–2025, parcel and tract  $\times$  month fixed effects, standard errors clustered by tract; each offense's pre-set canonical window, which differs from the central significant window in Table 2 (narcotics twelve rather than ten months, vandalism seven rather than six), is in **bold**. The full three-component estimates and placebos are in the accompanying workbook.

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

Table OA15: Residential Location Codes and Their Frontage/Interior Assignment

| CPD location code                    | Incidents | Main     | Strict   |
|--------------------------------------|-----------|----------|----------|
| Residence                            | 1,401,849 | Interior | Interior |
| Apartment                            | 1,032,507 | Interior | Interior |
| Residence-Garage                     | 134,912   | Frontage | Frontage |
| Residence Porch/Hallway <sup>†</sup> | 124,149   | Frontage | Dropped  |
| Residential Yard (Front/Back)        | 75,130    | Frontage | Frontage |
| Cha Apartment                        | 40,920    | Interior | Interior |
| Driveway - Residential               | 25,198    | Frontage | Frontage |
| Cha Hallway/Stairwell/Elevator       | 25,021    | Frontage | Dropped  |
| Residence - Porch / Hallway          | 19,974    | Frontage | Dropped  |
| Residence - Yard (Front / Back)      | 17,113    | Frontage | Frontage |
| Residence - Garage                   | 16,299    | Frontage | Frontage |
| House                                | 740       | Interior | Interior |
| Porch                                | 415       | Frontage | Dropped  |
| Yard                                 | 342       | Frontage | Frontage |
| Hallway                              | 111       | Frontage | Dropped  |
| Garage                               | 75        | Frontage | Frontage |
| Cha Hallway                          | 40        | Frontage | Dropped  |
| Vestibule                            | 28        | Frontage | Dropped  |
| Stairwell                            | 28        | Frontage | Dropped  |
| Cha Stairwell                        | 11        | Frontage | Dropped  |
| Cha Lobby                            | 9         | Frontage | Dropped  |
| Trailer                              | 4         | Interior | Interior |
| Cha Breezeway                        | 3         | Frontage | Dropped  |
| Coach House                          | 3         | Interior | Interior |
| Rooming House                        | 2         | Interior | Interior |

*Notes.* Every residential-address value of the Chicago Police Department `location_description` field, its total incident count over 2001–2025, and its assignment in the within-residential frontage/interior split (Table 3). *Main* is the baseline definition: frontage (the visible outdoor curtilage and building-exterior/entry codes) versus the dwelling interior. *Strict* restricts frontage to outdoor curtilage only (garage, yard, driveway, gangway) and drops the fused porch/hallway code and interior common areas; estimates under the strict definition are not reported.

<sup>†</sup>The fused `PORCH/HALLWAY` code combines a visible porch with a hidden hallway; it is counted as frontage in the main definition and dropped in the strict definition.

The location split has an objective counterpart. If residential and street incidents were priced differently only because they carry different information about *future* crime, their forecasting power for future doorstep crime should differ in the same direction. It does not, for the offense where it matters. Table OA16 regresses future doorstep crime on past surrounding crime separately by incident location, with no fixed effects, the informational benchmark available to a buyer at sale. For vandalism, residential and street incidents forecast future doorstep crime comparably at every window (for example  $R^2 = 0.148$  versus 0.125 at six months), yet only *residential* vandalism is priced. The capitalization difference is therefore unlikely to reflect a difference in objective recurrence, as the two kinds of incident predict the future at close to the same rate, though I do not test the small forecasting gap formally; the market prices the one visible at the dwelling. Homicide and narcotics forecast from the street and burglary from residential incidents, as expected

from where those offenses physically occur.

Table OA16: Forecast of Future Doorstep Crime by Incident Location, All Windows

| Win. | Homicide |       | Burglary |       | Narcotics |       | Vandalism |       |
|------|----------|-------|----------|-------|-----------|-------|-----------|-------|
|      | Res      | St.   | Res      | St.   | Res       | St.   | Res       | St.   |
| 1    | 0.000    | 0.000 | 0.032    | 0.015 | 0.007     | 0.099 | 0.020     | 0.021 |
| 2    | 0.000    | 0.001 | 0.069    | 0.024 | 0.014     | 0.156 | 0.050     | 0.049 |
| 3    | 0.000    | 0.002 | 0.100    | 0.029 | 0.023     | 0.186 | 0.079     | 0.073 |
| 4    | 0.000    | 0.003 | 0.126    | 0.033 | 0.029     | 0.205 | 0.104     | 0.093 |
| 5    | 0.000    | 0.004 | 0.149    | 0.035 | 0.036     | 0.218 | 0.127     | 0.109 |
| 6    | 0.000    | 0.005 | 0.169    | 0.037 | 0.042     | 0.232 | 0.148     | 0.125 |
| 7    | 0.000    | 0.006 | 0.189    | 0.038 | 0.049     | 0.241 | 0.167     | 0.141 |
| 8    | 0.000    | 0.008 | 0.208    | 0.037 | 0.054     | 0.251 | 0.185     | 0.155 |
| 9    | 0.000    | 0.009 | 0.226    | 0.036 | 0.060     | 0.258 | 0.200     | 0.168 |
| 10   | 0.001    | 0.011 | 0.240    | 0.034 | 0.066     | 0.263 | 0.214     | 0.178 |
| 11   | 0.001    | 0.012 | 0.253    | 0.033 | 0.071     | 0.267 | 0.227     | 0.188 |
| 12   | 0.001    | 0.014 | 0.265    | 0.031 | 0.077     | 0.271 | 0.238     | 0.196 |

*Notes.* Forecast  $R^2$  from regressing future doorstep crime (near ring 0–0.05 mi, after the sale) on past surrounding crime (far ring 0.20–0.30 mi, before the sale), estimated separately for residential (Res) and street (St.) incidents, no fixed effects (the informational benchmark available to a buyer at sale), all geocoded closed MLS sales before the constant-parcel and repeat-sales filters,  $N = 923,730$ , at each exposure window (Win., months). For *vandalism*, residential and street incidents forecast future doorstep crime comparably at every window, yet only residential vandalism is priced, so the capitalization difference cannot reflect a difference in objective recurrence. Homicide and narcotics forecast from the street and burglary from residential incidents, as expected from where those offenses physically occur.

## D The Relevance Split: What Is Harmed

Table 3, Panel B holds observability roughly fixed (both targets are frontage incidents, visible from the street) and varies what the offense harms, splitting vandalism into damage to the property and damage to a vehicle. Table OA17 reports the full twelve-window version under both fixed-effect designs. Under parcel and tract $\times$ year-month fixed effects the damage-to-property arm is negative and significant from the seventh exposure month ( $-0.89$  to  $-1.03$ ) while the damage-to-vehicle arm is null at every window, and the two differ most at the twelve-month window ( $p = 0.057$ ). The two sub-codes forecast their own recurrence at the same small rate (the Table OA10 design applied to each;  $\rho$  between  $-0.00$  and  $-0.02$ , indistinguishable,  $z < 1$ ), so the contrast holds the recurrence benchmark fixed and varies only what is harmed. Under parcel $\times$ era fixed effects both arms are negative and of similar size and are not distinguishable at any window; this specification drops to about half the sample ( $N = 123,133$ ), because parcel $\times$ era is identified only from parcels that resell within the same era, so it is lower-powered and its flat arms are consistent with, not evidence against, the parcel-and-month split.

Table OA17: The Relevance Split (Vandalism, by Target) Under Both Fixed-Effect Designs

|                                                                    | Exposure window (months before sale) |                 |                |                |                |                 |                   |                   |                   |                    |                   |                    |
|--------------------------------------------------------------------|--------------------------------------|-----------------|----------------|----------------|----------------|-----------------|-------------------|-------------------|-------------------|--------------------|-------------------|--------------------|
|                                                                    | 1                                    | 2               | 3              | 4              | 5              | 6               | 7                 | 8                 | 9                 | 10                 | 11                | 12                 |
| <i>Panel A. Parcel + tract×year-month fixed effects (headline)</i> |                                      |                 |                |                |                |                 |                   |                   |                   |                    |                   |                    |
| Damage to property                                                 | 0.85<br>(1.12)                       | 1.06<br>(0.83)  | 0.80<br>(0.63) | 0.22<br>(0.58) | 0.03<br>(0.51) | -0.49<br>(0.47) | -0.89**<br>(0.45) | -0.95**<br>(0.43) | -0.91**<br>(0.41) | -1.03***<br>(0.39) | -0.92**<br>(0.36) | -0.89***<br>(0.34) |
| Damage to vehicle                                                  | 0.70<br>(0.93)                       | 0.89<br>(0.67)  | 0.50<br>(0.56) | 0.33<br>(0.53) | 0.20<br>(0.48) | 0.21<br>(0.45)  | 0.05<br>(0.42)    | -0.07<br>(0.40)   | -0.13<br>(0.40)   | -0.08<br>(0.38)    | -0.09<br>(0.37)   | 0.02<br>(0.34)     |
| Property = vehicle ( <i>p</i> )                                    | 0.916                                | 0.874           | 0.724          | 0.896          | 0.818          | 0.298           | 0.143             | 0.142             | 0.168             | 0.078              | 0.102             | 0.057              |
| <i>Panel B. Parcel×era + tract×year-month fixed effects</i>        |                                      |                 |                |                |                |                 |                   |                   |                   |                    |                   |                    |
| Damage to property                                                 | -0.23<br>(1.97)                      | 2.49*<br>(1.47) | 1.17<br>(1.17) | 0.02<br>(1.02) | 0.33<br>(0.92) | 0.11<br>(0.80)  | -0.59<br>(0.73)   | -0.65<br>(0.68)   | -0.66<br>(0.64)   | -1.22*<br>(0.62)   | -0.99<br>(0.60)   | -0.91<br>(0.56)    |
| Damage to vehicle                                                  | 0.52<br>(1.20)                       | 0.29<br>(0.80)  | 0.11<br>(0.65) | 0.16<br>(0.67) | 0.20<br>(0.65) | -0.09<br>(0.60) | -0.30<br>(0.56)   | -0.37<br>(0.52)   | -0.62<br>(0.53)   | -0.80*<br>(0.48)   | -0.86*<br>(0.46)  | -0.81*<br>(0.42)   |
| Property = vehicle ( <i>p</i> )                                    | 0.759                                | 0.208           | 0.447          | 0.914          | 0.918          | 0.849           | 0.764             | 0.753             | 0.962             | 0.602              | 0.869             | 0.895              |

*Notes.* Full twelve-window version of Table 3, Panel B: vandalism frontage incidents split by target, damage to the property versus damage to a vehicle, each as its own near-far difference  $\delta = (\beta_{\text{near}} - \beta_{\text{far}}) \times 100$  in one regression, pooled 2007–2025, clustered by tract; standard errors below the estimates. The “Property = vehicle” row is the Wald *p*-value for equality of the two arms. *Panel A* is the headline design (parcel and tract×year-month fixed effects,  $N = 242,815$ ). *Panel B* absorbs parcel×era instead of parcel ( $N = 123,133$ ), which drops parcels that do not resell within an era.

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

Table OA18: Within-Property Visibility, All Estimable Offenses (Window-by-Window)

| Offense                      | Frontage $\delta$ [min, max] | Fr. sig <sup>-</sup> (plac.) | Interior $\delta$ [min, max] | Int. sig <sup>-</sup> | Fr. $\neq$ Int. |
|------------------------------|------------------------------|------------------------------|------------------------------|-----------------------|-----------------|
| Burglary                     | [-0.76, -0.37]               | 7/12 (0)                     | [-0.30, -0.11]               | 0/12                  | 1/12            |
| Narcotics                    | [-7.45, -2.35]               | 7/12 (0)                     | [-1.20, 0.92]                | 0/12                  | 6/12            |
| Vandalism                    | [-0.60, 1.02]                | 3/12 (0)                     | [-0.79, -0.34]               | 9/12                  | 3/12            |
| Homicide                     | [-23.69, 2.04]               | 0/12 (2)                     | [-0.65, 6.31]                | 0/12                  | 0/12            |
| Larceny/theft                | [-0.09, 0.17]                | 0/12 (0)                     | [-0.08, 0.45]                | 0/12                  | 0/12            |
| Motor-vehicle theft          | [0.52, 2.33]                 | 0/12 (0)                     | [1.02, 5.35]                 | 0/12                  | 0/12            |
| Robbery                      | [-2.41, 1.90]                | 0/12 (0)                     | [0.57, 3.89]                 | 0/12                  | 5/12            |
| Assault (non-dom.)           | [-0.43, 0.34]                | 0/12 (0)                     | [-0.49, 0.02]                | 1/12                  | 0/12            |
| Assault (dom.)               | [-1.14, 1.89]                | 0/12 (6)                     | [-0.24, 0.12]                | 0/12                  | 0/12            |
| Sex offenses                 | [-4.27, 1.98]                | 0/12 (0)                     | [-0.61, 0.12]                | 1/12                  | 0/12            |
| Prostitution                 | [-9.87, 10.85]               | 0/12 (6)                     | [0.62, 3.31]                 | 0/12                  | 0/12            |
| Disorderly conduct           | [-0.81, 3.83]                | 0/12 (0)                     | [-2.07, -0.98]               | 2/12                  | 0/12            |
| Trespass                     | [-0.79, 0.03]                | 1/12 (0)                     | [-0.81, -0.28]               | 0/12                  | 0/12            |
| Weapons                      | [-10.15, -1.48]              | 0/12 (0)                     | [-3.67, 2.33]                | 0/12                  | 1/12            |
| Arson                        | [-10.05, 0.95]               | 0/12 (0)                     | [-5.58, 4.33]                | 0/12                  | 0/12            |
| Fraud                        | [-4.24, -1.07]               | 0/12 (0)                     | [-0.41, -0.18]               | 5/12                  | 0/12            |
| Forgery                      | [-2.08, 10.26]               | 0/12 (1)                     | [-1.11, 0.35]                | 0/12                  | 0/12            |
| Bad checks                   | [-1.29, 15.59]               | 0/12 (5)                     | [0.27, 1.65]                 | 0/12                  | 0/12            |
| Family (non-violent)         | [-0.98, 6.86]                | 0/12 (0)                     | [0.75, 2.59]                 | 0/12                  | 0/12            |
| Gambling                     | [-19.36, 13.53]              | 0/12 (0)                     | [-35.03, 30.37]              | 1/12                  | 0/12            |
| Stolen property              | [-6.42, 16.84]               | 0/12 (0)                     | [-17.80, 1.04]               | 0/12                  | 0/12            |
| Liquor law                   | [-20.27, 10.80]              | 1/12 (8)                     | [-0.25, 7.95]                | 0/12                  | 1/12            |
| <i>All offenses (pooled)</i> | [-0.23, 0.01]                | 5/12 (0)                     | [-0.14, 0.07]                | 4/12                  | 0/12            |

*Notes.* Window-by-window companion to Table 3: residential incidents of each offense split between the visible frontage (garage, porch, yard, driveway, porch/hallway and common areas; Table OA15) and the hidden dwelling interior, estimated jointly, pooled 2007–2025, parcel and tract $\times$ month fixed effects, clustered by tract. For each offense the frontage and interior columns give the range of  $\delta = (\beta_{\text{near}} - \beta_{\text{far}}) \times 100$  across the twelve exposure windows; “sig<sup>-</sup>” counts windows with a significant negative coefficient at five percent, and the parenthetical is the number in which the post-sale placebo (crime after the sale) is significant. The last column counts windows in which frontage differs from interior at five percent (Wald). Frontage narcotics is a small subsample (incidents recorded at the porch or garage), hence its large per-incident magnitudes and standard errors.

