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Unlocking amenities: Estimating public good complementarity☆

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ABSTRACT

Public goods may exhibit complementarities that are essential for determining their individual value. Our results indicate that improving safety near parks can turn them from public bads to goods. Ignoring complementarities may lead to i) undervaluing the potential value of public goods; ii) overestimating heterogeneity in preferences; and iii) understating the value of public goods to minority households. Recent reductions in crime have "unlocked" almost $7 billion in property value in Chicago, New York and Philadelphia. Still, two-fifths of the potential value of park proximity, $10 billion, remains locked-in.

1. Introduction

Economic theory leans heavily on the idea that goods may be complements in consumption. While the joint demand for private goods has been studied extensively, little has been said about the joint demand for public goods. Studying the joint demand for public goods is difficult as individuals cannot purchase them directly, but only indirectly, such as through housing. To the best of our knowledge, no study has estimated the joint demand for public goods in a well-identified framework.[1] This raises issues for public investment decisions as their value may depend critically on complementary relationships.

In this paper, we study the complementarity between public safety and urban parks in major U.S. cities. Our hypothesis is intuitive: parks are less valuable when they are dangerous. As crime rises, the value of parks to nearby residents may disappear, and even become negative. This idea is not new. In her seminal work, Jacobs (1961) devotes a chapter to the "use of neighborhood parks," where she argues that parks are not inherently equal in value. Without formalizing the complementarity with crime, Jacobs writes:

> Unpopular parks are troubling not only because of the waste and missed opportunities they imply, but also because of their frequent negative effects … their dangers spill over into the areas

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[1] The closest analyses we know of consider the relationship between amenities and private consumption, Connolly (2008) and Graff Zivin and Neidell (2014), examine the relationship between weather and time use, and thus leisure as a good. Cuffe (2017) examines how rainfall influences museum attendance.

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surrounding, so that streets along such parks become known as danger places too and are avoided (Jacobs, 1961, p. 95).[2]

The empirical evidence presented in this study supports this hypothesis. Safety "unlocks" the value of parks. A corollary is that public safety is more valuable near parks. Thus, merely displacing crime away from parks may have social value. Indeed, reducing crime near parks or other public capital may be a boon to urban revival.[3] Complementarity also implies that it can be wasteful to equalize the level of public goods (parks) provision across communities without equalizing others (safety).

Paying attention to public good complementarity has important methodological implications for hedonic valuation (Banzhaf and Walsh, 2008), sorting behavior (Kuminoff et al., 2013), quality-of-life determination (Albouy and Lue, 2015), and how public goods are allocated across households that differ by race or income (Banzhaf et al., 2019). Our findings highlight two related points. First, complementarities can affect the validity of estimates from hedonic valuation and other revealed preference methods. Estimates from an unsafe area may not apply to a safe area and vice versa. Second, econometricians may attribute willingness-to-pay variation to differences in tastes, when they may instead be due to differences in endowments. Indeed, economics is known to have a long-standing ambivalence about explaining phenomena through variation in taste (Stigler and Becker, 1977). Our evidence suggests that ignoring complementarity could lead the analyst to infer that minority households, who tend to live in higher-crime areas, value safe parks less than actually they do.

Our empirical analysis uses detailed crime and housing data in Chicago, New York and Philadelphia from 2001 to 2016. In particular, we use 656,841 housing market transactions located within 3/8 miles of 1336 parks. We organize these transactions into "neighborhoods" surrounding each park and assign each a local crime risk measure based on nearby homicides.

We employ several strategies to estimate the value of park proximity and, more importantly, changes in that premium as a function of crime. As a foundation, we employ a neighborhood spatial differences (NSD) design, comparing areas near parks with areas farther away. This relies on applying 1336 fixed effects, one for each neighborhood.[4] The difference in housing prices near and far from parks identifies the average "park premium." A second difference identifies the complementarity using two sources of variation. In some specifications, the second difference captures changes in the park premium over time as crime rates fluctuate, controlling for local time trends. In others, the second difference relies upon spatial variation in the park premium between areas that are safe or dangerous. We evaluate the sensitivity of these estimates to controls, including neighborhood-by-year fixed effects and socio-economic characteristics interacted with both park proximity and local crime rates. We then examine instrumental variables (IV) estimates that use city-level crime reductions to predict property-level changes in crime risk. This helps remove potentially endogenous variation in crime changes from local neighborhood dynamics.

Estimates of the park premium for homes within roughly one block of parks remain stable at around 5% in safe areas. This premium falls to zero at approximately double the average crime rate in our sample, supporting the main hypothesis of complementarity. At higher levels of crime, there is some evidence of a "park discount," suggesting that parks may become a public bad. Tests developed by Oster (2017) provide indirect evidence that both the interaction and the main effect of parks in our main specifications are robust to omitted variables. Moreover, the results are quantitatively consistent across the range of specifications, including the IV.

Our analysis indicates that without a park–crime interaction, an analysis of heterogeneity in the park premium by neighborhood demographics might suggest that residents of minority neighborhoods value parks less than non-minorities. When we include the park–crime interaction term, we find no evidence of taste differences between the groups.

Complementarity affects the estimated benefits of parks and their distribution across neighborhoods. In total, our estimates indicate that park proximity alone contributes over $10.5 billion in total value to nearby homeowners across the three cities. However, on average, low-income neighborhoods receive negative benefits from park proximity. If they were to become safe, houses in low-income neighborhoods