Table OA19: Relevance: What the Offense Threatens and Whether It Is Capitalized

| Offense                                                      | What is threatened    | Capitalized |
|--------------------------------------------------------------|-----------------------|-------------|
| <i>Anchored to the address (stays with the property)</i>     |                       |             |
| Burglary                                                     | dwelling              | Yes         |
| Homicide <sup>‡</sup>                                        | place (violent event) | Yes         |
| Narcotics                                                    | place (environment)   | Yes         |
| Vandalism                                                    | structure/land        | Yes         |
| Disorderly conduct                                           | place (environment)   | Suggestive  |
| Arson                                                        | structure/land        | No          |
| Gambling                                                     | place (environment)   | No          |
| Liquor                                                       | place (environment)   | No          |
| Prostitution                                                 | place (environment)   | No          |
| Trespass <sup>†</sup>                                        | structure/land        | No          |
| Weapons                                                      | place (environment)   | No          |
| <i>Travels with the household (person or portable asset)</i> |                       |             |
| Sex offenses                                                 | person                | Suggestive  |
| Assault (domestic)                                           | person                | No          |
| Assault (non-dom.)                                           | person                | No          |
| Bad checks                                                   | financial             | No          |
| Family                                                       | person                | No          |
| Forgery                                                      | financial             | No          |
| Fraud                                                        | financial             | No          |
| Larceny/theft                                                | portable asset        | No          |
| Motor vehicle theft                                          | portable asset        | No          |
| Robbery                                                      | person                | No          |
| Stolen property                                              | portable asset        | No          |

*Notes.* Offenses classified by whether the harm stays with the address (dwelling, structure or land, or the place itself) or travels with the household (a person, or a portable or financial asset). “Capitalized” follows the offense sort in Appendix Table A3: *Yes* for the priced offenses, *Suggestive* for the suggestive tier, *No* otherwise.

<sup>‡</sup>Homicide is person-directed; it is placed here because its discount loads on incidents in public space near the home, not on the dwelling. Were it classified as travelling with the household, it would be the one priced offense in that group.

<sup>†</sup>Trespass is imprecisely estimated: no discount is detected, but the estimate is consistent with a discount as large as 0.2 to 0.3 percent per incident.

## E Additional Price-Formation Tables

This appendix collects supporting tables for Section 5.3. Table OA20 is the era-resolved pricing chain (ask, sale-to-list, price cut, close) reporting each offense at its identifying era, the counterpart to the pooled Table 4 in the main text; Table OA21 gives the era-resolved friction margins. Table OA22 reports the during-listing price cut in detail, by era and for the endogeneity-free fixed

three-month window. Table OA24 decomposes the asking-price experience gradient into the slope among experienced agents and the first-time-in-tract differential.

Table OA20: Price Formation Across Listing Margins

| Offense            | Era      | Ask<br>ln list     | Sale-to-list<br>ln close/list | Price cut<br>pp  | Close<br>ln close  |
|--------------------|----------|--------------------|-------------------------------|------------------|--------------------|
| Homicide           | Recovery | −0.82<br>(0.68)    | −0.15<br>(0.57)               | 7.39**<br>(3.42) | −4.12***<br>(1.47) |
| Narcotics          | Crisis   | 0.22<br>(0.28)     | −0.31***<br>(0.10)            | 0.07<br>(0.46)   | −0.55**<br>(0.27)  |
| Burglary           | Crisis   | −0.29<br>(0.30)    | 0.08<br>(0.10)                | −0.34<br>(0.55)  | −0.87***<br>(0.30) |
| Vandalism          | Crisis   | 0.07<br>(0.12)     | −0.04<br>(0.09)               | −0.06<br>(0.37)  | −0.54**<br>(0.25)  |
| Disorderly conduct | Recovery | −1.37***<br>(0.34) | −0.22<br>(0.17)               | 1.31<br>(1.50)   | −0.29<br>(0.41)    |
| Parcel FE          |          | Yes                | Yes                           | Yes              | Yes                |
| Tract × month FE   |          | Yes                | Yes                           | Yes              | Yes                |

*Notes.* Each offense is shown at its identifying era and central identifying close window; the price cut is on its during-listing spell clock. Columns are the opening ask (ln original list price), the sale-to-list ratio (ln of real close over real list), the probability of a price cut while the property is listed, and the closing price (ln real close). All are near–far differences (within 0–0.05 against 0.20–0.30 miles) in percent, except the price cut in percentage points, with parcel and tract × month fixed effects, clustered by tract. The close is estimated on the repeat-sale sample and the listing margins on the listing sample, so the columns are not a within-sample decomposition. Standard errors in parentheses.

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

Table OA21: Frictions: Days on Market and Withdrawal

| Offense                                                         | Estimate          |
|-----------------------------------------------------------------|-------------------|
| <i>A. Days on market (listings that sold; near ring 0–0.05)</i> |                   |
| Homicide                                                        | 13.53**<br>(5.77) |
| Sex offenses                                                    | 4.04***<br>(1.17) |
| <i>B. Withdrawal (all listings; ring per row)</i>               |                   |
| Homicide                                                        | 2.82**<br>(1.17)  |
| Robbery                                                         | 0.42**<br>(0.16)  |
| Weapons                                                         | 0.25**<br>(0.10)  |

*Notes.* Near–far estimates on the listing frictions: days on market (percent, Panel A) and withdrawal (percentage points, Panel B), near ring against the 0.20–0.30 mile annulus, clustered by tract, each at the median of its significant windows. Days on market is estimated on the sold sample; withdrawal on all listings, since a listing that sold was not withdrawn, with the exposure window for a withdrawn listing counted from its original list date (the listing clock used for the list-side margins). Panel A uses the doorstep 0–0.05 ring, recovery era, under parcel×era fixed effects (parcel fixed effects for sex offenses). Standard errors in parentheses.

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

Table OA22: Price Revision During the Listing

| Offense                                                  | During listing   |                   | First 3 months   |
|----------------------------------------------------------|------------------|-------------------|------------------|
|                                                          | Recovery         | Crisis            | Recovery         |
| <i>A. Probability of a price cut (percentage points)</i> |                  |                   |                  |
| Homicide                                                 | 7.43**<br>(3.43) | 15.42**<br>(7.67) | 6.12*<br>(3.31)  |
| Narcotics                                                | 0.43<br>(0.48)   | 0.08<br>(0.46)    | 0.56<br>(0.51)   |
| Burglary                                                 | -0.40<br>(0.48)  | -0.33<br>(0.55)   | -0.61*<br>(0.34) |
| Vandalism                                                | -0.46<br>(0.29)  | -0.06<br>(0.37)   | -0.12<br>(0.24)  |
| <i>B. Size of the markdown (percent)</i>                 |                  |                   |                  |
| Homicide                                                 | -0.38<br>(0.77)  | -3.42*<br>(1.88)  | -0.62<br>(0.59)  |
| Narcotics                                                | -0.18*<br>(0.11) | -0.20<br>(0.12)   | -0.05<br>(0.13)  |
| Burglary                                                 | -0.13*<br>(0.07) | -0.08<br>(0.12)   | -0.08<br>(0.05)  |
| Vandalism                                                | -0.02<br>(0.05)  | -0.09<br>(0.09)   | -0.02<br>(0.04)  |
| Parcel FE                                                | Yes              | Yes               | Yes              |
| Tract $\times$ listing-month FE                          | Yes              | Yes               | Yes              |

*Notes.* The dependent variable in Panel A is an indicator that the current list price is below the original asking price (a price cut), reported in percentage points; in Panel B it is the log ratio of current to original list price, in percent. The regressor is the near-far difference in doorstep incidents (within 0–0.05 against 0.20–0.30 miles) counted *during the listing*: the “during listing” columns count every incident between the original listing date and the close, with near and far measured over the same interval; the “first 3 months” column counts only incidents in the first three months after listing. All specifications include parcel and tract  $\times$  listing-month fixed effects and cluster by tract;  $N \approx 233,813$  listings. Standard errors in parentheses.

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

Table OA23: Does Crime Trigger a Mid-Listing Price Cut? A Discrete-Time Hazard

| Offense             | Prior month<br>(lag 1) | Cumulative<br>since listing | Lead crime<br>(placebo) |
|---------------------|------------------------|-----------------------------|-------------------------|
| Homicide            | 0.24<br>(0.35)         | -0.22<br>(0.20)             | 0.06<br>(0.34)          |
| Narcotics           | 0.02<br>(0.03)         | -0.02<br>(0.01)             | -0.02<br>(0.02)         |
| Burglary            | -0.10*<br>(0.05)       | -0.03<br>(0.03)             | -0.06<br>(0.05)         |
| Vandalism           | -0.03<br>(0.03)        | -0.03<br>(0.02)             | -0.01<br>(0.04)         |
| Larceny/theft       | -0.01<br>(0.02)        | -0.01<br>(0.01)             | -0.01<br>(0.02)         |
| Motor vehicle theft | 0.00<br>(0.09)         | -0.03<br>(0.08)             | -0.07<br>(0.08)         |

*Notes.* Discrete-time hazard of a mid-listing price cut, estimated on a listing-month panel: one row per listing per month on the market, up to the first downward list-price revision (dated from the MLS listing change history) or the off-market date. The outcome is an indicator for a cut in that month (mean monthly hazard  $\approx 6.8\%$ ); each cell is the near-far effect  $(\beta_{\text{near}} - \beta_{\text{far}}) \times 100$  in percentage points, with tract  $\times$  calendar-month and months-since-listing fixed effects, clustered by tract. “Prior month” is near-far crime in the month *before* the risk month; “Cumulative” is crime since listing through the prior month; “Lead crime” is the *following* month’s crime (placebo). Change-history coverage begins in 2015, so the sample is the recovery/COVID era. Standard errors in parentheses.

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

Table OA24: Experience Gradient vs First-Timer Discontinuity

| Offense   | Fixed effects | $\gamma_E$ (experienced slope) | $\gamma_0$ (first-timer) |
|-----------|---------------|--------------------------------|--------------------------|
| Narcotics | No agent FE   | -0.12 (0.07)                   | -0.02 (0.10)             |
| Narcotics | Agent FE      | -0.05 (0.05)                   | 0.02 (0.07)              |
| Vandalism | No agent FE   | -0.04 (0.03)                   | 0.18*** (0.07)           |
| Vandalism | Agent FE      | -0.01 (0.03)                   | 0.14** (0.06)            |
| Homicide  | No agent FE   | 0.09 (0.74)                    | 1.75 (1.61)              |
| Homicide  | Agent FE      | -0.21 (0.56)                   | 1.58 (1.25)              |

*Notes.* Asking-price experience gradient with a first-time-in-tract dummy (40.8% of listings), at the 12-month window, per unit of  $E$ .  $\gamma_E$  is the slope among agents with prior experience;  $\gamma_0$  is the first-timer differential in the near-far ask. For narcotics  $\gamma_0$  is null, but  $\gamma_E$  attenuates and is not significant once the first-timer differential is allowed for. For vandalism  $\gamma_E$  collapses to zero and  $\gamma_0$  is positive and significant: first-time agents ask *more* (a discontinuity, not experience accumulating). Homicide is too sparse to separate the two. Standard errors in parentheses.

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

Table OA25: Agent Local versus Total Experience

| Offense                                                                 | Window | Local experience<br>gradient (s.e.) | Total experience<br>gradient (s.e.) | Sig. windows<br>local / total |
|-------------------------------------------------------------------------|--------|-------------------------------------|-------------------------------------|-------------------------------|
| <i>Panel A. Listing agent, asking price (<math>N = 972,629</math>)</i>  |        |                                     |                                     |                               |
| Homicide                                                                | 10     | -0.79** (0.35)                      | -0.45 (0.49)                        | 1/12 / 3/12                   |
| Burglary                                                                | 8      | 0.02 (0.04)                         | -0.06 (0.09)                        | 0/12 / 0/12                   |
| Narcotics                                                               | 10     | -0.02 (0.03)                        | -0.21*** (0.05)                     | 0/12 / 12/12                  |
| Vandalism                                                               | 6      | -0.04 (0.03)                        | 0.04 (0.04)                         | 3/12 / 0/12                   |
| <i>Panel B. Buyer's agent, closing price (<math>N = 186,510</math>)</i> |        |                                     |                                     |                               |
| Homicide                                                                | 10     | -1.99** (0.86)                      | 0.41 (0.46)                         | 5/12 / 0/12                   |
| Burglary                                                                | 8      | 0.15 (0.12)                         | -0.07 (0.05)                        | 2/12 / 1/12                   |
| Narcotics                                                               | 10     | 0.11 (0.11)                         | -0.05 (0.07)                        | 1/12 / 0/12                   |
| Vandalism                                                               | 6      | 0.15* (0.08)                        | -0.01 (0.05)                        | 1/12 / 0/12                   |

*Notes.* Local experience  $E$  (prior same-agent listings in the tract) and total experience  $T$  (all prior listings) enter the same equation, each interacted with near and far exposure. Gradients are  $(\text{near} \times E - \text{far} \times E) \times 100$  and  $(\text{near} \times T - \text{far} \times T) \times 100$  per standard deviation, at each offense's central window; last column counts windows significant at five percent. Design as in Table 5. Correlation of  $E$  and  $T$ : 0.36 (listing), 0.37 (buyer).

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

Table OA26: The Quantity Margin: Listings and Sales

| Offense                                                                | P(listed)<br>(pp) | P(sold)<br>(pp)    | Gap<br>(pp)       |
|------------------------------------------------------------------------|-------------------|--------------------|-------------------|
| <i>Positive gap (listing response exceeds completed-sale response)</i> |                   |                    |                   |
| Narcotics                                                              | 0.02***<br>(0.01) | 0.00<br>(0.00)     | 0.03***<br>(0.00) |
| Burglary                                                               | 0.06***<br>(0.01) | 0.02***<br>(0.01)  | 0.04***<br>(0.01) |
| Arson                                                                  | 0.13**<br>(0.05)  | -0.01<br>(0.04)    | 0.14***<br>(0.04) |
| Assault (domestic)                                                     | 0.00<br>(0.01)    | -0.02***<br>(0.00) | 0.02***<br>(0.01) |
| Homicide                                                               | 0.10**<br>(0.05)  | -0.04<br>(0.04)    | 0.14***<br>(0.04) |
| Vandalism                                                              | 0.02***<br>(0.01) | 0.01<br>(0.01)     | 0.02***<br>(0.01) |
| Assault (non-dom.)                                                     | 0.01<br>(0.01)    | -0.01<br>(0.01)    | 0.01***<br>(0.00) |
| Weapons                                                                | 0.03*<br>(0.02)   | -0.01<br>(0.01)    | 0.04***<br>(0.01) |
| Gambling                                                               | 0.15**<br>(0.07)  | -0.02<br>(0.05)    | 0.17***<br>(0.06) |
| Disorderly conduct                                                     | 0.08***<br>(0.03) | 0.04<br>(0.03)     | 0.04**<br>(0.02)  |
| <i>No gap</i>                                                          |                   |                    |                   |
| Trespass                                                               | 0.01<br>(0.02)    | 0.00<br>(0.01)     | 0.01<br>(0.01)    |
| Liquor law                                                             | 0.00<br>(0.07)    | -0.07<br>(0.05)    | 0.06<br>(0.06)    |
| Motor-vehicle theft                                                    | -0.02<br>(0.02)   | -0.01<br>(0.01)    | -0.01<br>(0.01)   |
| Family (non-violent)                                                   | 0.05<br>(0.03)    | 0.02<br>(0.03)     | 0.02<br>(0.02)    |
| Forgery                                                                | 0.06<br>(0.04)    | 0.03<br>(0.03)     | 0.03<br>(0.03)    |
| Bad checks                                                             | -0.05<br>(0.05)   | -0.01<br>(0.05)    | -0.04<br>(0.06)   |
| Stolen property                                                        | 0.08<br>(0.14)    | 0.15<br>(0.12)     | -0.07<br>(0.11)   |
| Fraud                                                                  | -0.02<br>(0.01)   | -0.01<br>(0.01)    | -0.01<br>(0.01)   |
| Robbery                                                                | 0.01<br>(0.01)    | 0.01<br>(0.01)     | 0.01<br>(0.01)    |
| Larceny/theft                                                          | 0.00<br>(0.00)    | 0.00<br>(0.00)     | 0.00<br>(0.00)    |
| Prostitution                                                           | -0.02<br>(0.02)   | -0.02**<br>(0.01)  | 0.00<br>(0.02)    |
| Sex offenses                                                           | -0.01<br>(0.02)   | -0.01<br>(0.02)    | 0.00<br>(0.02)    |

*Notes.* Near-far effect on the annual probability a parcel is *listed* and that it *sells*, in percentage points (s.e. in parentheses; base rates 6.0 and 3.8 percent). P(sold) is the annual turnover rate at the parcel level, the object measured at the tract level by Boggess et al. (2013). The *gap* is listed minus sold, so a positive gap means the listing response exceeds the completed-sale response; it is not a direct withdrawal estimate (withdrawal: Table OA21). Each annual parcel-year regression includes tract and community-area  $\times$  month fixed effects and property controls (log building and land area, bedrooms, full and half bathrooms, year built); standard errors are clustered by tract. Parcel fixed effects are omitted so that never-listing parcels remain in the panel.  $N = 13,682,888$  parcel-years.

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

## F Rental Market

Table OA27: The Rental Market: Full Offense Battery

| 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>No consistent rent effect (near zero; prostitution's parcel×era minimum is significant at one window)</i> |                 |               |              |                 |                |              |
| Narcotics <sup>†</sup>                                                                                       | 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)     |
| Larceny/theft <sup>†</sup>                                                                                   | −0.01** (0.00)  | 0.00 (0.01)   | 6/12 (2)     | −0.02 (0.01)    | 0.00 (0.01)    | 0/12 (0)     |
| Assault (non-dom.)                                                                                           | −0.06 (0.04)    | −0.03 (0.02)  | 2/12 (0)     | −0.05 (0.04)    | −0.01 (0.02)   | 0/12 (0)     |
| Motor-vehicle theft                                                                                          | −0.12 (0.08)    | −0.01 (0.04)  | 0/12 (0)     | −0.13 (0.09)    | −0.01 (0.03)   | 0/12 (0)     |
| Robbery                                                                                                      | 0.01 (0.04)     | 0.07 (0.06)   | 0/12 (0)     | 0.04 (0.05)     | 0.12 (0.11)    | 0/12 (0)     |
| Assault (dom.) <sup>†</sup>                                                                                  | −0.03 (0.05)    | 0.02 (0.09)   | 0/12 (5)     | −0.06 (0.11)    | 0.03 (0.08)    | 0/12 (0)     |
| Fraud                                                                                                        | −0.01 (0.04)    | 0.08 (0.06)   | 0/12 (0)     | 0.03 (0.04)     | 0.07*** (0.03) | 0/12 (1)     |
| Trespass                                                                                                     | −0.17* (0.10)   | 0.01 (0.09)   | 0/12 (0)     | −0.13 (0.10)    | 0.05 (0.10)    | 0/12 (0)     |
| Forgery                                                                                                      | −0.08 (0.15)    | 0.12 (0.10)   | 0/12 (0)     | 0.06 (0.16)     | 0.22 (0.16)    | 0/12 (0)     |
| 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 (too sparse near leases to sign)</i>                                             |                 |               |              |                 |                |              |
| Homicide                                                                                                     | −0.55 (0.61)    | 1.03 (1.20)   | 0/12 (0)     | −0.59 (0.59)    | 1.16 (1.22)    | 0/12 (0)     |
| Disorderly conduct                                                                                           | −0.40 (0.30)    | 0.15 (0.25)   | 0/12 (0)     | −0.25 (0.16)    | 0.40 (0.29)    | 0/12 (0)     |
| Weapons                                                                                                      | 0.02 (0.26)     | 0.17 (0.22)   | 0/12 (0)     | −0.08 (0.23)    | 0.22 (0.22)    | 0/12 (0)     |
| Arson                                                                                                        | −0.21 (0.47)    | 0.20 (0.47)   | 0/12 (0)     | −0.27 (0.43)    | 0.54 (0.85)    | 0/12 (0)     |
| Bad checks <sup>†</sup>                                                                                      | −0.03 (0.18)    | 0.82** (0.37) | 0/12 (3)     | −0.16 (0.17)    | 0.67 (0.43)    | 0/12 (0)     |
| Family (non-violent)                                                                                         | 0.07 (0.22)     | 0.36* (0.21)  | 0/12 (0)     | 0.11 (0.38)     | 0.68 (0.50)    | 0/12 (0)     |
| Kidnapping <sup>†</sup>                                                                                      | −0.70* (0.36)   | −0.30 (0.27)  | 0/12 (3)     | −0.62 (0.72)    | 0.05 (0.29)    | 0/12 (0)     |
| Embezzlement                                                                                                 | −0.39 (0.34)    | 0.96 (0.94)   | 0/12 (0)     | −0.06 (0.48)    | 0.94 (0.83)    | 0/12 (0)     |
| Child pornography/obscenity                                                                                  | −0.02 (0.56)    | 0.40 (0.74)   | 0/12 (0)     | −0.34 (1.06)    | 0.34 (0.51)    | 0/12 (0)     |
| Extortion <sup>†</sup>                                                                                       | −1.30* (0.70)   | −0.28 (0.33)  | 2/12 (3)     | −1.13** (0.51)  | −0.19 (0.26)   | 2/12 (5)     |
| Gambling <sup>†</sup>                                                                                        | −1.50 (1.44)    | 3.54 (2.25)   | 0/12 (4)     | −1.25 (1.51)    | 2.88 (4.05)    | 0/12 (12)    |

*Notes.* Asking-rent near-far estimates on lease listings for the full offense battery, the near ring against the 0.20–0.30 mile annulus, pooled 2007–2025 with tract×listing-month fixed effects, clustered by tract (118,849 listings under parcel fixed effects; 98,813 under parcel×era). Under each fixed-effect specification, parcel (PIN) and parcel×era, Min and Max are the most negative and most positive estimate across the twelve exposure windows (standard error in parentheses), and Sig. counts windows significant at five percent; the parenthetical is the number of windows in which the post-date placebo (crime after the listing) is significant. A dagger (†) marks an offense whose placebo fails in two or more windows, shown only for completeness. Priced: asking rent lower, two or more significant windows under an identifying fixed-effect specification with a clean placebo. The compact version is Table 6. “Child pornography/obscenity” is predominantly child pornography (IUCR 1582). Non-domestic assault meets the window rule (2/12, clean placebo), but its estimates are small (about −0.06%) and it is not counted as priced.

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

Table OA28: Rental Agent Local Experience (Letting Agent)

| Offense               | Discount           | Experience grad./SD  |
|-----------------------|--------------------|----------------------|
| <i>Asking rent</i>    |                    |                      |
| Sex offenses          | −0.21**<br>(0.09)  | −0.048<br>(0.065)    |
| Prostitution          | −0.15<br>(0.11)    | −0.012<br>(0.072)    |
| Liquor law            | −0.27***<br>(0.10) | −0.021<br>(0.059)    |
| Burglary              | −0.11*<br>(0.06)   | 0.047<br>(0.035)     |
| Narcotics             | 0.10<br>(0.08)     | −0.035*<br>(0.019)   |
| <i>Days on market</i> |                    |                      |
| Sex offenses          | 1.07<br>(0.75)     | −0.085<br>(0.632)    |
| Prostitution          | −2.00***<br>(0.69) | 0.922<br>(1.029)     |
| Liquor law            | 1.06<br>(1.81)     | −0.011<br>(0.860)    |
| Burglary              | 0.68<br>(0.46)     | 0.322<br>(0.451)     |
| Narcotics             | 1.01<br>(0.72)     | −0.586***<br>(0.215) |
| <i>Withdrawal</i>     |                    |                      |
| Sex offenses          | −0.11<br>(0.32)    | −0.407**<br>(0.185)  |
| Prostitution          | −0.60*<br>(0.34)   | 0.325<br>(0.340)     |
| Liquor law            | 0.04<br>(0.43)     | 0.515<br>(0.382)     |
| Burglary              | −0.04<br>(0.19)    | 0.049<br>(0.117)     |
| Narcotics             | 0.10<br>(0.18)     | 0.009<br>(0.113)     |

*Notes.* Near–far effect on lease listings interacted with the letting (listing) agent’s local experience  $E = \ln(1 + \text{prior same-agent same-tract lease listings})$ , standardized. Outcomes are  $\ln(\text{asking rent})$ ,  $\ln(\text{days on market})$ , and  $P(\text{withdrawal})$ . Parcel $\times$ era + tract $\times$ month + agent (email) fixed effects, clustered by tract; each offense at its rental representative window. “Discount” is the near–far effect at mean experience; “Experience grad./SD” is the change per SD of  $E$ . Standard errors in parentheses.

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

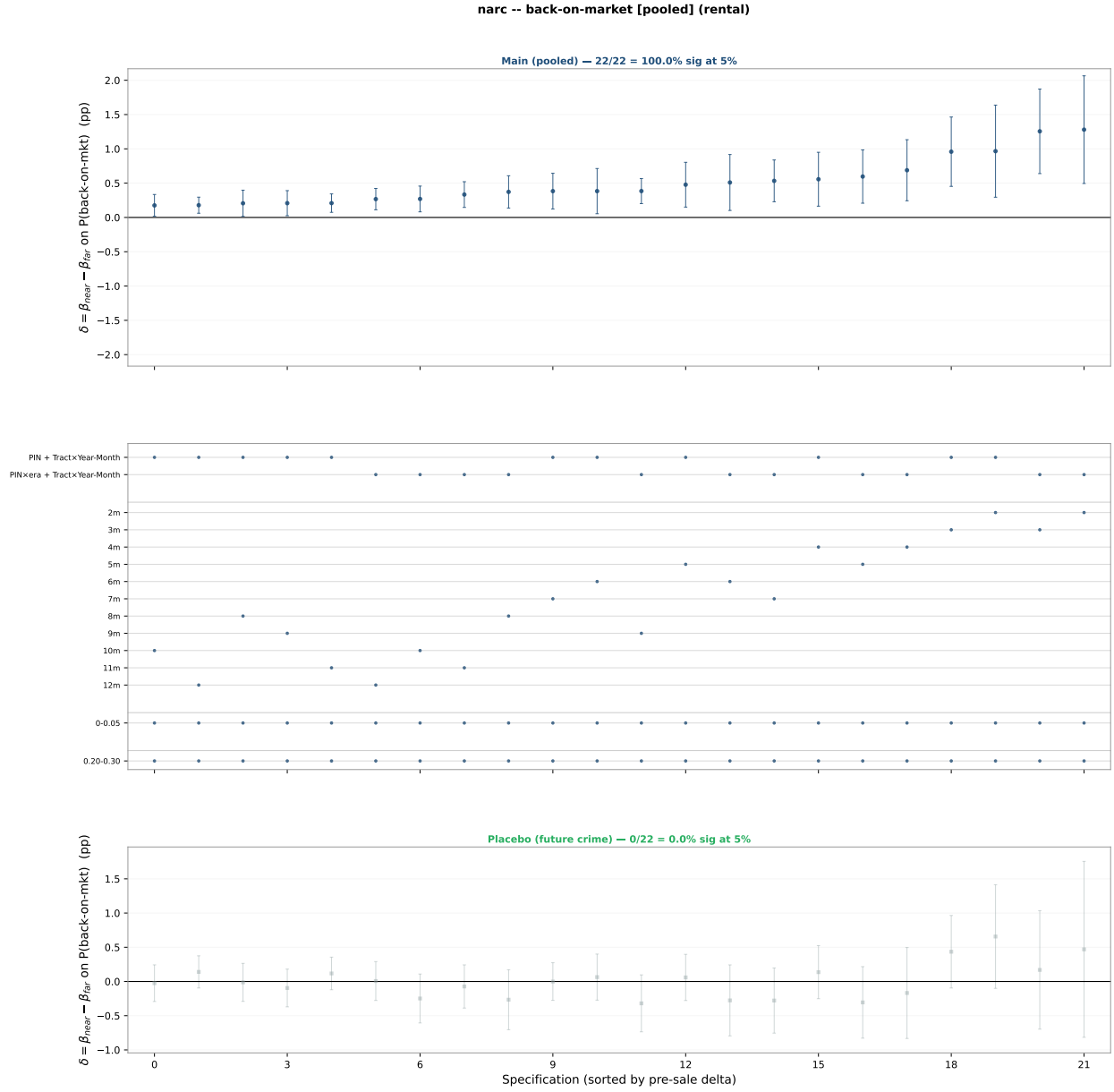


Figure OF3: Rental Leasing Friction: Back-on-Market Response to Nearby Narcotics

*Notes.* Specification-curve “mirror” of the near–far effect ( $\delta = \beta_{\text{near}} - \beta_{\text{far}}$ , near 0–0.05 vs. far 0.20–0.30 mi) of nearby narcotics on the probability a lease returns to the market (percentage points), MLS lease listings, 2007–2025, clustered by tract. The top panel sorts the twenty-two specifications (eleven exposure windows, two to twelve months,  $\times$  two fixed-effect designs: parcel + tract×month, and parcel×era + tract×month) by point estimate with 95% confidence intervals; the decoder panel identifies each specification; the bottom panel is the future-crime placebo (crime dated after the listing). Main: 22/22 significant at 5%; placebo: 0/22.

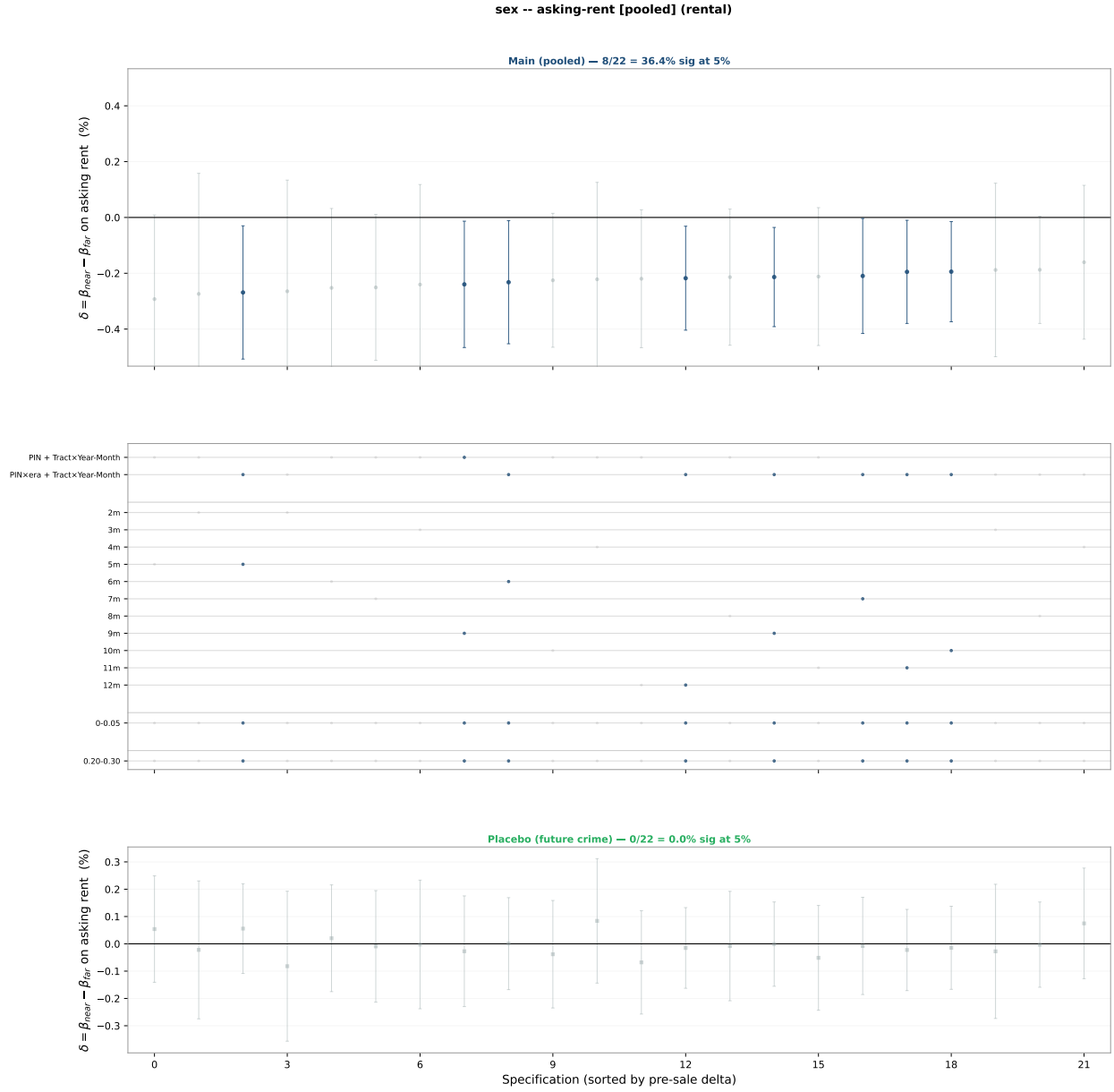


Figure OF4: Rental Asking-Rent Response to Nearby Sex Offenses

*Notes.* Specification-curve “mirror” of the near–far effect ( $\delta = \beta_{\text{near}} - \beta_{\text{far}}$ , near 0–0.05 vs. far 0.20–0.30 mi) of nearby sex offenses on  $\ln(\text{asking rent})$ , percent, MLS lease listings, 2007–2025, clustered by tract. Twenty-two specifications (eleven exposure windows, two to twelve months,  $\times$  parcel and parcel $\times$ era fixed effects), 95% confidence intervals; bottom panel is the future-crime placebo. Main: 8/22 significant at 5% (all in the discount direction); placebo: 0/22.

## G Heterogeneity

Table OA29: SES Heterogeneity Gradients, All Offenses

| Offense             | Inc     | Pov      | Col     | Own    | HV      | Rent    | Blk      | His      | Seg      | Vac   |
|---------------------|---------|----------|---------|--------|---------|---------|----------|----------|----------|-------|
| Homicide            | 2.28**  | -1.72    | 3.02**  | -0.43  | 2.11    | 1.82**  | -2.99*   | -0.11    | -2.10    | -0.56 |
| Burglary            | 0.36*** | -0.43**  | 0.52**  | 0.02   | 0.40**  | 0.30*** | -0.61*   | -0.40*   | -0.43    | -0.13 |
| Narcotics           | 0.26*** | -0.29*** | 0.25*** | 0.19   | 0.44*** | 0.18**  | -0.27**  | 0.06     | -0.15    | 0.07  |
| Vandalism           | 0.15**  | -0.04    | 0.31**  | -0.17  | 0.18    | 0.13**  | -0.46**  | -0.04    | -0.60*** | -0.12 |
| Theft               | 0.02**  | -0.04    | 0.04    | -0.02  | 0.03    | 0.01*   | -0.02    | -0.01    | 0.01     | 0.02  |
| Motor-vehicle theft | -0.13   | 0.30**   | -0.12   | -0.16  | -0.10   | -0.10   | 0.17     | -0.05    | 0.01     | 0.00  |
| Robbery             | 0.12    | -0.34    | 0.24    | 0.46** | 0.03    | 0.14    | 0.05     | -0.57**  | 0.05     | -0.08 |
| Assault (non-dom.)  | 0.08*   | -0.11    | 0.21**  | 0.00   | 0.12    | 0.08**  | -0.26*   | -0.10    | -0.26*   | -0.03 |
| Assault (dom.)      | 0.14    | 0.00     | 0.26**  | 0.03   | 0.11    | 0.12*   | -0.15    | -0.16    | -0.09    | -0.03 |
| Disorderly conduct  | 0.29    | -0.13    | 0.48    | -0.49  | 0.23    | 0.29    | -1.04    | 0.79     | -0.81    | 0.18  |
| Trespass            | 0.31*** | -0.64*** | 1.02*** | 0.15   | 0.58*** | 0.19*** | -0.95*** | -0.74*** | -1.06**  | 0.04  |
| Prostitution        | 0.13    | -0.09    | 0.22    | 0.07   | 0.08    | 0.02    | -0.25    | 0.29     | -0.77    | 0.15  |
| Fraud               | 0.25**  | -0.37**  | 0.63*** | 0.20*  | 0.23**  | 0.19**  | -1.16*** | -0.40    | -0.29    | -0.07 |
| Sex offenses        | -0.09   | -0.13    | 0.38    | -0.17  | -0.14   | -0.12   | -0.58    | -0.33    | -0.87    | -0.29 |
| Forgery             | 0.04    | -0.35    | -0.30   | -0.17  | -0.07   | -0.04   | 0.67     | 0.34     | 0.23     | 0.23  |
| Weapons             | -0.17   | 0.03     | -0.10   | -0.13  | -0.14   | 0.01    | 0.13     | -0.08    | 0.38     | -0.14 |
| Arson               | 0.02    | 0.45     | 1.06    | -2.71  | 1.86    | -0.11   | -2.94    | 2.24     | -3.17    | 0.71  |
| Stolen property     | 1.46    | -4.31*** | 3.16*   | -0.25  | 1.59**  | 1.28    | -3.74    | -3.52    | 0.95     | 0.66  |
| Liquor              | 1.01*   | -0.88    | 1.21    | 0.36   | 1.02**  | 1.03**  | -2.82    | 0.09     | -0.40    | 0.26  |
| Gambling            | -0.18   | -0.86    | -0.60   | 1.84   | -1.72   | -0.76   | 0.37     | -1.17    | 1.05     | 2.02  |
| Bad checks          | -0.27   | 0.60     | -0.91   | 0.12   | 0.40    | -0.21   | 0.77     | 0.57     | 1.37     | 0.16  |
| Family offenses     | -0.43   | 0.79     | -1.13   | -0.73  | -0.43   | -0.01   | 1.29     | 0.25     | 1.74*    | 1.17* |

*Notes.* Per-SD interaction gradient (% change in the near-far discount per SD of the tract moderator) for all estimable offenses; continuous interaction,  $z$  standardized over the sale sample, fully interacted, each gradient net of the tract's predetermined crime level (also interacted), parcel and tract $\times$ month fixed effects, clustered by tract. Columns: Inc=median household income, Pov=poverty rate, Col=% college-plus, Own=% owner-occupied, HV=median home value, Rent=median gross rent, Blk=% Black, His=% Hispanic, Seg=racial segregation (divergence) index, Vac=vacancy rate.

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

## H Inference and Specification Robustness

Table OA30: Arm's-Length versus Distressed Sales: Pooled Decomposition

| Offense            | Window | Arm's-length (%)   | Distressed (%)   | $p(\text{diff})$ |
|--------------------|--------|--------------------|------------------|------------------|
| Homicide           | 10     | -1.78*<br>(1.08)   | -8.99<br>(4.74)  | 0.128            |
| Burglary           | 8      | -0.12<br>(0.10)    | -0.73<br>(0.52)  | 0.254            |
| Narcotics          | 10     | -0.00<br>(0.13)    | -0.64<br>(0.26)  | 0.026            |
| Vandalism          | 6      | -0.09<br>(0.07)    | -0.69<br>(0.45)  | 0.186            |
| Disorderly conduct | 12     | -0.95***<br>(0.36) | 0.58<br>(2.09)   | 0.469            |
| Prostitution       | 11     | -0.29<br>(0.26)    | 0.89<br>(0.63)   | 0.111            |
| Weapons            | 1      | -0.23<br>(1.26)    | -5.01<br>(12.85) | 0.711            |
| Arson              | 4      | -2.36<br>(2.56)    | -1.79<br>(10.54) | 0.958            |

*Notes.* Near-far discount  $\delta = (\beta_{\text{near}} - \beta_{\text{far}}) \times 100$  on  $\ln(\text{sale price})$ , pooled 2007–2025, at each offense's headline window (months). Arm's-length and distressed discounts come from one regression with a distress interaction;  $p(\text{diff})$  is the Wald test of their equality. Arm's-length = ordinary buyer-chosen sales; distressed = REO, short-sale, or foreclosure (about 13 percent of transactions). Parcel and tract  $\times$  year-month fixed effects, clustered by tract. Standard errors in parentheses; stars on the arm's-length estimate. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Table OA31: Sale Prices: Alternative Standard Errors

| Crime                              | $\delta$ (%) | Tract  | Conley 0.5 km | Conley 1.0 km<br>standard error | Community area |
|------------------------------------|--------------|--------|---------------|---------------------------------|----------------|
| <i>Panel A. Era-salient window</i> |              |        |               |                                 |                |
| Homicide                           | -4.09***     | (1.46) | (0.89)        | (0.93)                          |                |
| Burglary                           | -0.50*       | (0.26) | (0.14)        | (0.14)                          |                |
| Narcotics                          | -0.41**      | (0.17) | (0.11)        | (0.11)                          |                |
| Vandalism                          | -0.43**      | (0.22) | (0.12)        | (0.12)                          |                |
| <i>Panel B. Pooled (all eras)</i>  |              |        |               |                                 |                |
| Homicide                           | -3.02**      | (1.38) | (0.78)        | (0.83)                          | (1.40)         |
| Burglary                           | -0.24**      | (0.12) | (0.06)        | (0.07)                          | (0.12)         |
| Narcotics                          | -0.29**      | (0.14) | (0.08)        | (0.08)                          | (0.19)         |
| Vandalism                          | -0.17**      | (0.08) | (0.05)        | (0.05)                          | (0.09)         |

*Notes.* Near-far close-price discount  $\delta$  (%) for each priced offense, estimated on the full 2007–2025 repeat-sales sample ( $N = 242,815$ ) with parcel and tract $\times$ month fixed effects. Panel A reports the era-salient near-far contrast from a model with era interactions (homicide recovery, window 10; burglary, narcotics, and vandalism crisis, windows 8, 12, and 7). Panel B reports the pooled (all-era) discount, the headline specification of Table 2 (homicide at window 10, burglary 8, narcotics 10, vandalism 6). Column 2 is the baseline standard error clustered by census tract. Columns 3–4 report Conley (1999) spatial heteroskedasticity- and autocorrelation-consistent standard errors (Bartlett kernel, 0.5 km and 1.0 km cutoffs), computed on residuals after absorbing the fixed effects, with no degrees-of-freedom correction. Column 5 clusters by Chicago’s 77 community areas (pooled specification only);  $p$ -values: homicide 0.034, burglary 0.043, narcotics 0.118, vandalism 0.059. Significance stars use the tract-clustered standard errors.

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

Table OA32: Higher-Moment Dummy Test at the Zero-Crime Mass Point

|                                                             | Narcotics           | Vandalism             | Burglary               |
|-------------------------------------------------------------|---------------------|-----------------------|------------------------|
| <i>Panel A. Variance (<math>\hat{\varepsilon}^2</math>)</i> |                     |                       |                        |
| Raw residual                                                | 0.0001<br>(0.0006)  | -0.0004<br>(0.0003)   | -0.0001<br>(0.0004)    |
| Winsorized (1/99)                                           | 0.0001<br>(0.0002)  | 0.0000<br>(0.0002)    | -0.0001<br>(0.0002)    |
| <i>Panel B. Skewness (<math>\hat{\varepsilon}^3</math>)</i> |                     |                       |                        |
| Raw residual                                                | -0.0002<br>(0.0009) | -0.0015**<br>(0.0007) | -0.0022***<br>(0.0009) |
| Winsorized (1/99)                                           | 0.0002<br>(0.0003)  | -0.0002<br>(0.0002)   | -0.0007**<br>(0.0003)  |
| Observations                                                | 242,815             |                       |                        |

*Notes.* Coefficient on the bunching-point indicator  $\mathbf{1}(\text{near} = 0)$  added to the conditioned second and third moments of the price residual (Caetano et al., 2021 higher-moment version), at the headline PIN + tract $\times$ close-month design; standard errors clustered by tract in parentheses. A null coefficient means the moment is continuous at the mass point. The variance, the signature of heterogeneous selection, is a precise null for all three crimes. Raw-residual skewness flags for vandalism and burglary are driven by tail sales; vandalism’s vanishes under winsorizing, while burglary’s remains significant at the five-percent level (-0.0007). \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

## H.1 Full Specification Curves (All Exposure Windows)

Figures OF5–OF8 report the sale-price specification curve for the four priced offenses across all 103 specifications. The top panel sorts the doorstep near–far discount  $\delta$  (%) across the grid: 72 specifications vary the exposure window (1–12 months), the fixed-effect design (parcel + tract  $\times$  month, or parcel  $\times$  era + tract  $\times$  month), and the far ring (0.20–0.30, 0.25–0.35, 0.30–0.40 mi) under a linear count with no controls; 24 add the full predetermined-control set at the 0.20–0.30 ring; 5 add one control at a time at the headline specification; and 2 replace the linear count with a presence indicator or an inverse hyperbolic sine. Navy marks estimates significant and negative at five percent; maroon marks the main specification. The middle panel decodes each specification; the bottom panel is the post-sale placebo. Pooled 2007–2025, clustered by tract.

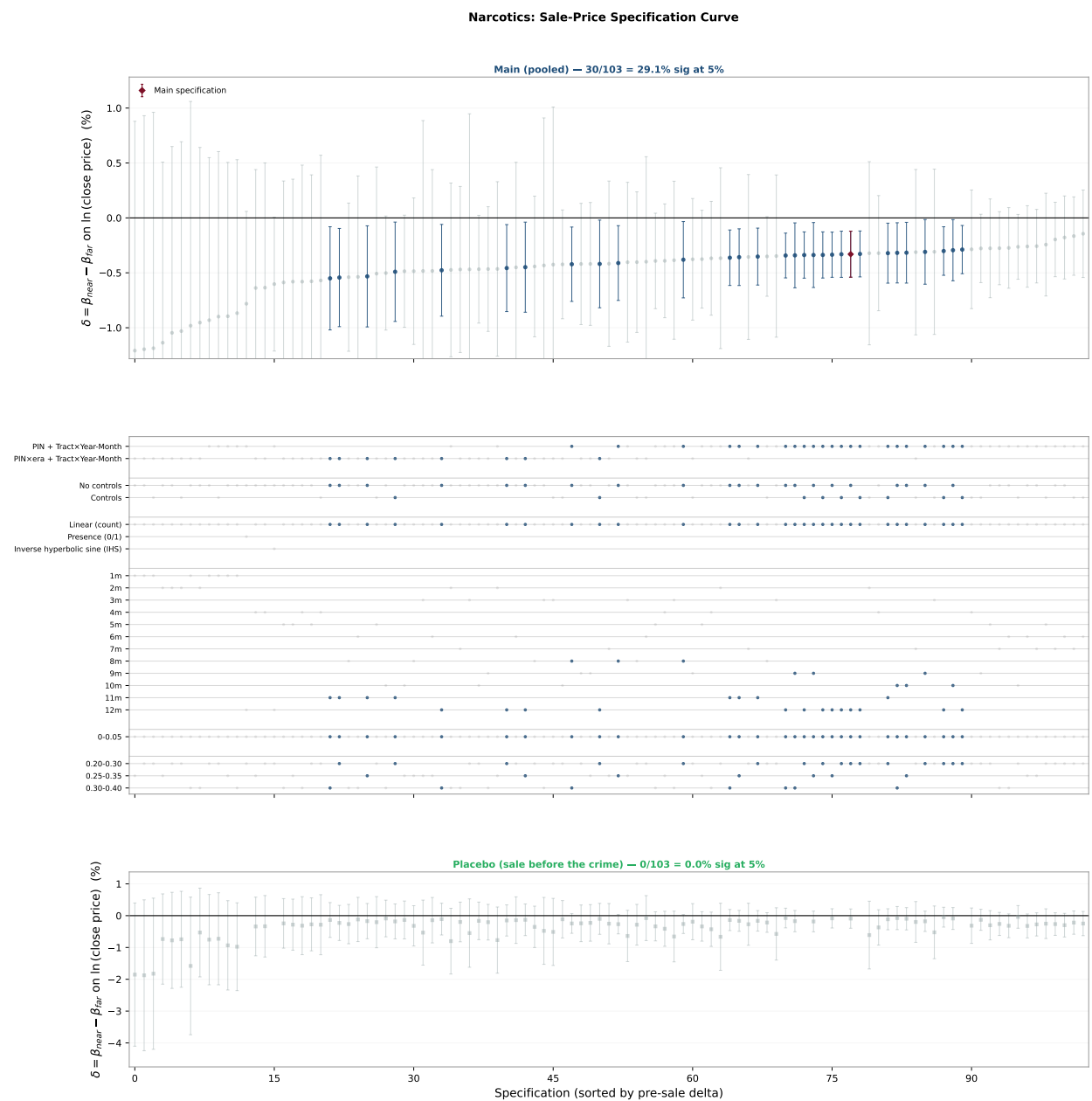


Figure OF5: Narcotics: Sale-Price Discount Across All 103 Specifications. Main: 30/103 significant; placebo 0/103.

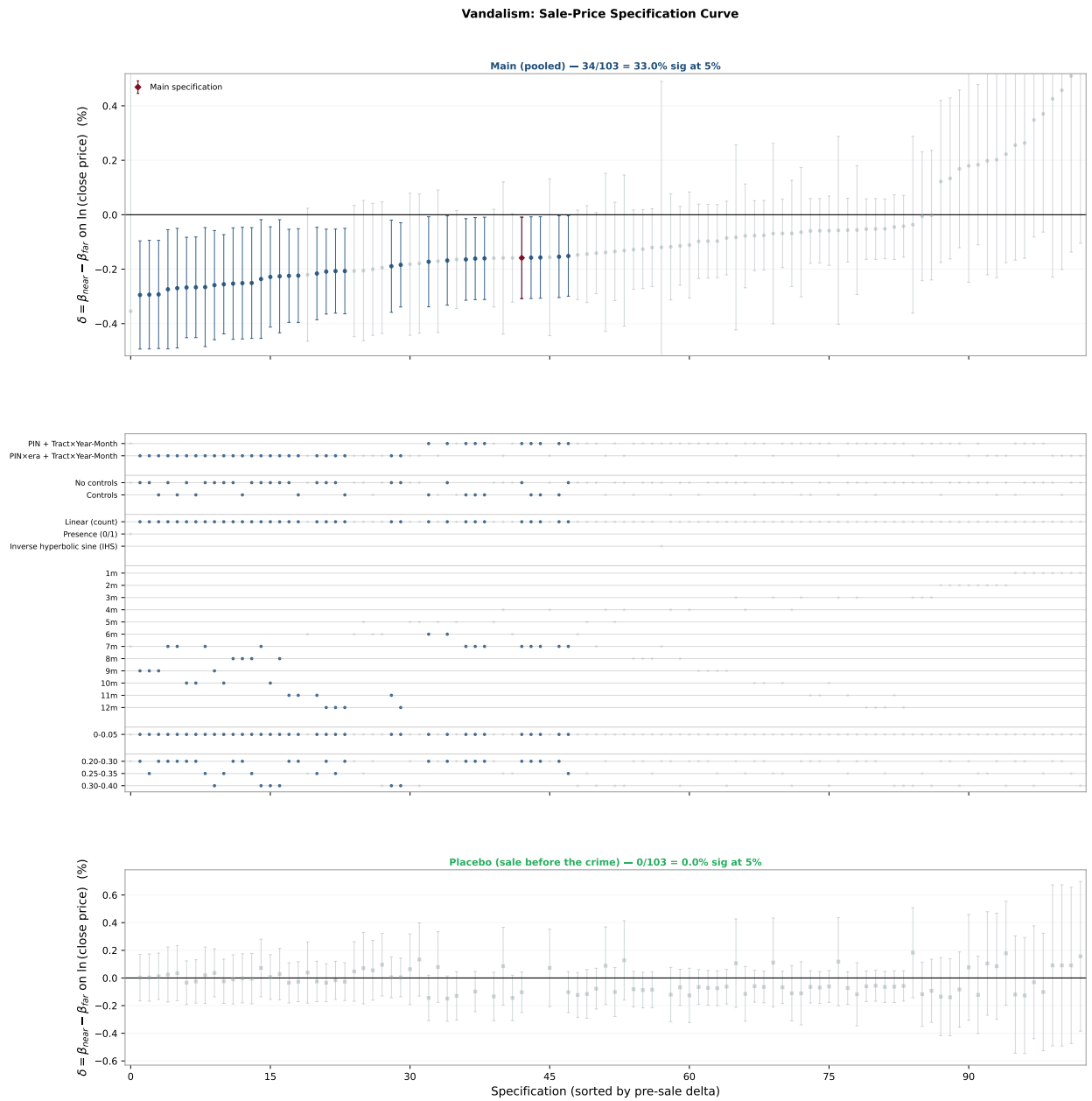


Figure OF6: Vandalism: Sale-Price Discount Across All 103 Specifications. Main: 34/103 significant; placebo 0/103.

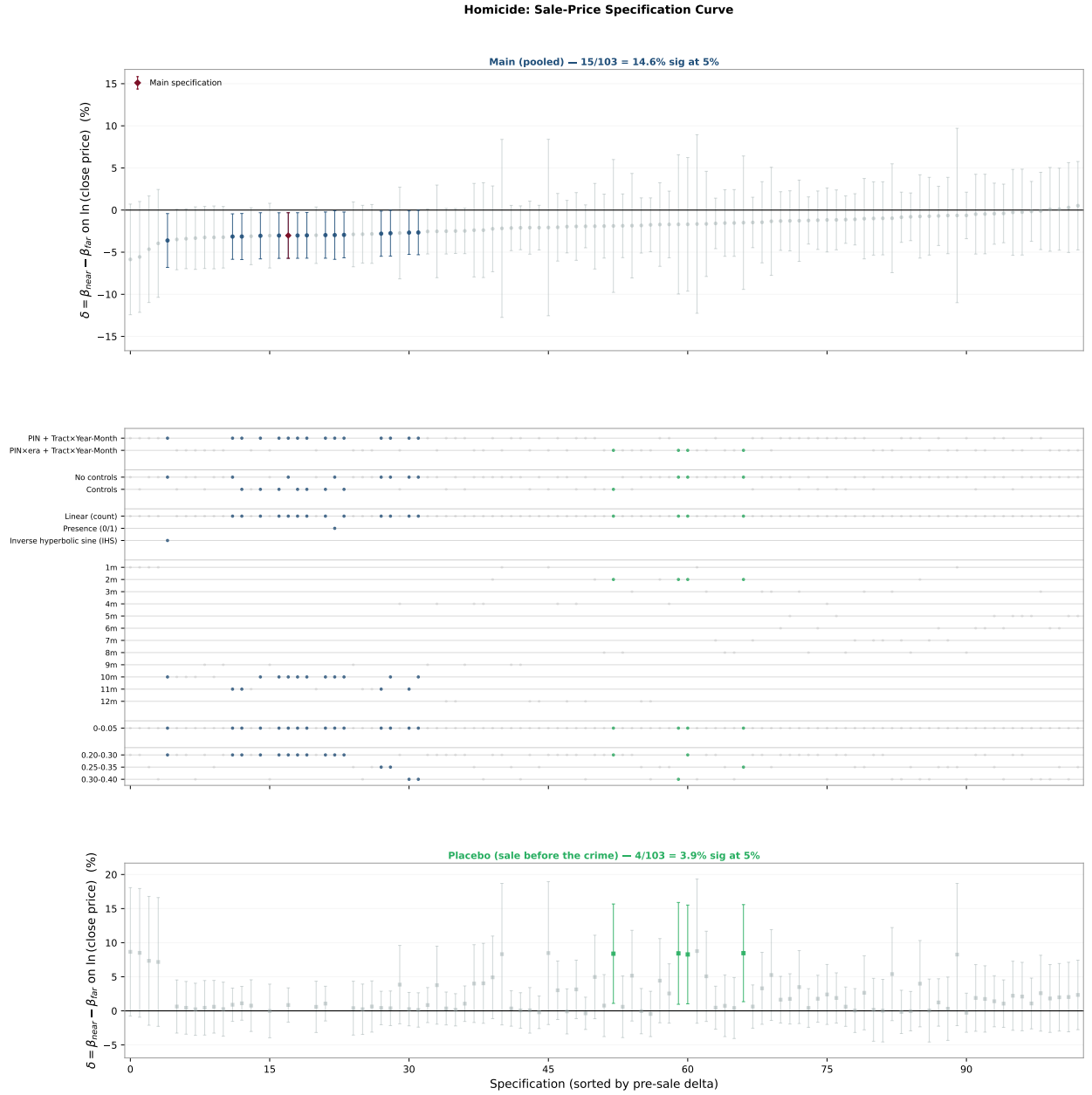


Figure OF7: Homicide: Sale-Price Discount Across All 103 Specifications. Main: 15/103 significant; placebo 4/103, all positive and at the two-month window.

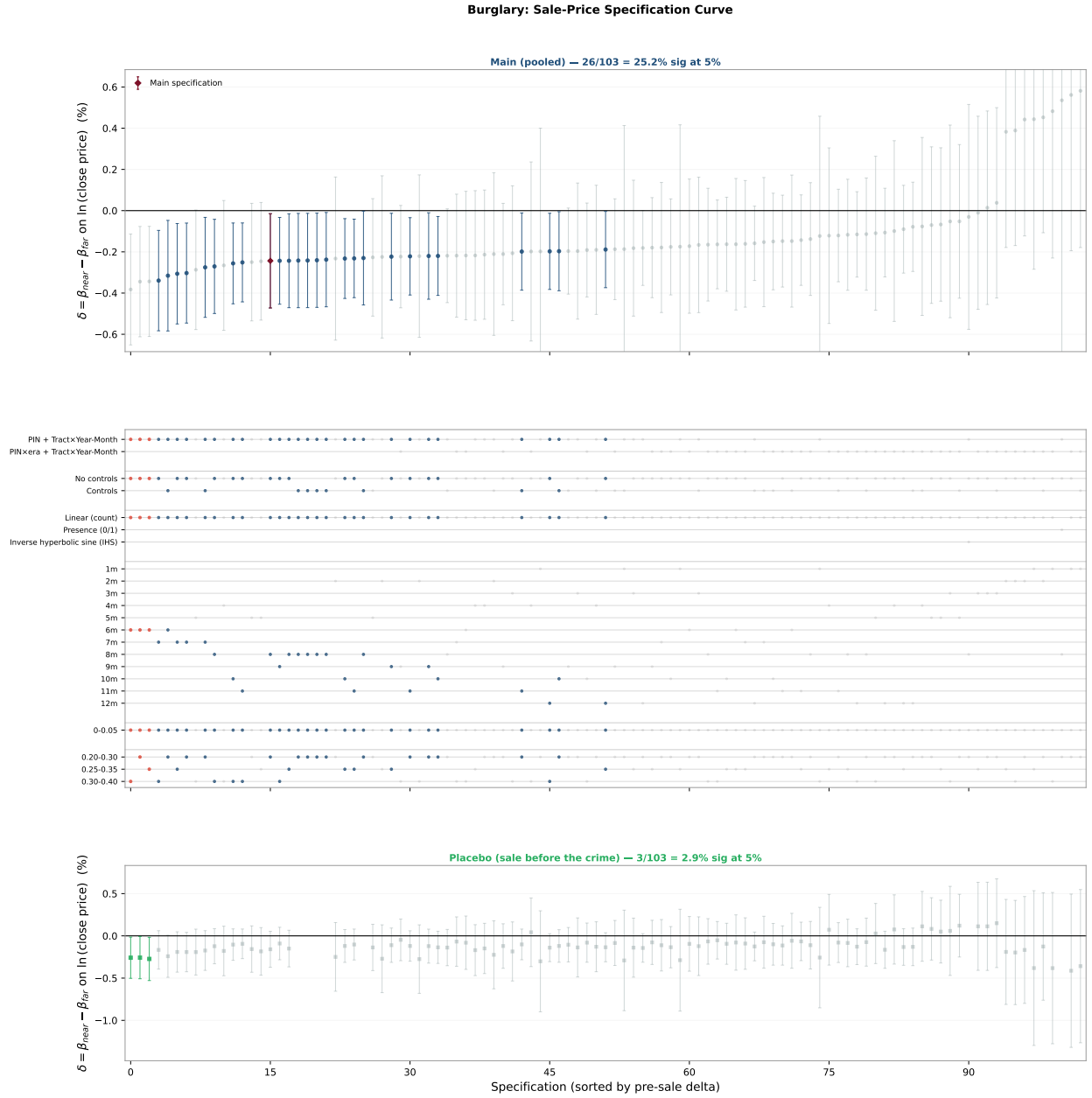


Figure OF8: Burglary: Sale-Price Discount Across All 103 Specifications. Main: 26/103 significant, all in the discount direction; placebo 3/103.

## H.2 Off-Doorstep Falsification

Moving the treatment band outward from the doorstep (0–0.05 miles) to 0.05–0.20 miles, against the same 0.20–0.30 mile comparison ring, Figure OF9 reports the 48-specification curve for each priced offense.

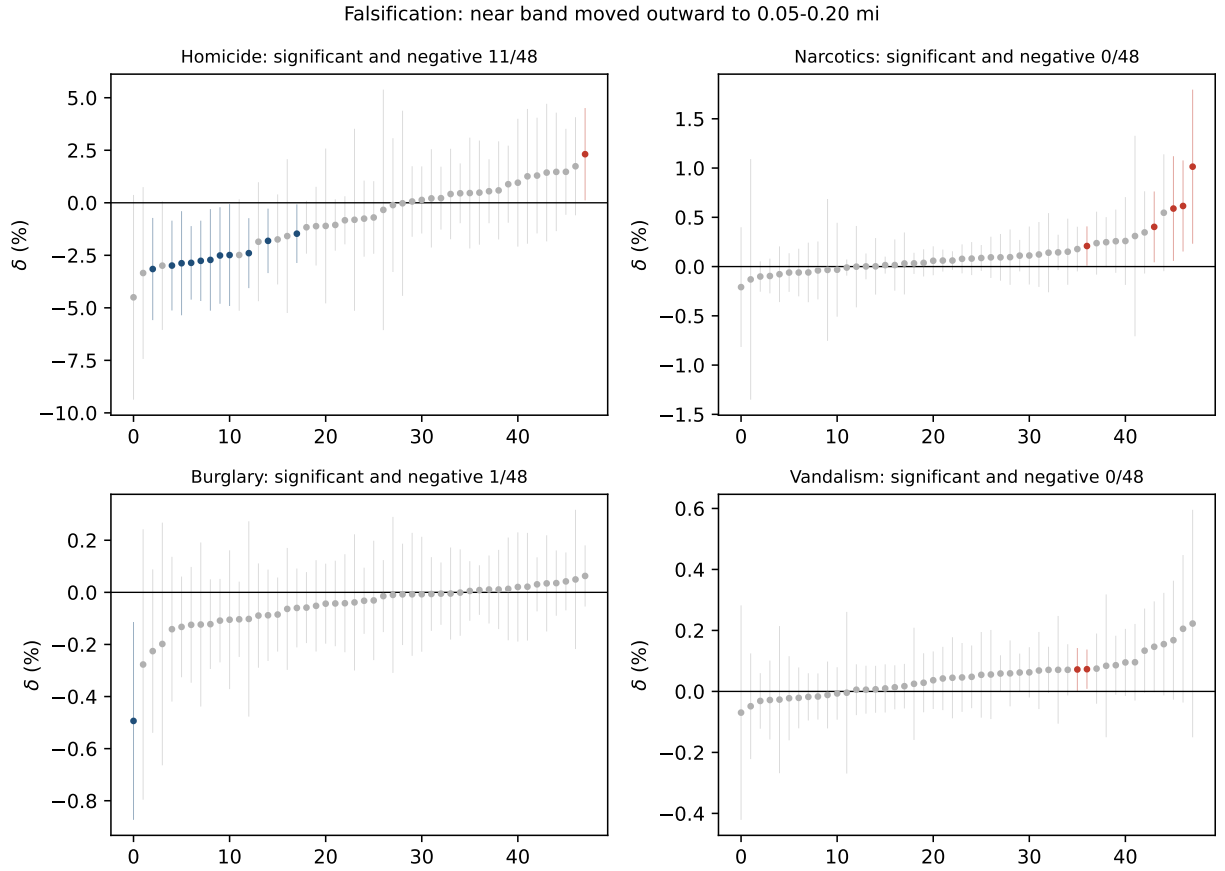


Figure OF9: Off-Doorstep Falsification: Treatment Band Moved to 0.05–0.20 Miles

*Notes.* Each panel sorts the near–far discount  $\delta$  (%) for the band 0.05–0.20 miles against 0.20–0.30 miles across 48 specifications (grid described in Section 5.6 of the main paper). Navy marks estimates significant and negative at five percent; red, significant and positive. Significant and negative: homicide 11/48 (all in the 0.10–0.15 mile band), burglary 1/48, narcotics and vandalism 0/48. Post-sale placebos significant and negative: homicide 0/48, narcotics 3/48, burglary and vandalism 5/48. Pooled 2007–2025, clustered by tract.



# I Functional Form, All Offenses

Table OA33: Functional Form, All Estimable Offenses

| Offense             | Raw count          | Presence ( $\geq 1$ ) | IHS               | Window |
|---------------------|--------------------|-----------------------|-------------------|--------|
| Homicide            | -3.02**<br>(1.38)  | -2.97**<br>(1.47)     | -3.61**<br>(1.63) | 10     |
| Burglary            | -0.24**<br>(0.12)  | 0.54<br>(0.98)        | -0.03<br>(0.28)   | 8      |
| Narcotics           | -0.33***<br>(0.11) | -0.78*<br>(0.43)      | -0.60*<br>(0.31)  | 12     |
| Vandalism           | -0.16**<br>(0.08)  | -0.35<br>(1.12)       | -0.12<br>(0.31)   | 7      |
| Theft               | 0.01<br>(0.02)     | 12.01*<br>(6.92)      | -0.15<br>(0.64)   | 12     |
| Motor-vehicle theft | -0.02<br>(0.09)    | -1.23<br>(0.84)       | -0.35<br>(0.32)   | 12     |
| Robbery             | -0.03<br>(0.13)    | -0.35<br>(0.74)       | -0.25<br>(0.28)   | 12     |
| Assault (non-dom.)  | -0.04<br>(0.04)    | 0.28<br>(1.77)        | -0.33<br>(0.40)   | 12     |
| Assault (dom.)      | -0.19*<br>(0.10)   | -0.83<br>(1.31)       | -0.59**<br>(0.28) | 12     |
| Disorderly conduct  | -0.65**<br>(0.29)  | -1.00**<br>(0.48)     | -0.81*<br>(0.42)  | 12     |
| Trespass            | -0.14<br>(0.10)    | -0.66<br>(0.59)       | -0.52*<br>(0.28)  | 12     |
| Prostitution        | 0.08<br>(0.12)     | 0.76<br>(0.98)        | 0.36<br>(0.73)    | 12     |
| Fraud               | -0.11<br>(0.08)    | -0.16<br>(1.15)       | -0.49<br>(0.33)   | 12     |
| Sex offenses        | -0.47**<br>(0.23)  | -0.66<br>(0.47)       | -0.74**<br>(0.33) | 12     |
| Forgery             | 0.21<br>(0.17)     | 0.39<br>(0.44)        | 0.26<br>(0.32)    | 12     |
| Weapons             | 0.10<br>(0.28)     | 0.35<br>(0.50)        | 0.19<br>(0.50)    | 12     |
| Arson               | 0.07<br>(1.03)     | -0.43<br>(1.08)       | -0.12<br>(1.19)   | 12     |
| Stolen property     | -0.43<br>(1.05)    | -0.72<br>(1.41)       | -0.73<br>(1.50)   | 12     |
| Liquor              | 0.36<br>(0.35)     | 0.95<br>(0.71)        | 0.76<br>(0.61)    | 12     |
| Gambling            | -1.94<br>(1.75)    | -3.39<br>(2.84)       | -3.13<br>(2.47)   | 12     |
| Bad checks          | 0.66**<br>(0.33)   | 1.06**<br>(0.46)      | 1.07**<br>(0.44)  | 12     |
| Family offenses     | 1.00*<br>(0.51)    | 1.69**<br>(0.71)      | 1.69**<br>(0.69)  | 12     |

*Notes.* Near-far close-price effect (%) of each estimable offense's doorstep exposure entering as a raw count, a presence indicator ( $\geq 1$ ), and inverse hyperbolic sine, each at the offense's representative window, pooled 2007–2025, parcel and tract $\times$ month fixed effects, clustered by tract. The raw count is the form the dose-response implies (Appendix Table OA6). Priced offenses are discussed in the functional-form robustness check (Section 5.6). Standard errors in parentheses.

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

## J Nonparametric Distance–Time Surfaces

The near ring and exposure window in the main text are read from a nonparametric estimate of the price effect as a joint function of distance  $D$  and time since incident  $L$ . Figure OF10 plots the estimated surface  $f(D, L)$ , a tensor-product spline in distance and lag, on the MLS close-date sample, 2007–2025, for the three property offenses with enough incidents to identify a two-dimensional smooth: burglary, narcotics, and vandalism. Homicide is too sparse to support a stable two-dimensional surface. In every case the effect is concentrated at short distances and recent lags and flattens to zero well before the far ring, which is what justifies the doorstep near ring and the 0.20–0.30 mile control annulus.

Each surface is a tensor-product spline  $\text{te}(D, L)$  estimated following Korolev (2026): a signal regression of the price, demeaned by parcel and tract  $\times$  month, on the crime count, with the smooth entering through a by term and grid-centered for identification, and the penalty chosen from the data by fast REML. This is a varying-coefficient model: the by term makes the crime count the treatment, and the tensor smooth lets its coefficient vary over distance and lag. The moderators are the geometry of exposure, how far the incident occurred and how recently, rather than subpopulation attributes such as buyer or neighborhood characteristics, so the surface shows how the effect of a given incident varies with the distance and recency of exposure. Following his procedure I add no wiggleness inflation, so the smoothness is what fast REML selects. Because exposure enters as counts on a discrete grid of six distance rings and twelve monthly lags rather than as continuous distances, the tensor bases are correspondingly small, six and four, below the  $k = c(10, 10)$  he uses for continuous smooths. The surface is not additively separable: an additive form  $f(D) + f(L)$  is rejected against the full interaction by AIC, so distance and time since incident do not enter as a sum. Multiplicative (rank-one) separability is a distinct property, and it holds only descriptively and only relative to the far-ring reference. Referenced to the untreated far annulus, the same reference the near–far design assumes, the near-band surface is approximately rank-one for burglary, meaning its spatial decay has the same shape at every lag with only the amplitude fading; for narcotics it is not, the decay shape itself changing with lag. This rank-one share is descriptive, not a formal test. I therefore estimate the full tensor-product surface rather than imposing either restriction.

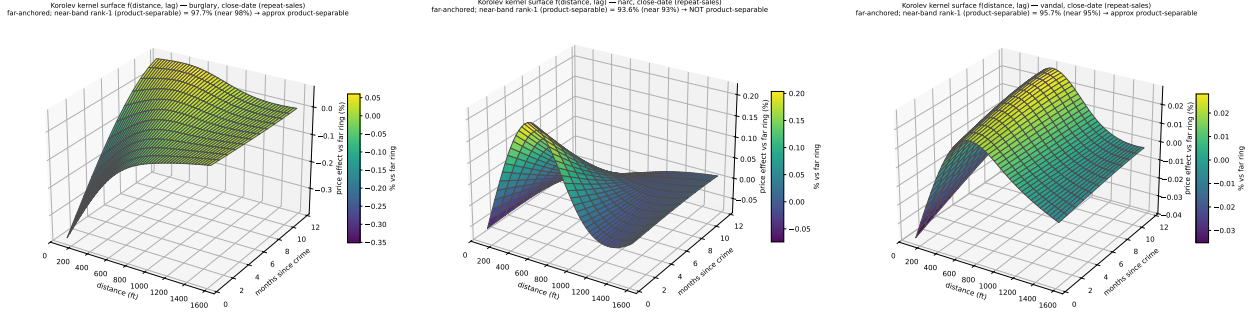


Figure OF10: Estimated price-effect surfaces  $f(D, L)$  over distance and time since incident, MLS close-date sample, 2007–2025: burglary (left), narcotics (center), vandalism (right). Each is a tensor-product spline fit over the full distance domain and referenced to the far ring: its value at the 0.20–0.30 mile annulus is set to zero, consistent with the design’s assumption that the annulus is untreated. The effect declines steeply in distance and lag and is a precise zero before the far ring. (This far-ring reference is what makes the rank-one comparison in the text well-defined; the main-text distance profile is instead centered over its plotted range, a different reference for a different question.)

The distance profile is fit over 0–0.30 miles, the full domain spanning the far comparison ring, and centered on its average over that range, so it identifies how the effect varies with distance rather than its level, which the fixed effects absorb. The doorstep estimate is essentially invariant across the basis sizes tried, moving by only 0.011 between  $c(4, 4)$  and  $c(6, 12)$ ; the reported profile uses the  $c(6, 12)$  fit, with one distance basis per 0.05-mile ring across the domain. The profile averages over the one-to-twelve-month exposure window whereas the near–far estimates are reported at each offense’s salient window, so the profile’s homicide level is smaller than the headline discount rather than in conflict with it; the smooth identifies the level precisely only for offenses with dense exposure, and for rarer offenses the near–far difference carries the precision.

## K Cross-Crime Confounding

A natural concern is that a discount attributed to one crime is in fact capitalizing a *different*, spatially correlated crime, that the homicide gradient, say, merely tracks nearby burglary or narcotics activity. Two facts limit this concern for homicide, burglary, and narcotics: the near-ring counts of distinct crimes are mostly weakly correlated, and, entered jointly at a common twelve-month window, each of these offenses retains its own coefficient. Vandalism is the exception.

**Near-ring counts are close to orthogonal.** Figure OF11 reports the pairwise Pearson correlation of the near-ring (0–0.05 mile) incident counts across all 923,730 MLS listings (all geocoded closed sales before the constant-parcel and repeat-sales filters), for all twenty-two non-sparse offense groups. The priced crimes are nearly independent of one another: homicide correlates at most 0.14 with any other offense group, and the strongest pair among the four priced crimes, burglary and vandalism, reaches only 0.39. Because omitted-variable bias scales with the correlation between the included and the omitted regressor, a near-orthogonal design leaves little room for one crime’s gradient to load on another’s: even a large omitted effect, multiplied by a correlation near 0.1,

perturbs the estimated coefficient negligibly.

**A joint horse-race of all twenty-two offenses.** The mechanical argument can be checked directly. Racing only the priced crimes against one another would be selection one step later, so I enter *all twenty-two* offenses' near-far terms in a single regression (common twelve-month window, parcel and tract-by-month fixed effects, clustered by tract,  $N = 242,815$ ; Table OA34). For homicide, burglary, narcotics, and disorderly conduct, the discount conditional on the other twenty-one is nearly identical to its separate estimate: narcotics  $-0.29\%$  ( $p = 0.01$ ) against  $-0.33\%$  separately, burglary  $-0.17\%$  against  $-0.18\%$ , homicide  $-2.4\%$  against  $-2.5\%$ , and disorderly conduct  $-0.58\%$ . The offenses that do not price remain null when entered jointly: larceny/theft, robbery, and motor-vehicle theft stay within hundredths of a percent of zero. Standard errors are essentially unchanged, since the near-ring counts are weakly correlated. Because the horse-race is run at a common twelve-month window, it does not test vandalism's six-month discount: at twelve months vandalism's separate estimate is already small and insignificant ( $-0.05$ , s.e.  $0.06$ ), and its joint estimate is  $-0.02$ .

Table OA34: Cross-Crime Horse Race: Each Offense Conditional on the Other Twenty-One

| Offense             | Separate     |        | Joint (all 22) |        |
|---------------------|--------------|--------|----------------|--------|
|                     | Discount (%) | (SE)   | Discount (%)   | (SE)   |
| Homicide            | −2.49*       | (1.36) | −2.44*         | (1.36) |
| Burglary            | −0.18*       | (0.09) | −0.17*         | (0.09) |
| Narcotics           | −0.33***     | (0.11) | −0.29**        | (0.11) |
| Vandalism           | −0.05        | (0.06) | −0.02          | (0.06) |
| Disorderly conduct  | −0.65**      | (0.29) | −0.58**        | (0.29) |
| Sex offenses        | −0.47**      | (0.23) | −0.40*         | (0.23) |
| Assault (domestic)  | −0.19*       | (0.10) | −0.16          | (0.10) |
| Assault (non-dom.)  | −0.04        | (0.04) | −0.01          | (0.04) |
| Robbery             | −0.03        | (0.13) | −0.01          | (0.13) |
| Larceny/theft       | 0.01         | (0.02) | 0.02           | (0.02) |
| Motor vehicle theft | −0.02        | (0.09) | −0.00          | (0.08) |
| Trespass            | −0.14        | (0.10) | −0.07          | (0.09) |
| Fraud               | −0.11        | (0.08) | −0.14*         | (0.08) |
| Weapons             | 0.10         | (0.28) | 0.20           | (0.28) |
| Arson               | 0.07         | (1.03) | 0.15           | (1.03) |
| Prostitution        | 0.08         | (0.12) | 0.11           | (0.11) |
| Forgery             | 0.21         | (0.17) | 0.23           | (0.17) |
| Bad checks          | 0.66         | (0.33) | 0.72           | (0.34) |
| Liquor              | 0.36         | (0.35) | 0.56           | (0.37) |
| Family              | 1.00         | (0.51) | 1.12           | (0.51) |
| Gambling            | −1.94        | (1.75) | −1.10          | (1.85) |
| Stolen property     | −0.43        | (1.05) | −0.48          | (1.03) |

*Notes.* Each offense's near-far discount  $\delta = (\beta_{\text{near}} - \beta_{\text{far}}) \times 100$  (percent sale-price change per doorstep incident), at a common twelve-month exposure window, pooled 2007–2025, with parcel and tract  $\times$  year-month fixed effects, clustered by tract ( $N = 242,815$ , 679 clusters). The *Separate* columns estimate one offense at a time; the *Joint* columns enter all twenty-two offenses' near and far terms in a single regression, so each discount is conditional on the other twenty-one. Reported as a robustness exhibit; standard errors are essentially unchanged in the joint specification. Stars from the joint  $p$ -value, shown only for discounts.

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

**Where co-movement occurs.** The two most correlated pairs in Figure OF11, larceny/theft with fraud (0.65) and larceny/theft with non-domestic assault (0.52), involve only offenses that do not price. Vandalism, however, co-moves with the high-volume offenses: domestic assault (0.49), non-domestic assault (0.45), and other offenses (0.41), and it correlates with burglary at 0.39. Narcotics correlates at 0.37 with domestic assault and with gambling. Homicide correlates at most 0.14 with any other offense.

**The tighter ring is the conservative choice.** Widening the near ring to 0–0.10 mile raises the cross-crime correlations, burglary and vandalism rise from 0.39 to 0.57, because a larger disk

pools more of the shared spatial background. The 0–0.05 mile ring used throughout is therefore the specification under which distinct crimes are *most* separable, not the one that most helps the argument. Choosing it makes cross-crime confounding harder to sustain, not easier.

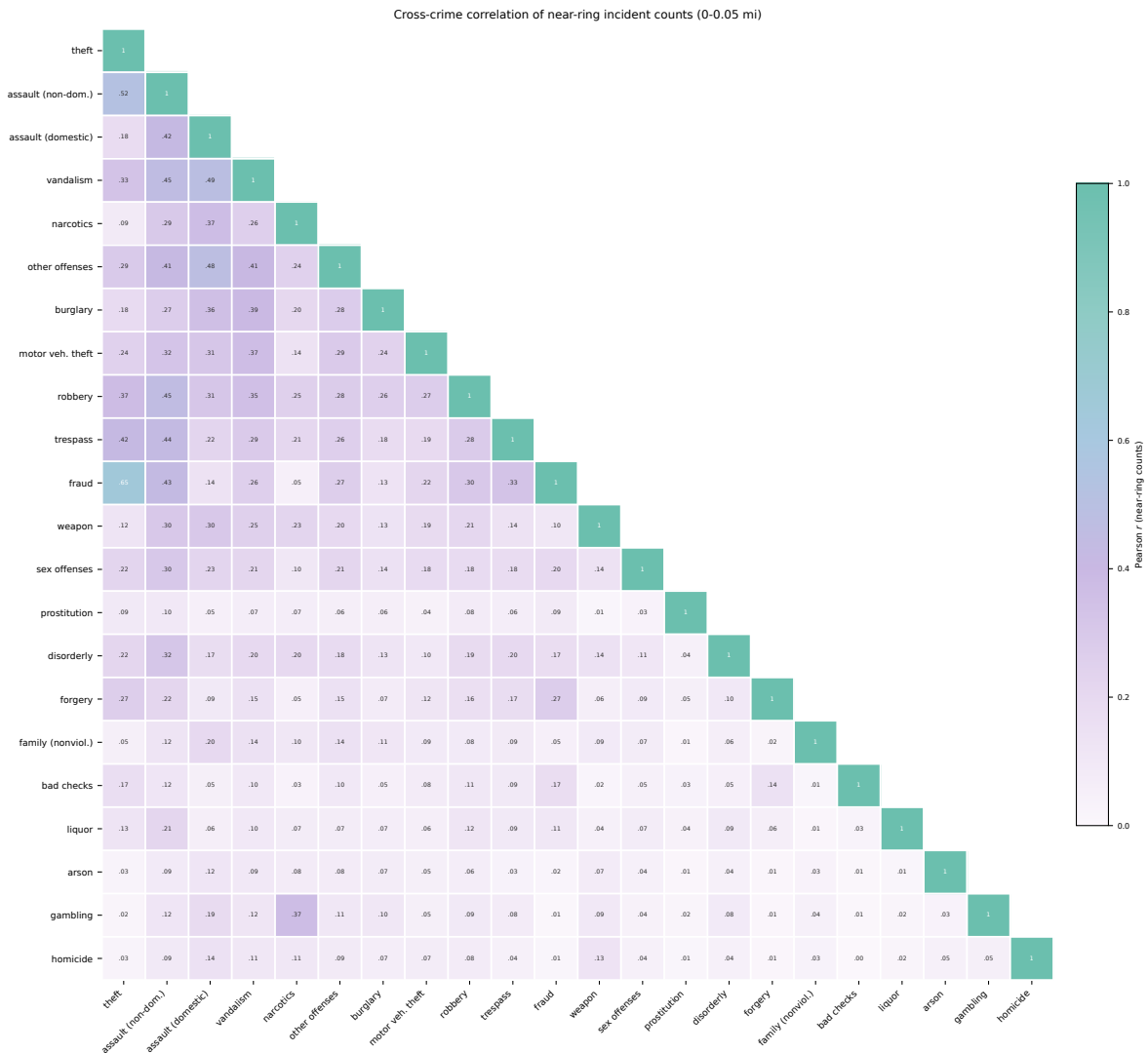


Figure OF11: Cross-crime correlation of near-ring incident counts. Pairwise Pearson correlation of the 0–0.05 mile incident counts of the twenty-two non-sparse offense groups, across 923,730 MLS listings. Homicide, burglary, and narcotics are near-orthogonal to the other offenses; vandalism is the exception, correlating moderately with the high-volume nuisance offenses (domestic and non-domestic assault and the “other” group) with which it co-occurs. The only strongly correlated pairs (larceny/theft–fraud, larceny/theft–assault) are among the crimes that do not price. The wider 0–0.10 mile ring raises these correlations, confirming that the 0–0.05 mile ring is the more separable specification.

## L Does Repeat-Sales Selection Drive the Discount? A Heckman Correction

The capitalization estimates are identified from repeat sales: a parcel enters the identifying sample only if it transacts at least twice, so that its fixed effect can be differenced out. This design invites an obvious objection. If nearby crime changes *which* parcels resell, the sample of twice-sold homes is endogenously selected, and the estimated discount could reflect who-sells rather than what-crime-does-to-price. I confront this directly with a Gatzlaff–Haurin two-equation selection correction (Gatzlaff and Haurin, 1997; Heckman, 1979), adapted to the MLS close-date panel.

**A selection model with an excludable instrument.** Selection operates through two transaction margins, each estimated as an annual-risk-set probit. A *first-close* probit models whether a parcel records its first MLS sale in a given year (covariates: living area, lot size, age, unemployment); a *second-close* probit models whether a previously-sold parcel resells (adding the excluded instrument). From each probit I form the inverse-Mills ratio  $m = \phi(Z\pi)/\Phi(Z\pi)$  and attach it to the corresponding transaction. Because a home’s first and second sales draw their  $m$  from *different* probits, the selection term varies within a parcel and is not swept away by the parcel fixed effect, a subtle but essential feature that makes the correction identified in a repeat-sales design.

The excluded instrument is mortgage-rate *lock-in*: the gap between the prevailing thirty-year rate at a potential resale and the rate the owner locked in at purchase,  $r_{30}(t) - r_{30}(t_{\text{prior}})$ . Lock-in strongly governs *whether* an owner sells (surrendering a below-market mortgage is costly, so a large positive gap suppresses mobility), but it bears no plausible relation to the *cross-sectional near-versus-far crime gradient* realized at the moment of sale. It shifts selection without touching the price equation.

**Results.** Table OA35 delivers the sharp result. The inverse-Mills coefficient is large, positive, and significant at the 0.1% level for every priced crime: the repeat-sales sample *is* selected, and the correction has real bite. This matters because it means the test is not vacuous: the data do exhibit the selection the objection worries about, and the Heckman term is capable of moving the estimate. Adding  $m$  changes the point estimates little: homicide shifts from  $-4.35\%$  to  $-4.18\%$ , narcotics from  $-0.47\%$  to  $-0.52\%$ , vandalism from  $-0.40\%$  to  $-0.34\%$ , and burglary from  $-0.36\%$  to  $-0.33\%$ ; burglary is not significant either before or after the correction in this smaller sample.

The logic is what makes this convincing. A null correction is uninformative when the instrument is weak or selection is absent; one learns nothing from a test with no power. Here selection is strongly present ( $m$  highly significant) and is explicitly purged, and the point estimates change little. The composition of the twice-sold sample is not random,  $m$  says as much, but that selection is *orthogonal to the near-versus-far crime gradient*: it shifts the price level, not the crime slope. For homicide, narcotics, and vandalism, the discounts are therefore not explained by which homes resell; for burglary the check is uninformative because the estimate is imprecise in this sample.

Table OA35: Capitalization Under a Repeat-Sales Selection Correction

| Crime     | Uncorrected<br>$\delta$ (%) | Selection-corrected<br>$\delta$ (%) | Selection term<br>$m$ (Mills) |
|-----------|-----------------------------|-------------------------------------|-------------------------------|
| Homicide  | −4.35**                     | −4.18**                             | 0.72***                       |
| Burglary  | −0.36                       | −0.33                               | 0.72***                       |
| Narcotics | −0.47***                    | −0.52***                            | 0.72***                       |
| Vandalism | −0.40**                     | −0.34**                             | 0.72***                       |

Near = 0–0.05 mi, far = 0.20–0.30 mi; outcome  $\ln(\text{close price})$ ; PIN and tract $\times$ close-month fixed effects; standard errors clustered by tract; pooled sample, each offense at a fixed per-offense window (homicide 10, burglary 8, narcotics 12, vandalism 7 months), pre-set canonical windows that differ from the peak-significant windows in Table 2, with a persistence control for near-ring crime beyond the window cumulated to twelve months.  $m$  is the inverse-Mills ratio from a two-probit Gatzlaff–Haurin selection model (first-close and second-close probits; excluded instrument = mortgage-rate lock-in, plus unemployment). “Uncorrected” and “selection-corrected” run on the identical  $m$ -matched repeat-close sample ( $N = 130,564$ ) for comparability, smaller and differently selected than the full repeat-sales sample of the main results, so the uncorrected homicide estimate here (−4.35) exceeds the headline repeat-sales discount (−3.0 percent); the object of the test is the movement from uncorrected to corrected, not the level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

## M Is Nearby Crime Exogenous? A Bunching Test

The capitalization estimates identify off within-parcel variation: a home enters the identifying sample only through repeated sales, and the near–far discount is the price movement associated with a change in nearby crime, net of the parcel and neighborhood–month fixed effects. Appendix L shows the estimate is robust to selection into the twice-sold sample. Here I test a distinct assumption, that the near-ring crime count is itself *exogenous* conditional on the design, i.e. uncorrelated with unobserved determinants of price. I do so with a bunching test that requires no exclusion restriction, exploiting a feature of the data: the near-ring count has a *mass point* at zero, because most homes record no incident in the exposure window.

**The dummy test.** I implement the test of Caetano et al. (2021). I augment the headline near–far specification (Equation (5)) with a boundary indicator for zero doorstep exposure, under the same fixed effects:

$$\ln(\text{close})_{it} = \beta_{\text{near}}\text{Near}_{it} + \beta_{\text{far}}\text{Far}_{it} + (\text{persistence}) + \gamma \mathbf{1}(\text{Near}_{it} = 0) + \mu_i + \lambda_{c(i)t} + \varepsilon_{it}, \quad (1)$$

where  $\mu_i$  is the parcel fixed effect and  $\lambda_{c(i)t}$  the tract $\times$ month fixed effect of Equation (5), and I test whether  $\gamma = 0$ . The logic, due to Caetano (2015) and applied to crime counts with a mass at zero by Caetano and Maheshri (2018) to select among dynamic-spillover models of Dallas police data, is that confounders tend to be *discontinuous* at a mass point: if the zero-crime homes lie on the smooth dose–response traced by the crime-exposed homes, the indicator is zero; a nonzero coefficient signals that the mass-point observations are selected. The dummy version dominates the

original nonparametric test: it is more powerful, applies to discrete or mixed treatments, and, in a two-way fixed-effects design, jointly detects failures of exogeneity, functional form, *and* the “strong” parallel-trends condition (parallel trends plus uncorrelated treatment effects, the assumption whose failure drives the negative-weights problem).

**Which crimes can be tested.** The test needs positive support beyond the mass point, homes at one, two, three or more nearby incidents, to trace the dose–response. Homicide fails this: its near ring is  $\approx 99\%$  zero, with essentially no observations above one (Figure OF12); arson and stolen property are similarly sparse (not shown), so the test is not identified for them; they are, by construction, a presence contrast rather than a dose. Narcotics, vandalism, and burglary have workable mass points (48–75% zero, with 12–27% of homes at two or more) and are tested.

**Selection into zero exposure.** Table OA36 reports the boundary coefficient as fixed effects escalate. Uncontrolled, it is large and highly significant: for narcotics, zero-crime homes sell for about 49% (a 39.7 log-point boundary coefficient) more than the dose–response predicts, and they differ so much on unobservables that the raw near–far slope even flips positive. Tract $\times$ close-month fixed effects shrink it by an order of magnitude, and under the headline PIN + tract $\times$ close-month design it is a precise null for both narcotics and vandalism (Figure OF13). Crucially, the discount  $\delta$  is essentially unchanged whether or not the dummy is included (Panel B). The within-property, within-neighborhood-month design thus absorbs the cross-sectional discontinuity that the raw data plainly exhibit; the diagnostics provide supporting evidence that the estimated discount is not driven by *which* homes have no nearby crime.

**Burglary.** The boundary and skewness tests both indicate that zero-exposure parcels differ for burglary. Two estimates identify the slope from exposed parcels only: adding the boundary dummy gives  $-0.552$  (s.e. 0.168), and the binned dose-response (Appendix Table OA6) gives 0.48 (s.e. 0.25) for a single incident and  $-1.85$  (s.e. 0.76) for four or more, a cumulative rather than per-incident effect. The rejection does not establish the direction of any remaining bias, so the linear headline,  $-0.244$  (s.e. 0.117), is a maintained approximation rather than a bound. The jump is not spillover from the adjacent ring (controlling for 0.05–0.10 burglary leaves it at  $-0.99$ , s.e. 0.32), and at a 0–0.10 near ring the test does not reject ( $-0.06$ , s.e. 0.27) while the discount remains significant ( $-0.14$ , s.e. 0.07).

**Higher moments.** Finally, I apply the same dummy test to the second and third moments of the price residual, conditioned on the covariates (Figure OF14 and Table OA32). A discontinuity in the *variance* at the mass point would signal *heterogeneous* selection, the correlated random-effects failure that also invalidates two-way fixed effects. Adding  $\mathbf{1}(\text{near} = 0)$  to the conditioned second moment yields a precise null for all three crimes (burglary  $p = 0.64$ , narcotics 0.72, vandalism 0.84; winsorized, at the headline parcel and tract $\times$ close-month design, 0–0.05 mile, six-month flow): the diagnostics do not detect a variance discontinuity at zero exposure. Winsorization removes

Table OA36: Boundary-Dummy Test of Identification (Caetano–Caetano–Fe–Nielsen 2021)

|                                                                                          | Narcotics            | Vandalism           | Burglary             |
|------------------------------------------------------------------------------------------|----------------------|---------------------|----------------------|
| <i>Panel A. Boundary dummy <math>\mathbf{1}(\text{near} = 0)</math>, coefficient (%)</i> |                      |                     |                      |
| Raw (no fixed effects)                                                                   | 39.671***<br>(2.811) | 4.328***<br>(1.249) | −1.353<br>(1.176)    |
| Tract $\times$ close-month                                                               | 4.371***<br>(0.785)  | 2.566***<br>(0.573) | 0.329<br>(0.538)     |
| PIN + tract $\times$ close-month (headline)                                              | 0.014<br>(0.344)     | −0.259<br>(0.249)   | −0.978***<br>(0.317) |
| + quadratic dose-response                                                                | 0.082<br>(0.408)     | −0.438<br>(0.285)   | −1.057**<br>(0.442)  |
| <i>Panel B. Near–far discount <math>\delta</math> (%), headline design</i>               |                      |                     |                      |
| $\delta$ , without dummy                                                                 | −0.331***<br>(0.107) | −0.158**<br>(0.076) | −0.244**<br>(0.117)  |
| $\delta$ , with dummy                                                                    | −0.329***<br>(0.114) | −0.212**<br>(0.092) | −0.552***<br>(0.168) |
| Observations                                                                             |                      | 242,815             |                      |

*Notes.* The dummy test of Caetano et al. (2021) adds an indicator of the bunching point,  $\mathbf{1}(\text{near} = 0)$ , to  $\ln(\text{close}) = \beta_1 \text{near} + \beta_2 \text{far} + \text{persistence} + \varepsilon$ ; under valid identification its coefficient is zero. In two-way fixed-effects designs the test jointly detects failures of exogeneity, linearity, and strong parallel trends. Near = 0–0.05 mi, far = 0.20–0.30 mi, each crime at a fixed per-offense exposure window (narcotics twelve, vandalism seven, burglary eight months), pre-set canonical windows that differ from the peak-significant windows in Table 2, with a 7–12 month persistence control; single-parcel sample; standard errors clustered by tract in parentheses. The large raw discontinuity (narcotics 39.7 log points, about 49%: zero-crime homes are dramatically different uncontrolled) is progressively absorbed by fixed effects and is a precise null under the headline PIN + tract  $\times$  close-month design; the discount  $\delta$  is stable with and without the dummy for narcotics and vandalism. The boundary and skewness tests both indicate that zero-exposure parcels differ for burglary. Two estimates identify the slope from exposed parcels only: adding the boundary dummy gives −0.552 (s.e. 0.168), and the binned dose-response (Appendix Table OA6) gives 0.48 (s.e. 0.25) for a single incident and −1.85 (s.e. 0.76) for four or more, a cumulative rather than per-incident effect. The rejection does not establish the direction of any remaining bias, so the linear headline, −0.244 (s.e. 0.117), is a maintained approximation rather than a bound. The jump is not spillover from the adjacent ring (controlling for 0.05–0.10 burglary leaves it at −0.99, s.e. 0.32), and at a 0–0.10 near ring the test does not reject (−0.06, s.e. 0.27) while the discount remains significant (−0.14, s.e. 0.07). Homicide, arson, and stolen property are not testable (near-ring  $\approx$  99% zero: no dose-response support). \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

the skewness rejection for vandalism, but burglary’s remains significant at the five-percent level ( $p = 0.02$ ). For burglary, the skewness rejection is consistent with the selection at zero exposure that the boundary dummy detects. Identifying the slope from exposed parcels only gives −0.552 (s.e. 0.168); because the test does not establish the direction of bias, the linear −0.244 is a maintained approximation.

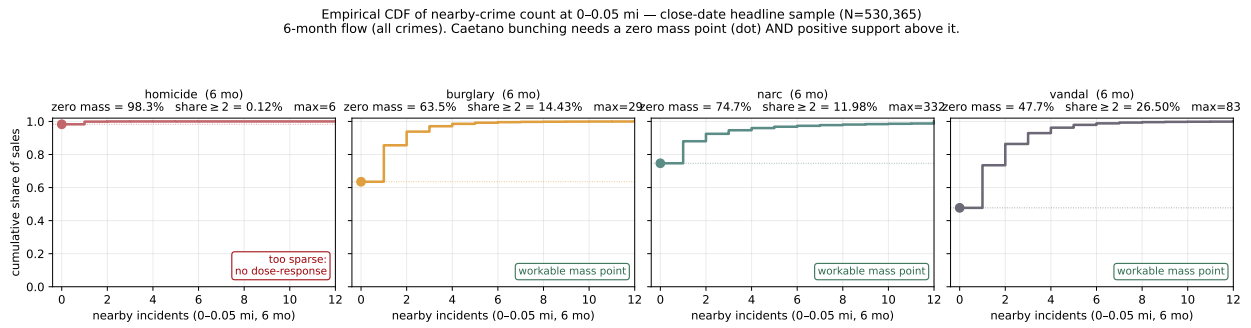


Figure OF12: Which crimes can be tested: empirical CDF of the near-ring count. The bunching test needs a zero mass point *and* positive support above it. Homicide is 99% zero with no dose-response; narcotics, vandalism, and burglary have workable mass points.

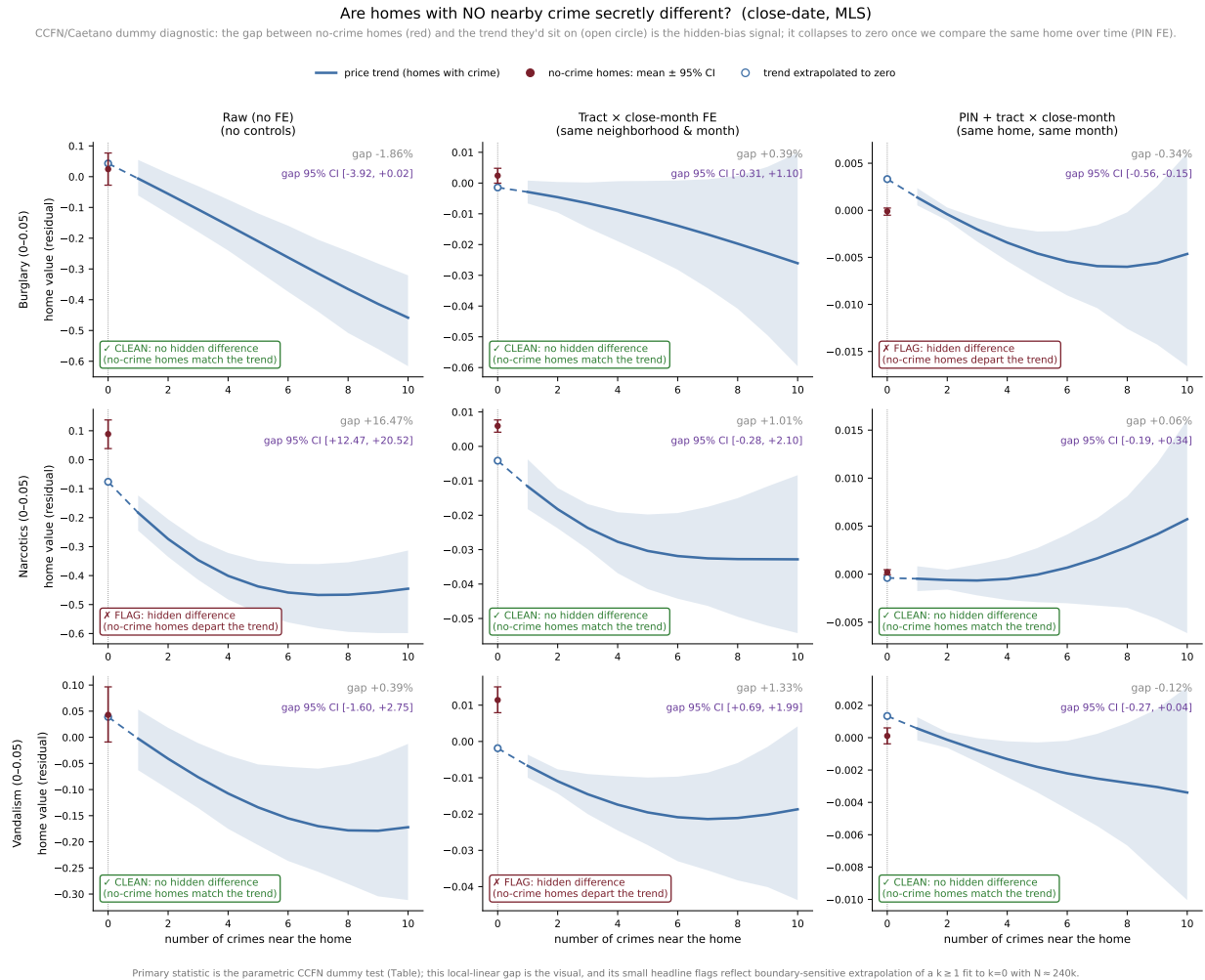
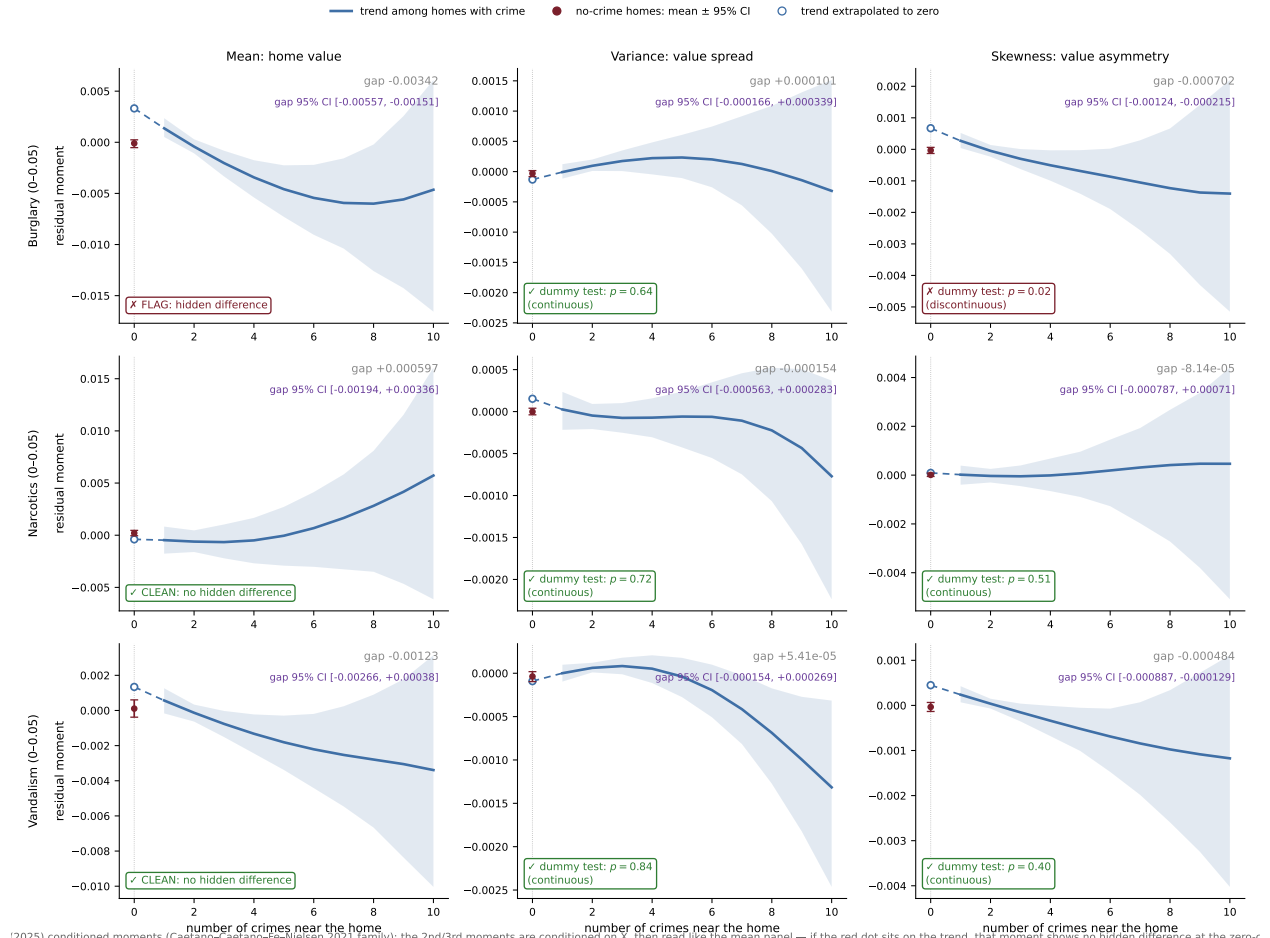


Figure OF13: The dummy diagnostic. Blue: dose-response among crime-exposed homes; red: no-crime homes; open circle: the trend extrapolated to zero. For narcotics and vandalism the large gap in the raw column collapses to zero under the headline PIN + tract $\times$ close-month design; for burglary a gap remains, so its discount is identified partly from the exposed-versus-unexposed contrast; the rejection does not establish the direction of any bias (Table OA36).

Higher-moment test at the headline design (close-date): variance continuous at zero for all three; burglary differs in mean and skewness



(2025) conditioned moments (Caetano-Caetano-Pe-Nielsen 2021 family): the 2nd/3rd moments are conditioned on  $X$ , then read like the mean panel — if the red dot sits on the trend, that moment shows no hidden difference at the zero-c  
The variance panel is the key heterogeneous-selection signal and is continuous at zero for all three; burglary's skewness also rejects in the parametric dummy test ( $p = 0.02$ ).

Figure OF14: Higher-moment test. The mean, variance, and skewness of the conditioned price residual at the zero-crime mass point. The parametric dummy test finds the *variance*, the signature of heterogeneous selection, continuous at zero for all three crimes (burglary  $p = 0.64$ , narcotics 0.72, vandalism 0.84); under winsorizing the skewness rejection is removed for vandalism but not for burglary. The local-linear gaps shown are boundary-sensitive and are superseded by the dummy verdict.

## N Market Tightness, Contract Timing, and Distressed Sales

A nearby incident can lower the recorded sale price by revising what buyers believe the home is worth, or by giving a buyer under contract leverage to reopen the agreed price. Both move the close price in the same direction, and a transaction reveals only the equilibrium price, so the main regression cannot separate them. I look for the signature of bargaining on the margin where a leverage account differs most sharply from revaluation, the buyer's market power.

I assign each sale the months of supply (active inventory over trailing three-month sales) in its community area in the month it was listed. The measure is computed leave-one-out, so the focal sale is excluded, and is therefore predetermined relative to the close and not circular with the outcome. It is a standard measure of market tightness, defined within year so that a soft market is soft relative to its own year rather than to the 2009–2012 downturn. A buyer in a soft market holds outside options and leverage that a market of competing offers removes, so a bargaining channel predicts a larger discount where supply is high. Estimating the discount across terciles of supply, with the parcel and tract-by-month fixed effects and stale-crime controls of the main specification, I find no such gradient. For burglary, narcotics, and vandalism the market-tightness comparisons do not detect a difference in discounts between soft and tight markets, with the test of equality returning  $p$  above 0.4 at every identified window. Homicide is imprecise: at the twelve-month window its estimate is  $-4.8$  percent in soft markets against  $-0.3$  in tight, but the difference is not significant ( $p = 0.13$ ) and the ordering reverses at shorter windows, so the homicide comparison is inconclusive. A tercile split defined over the whole sample period does produce an apparent gradient, but it places most soft-market sales in the 2009–2012 crisis; the gradient is an era effect that the within-year definition removes.

Two more direct tests are available but lack power in these data. The first compares crime in the twelve months before the contract date, which is embedded in the agreed price, with crime between contract and close, which arrives after the price is set and can move it only through renegotiation. Pre-contract exposure carries the usual discounts, about 0.2 percent per burglary and 0.3 percent per narcotics incident. Post-contract exposure is insignificant for every offense, but its standard errors are too wide to be informative: the burglary and narcotics estimates carry standard errors of 0.24 and 0.58, and the homicide estimate 3.3, so each interval spans effects as large as the belief-channel discount. The window between contract and close averages about six weeks, so few incidents fall in it, and holding that window to a fixed length requires restricting to slow closings, which selects troubled deals and no longer reproduces the belief channel itself. The second test compares distressed sales with arm's-length sales, but distressed transactions are about thirteen percent of the sold sample (REO, short-sale, or foreclosure, the same definition as Table OA30) and their estimates are too dispersed to sign.

The market-tightness comparisons do not detect a difference in discounts and provide limited support for this particular bargaining mechanism. The short-horizon renegotiation and distressed-sale margins remain beyond the reach of these data.

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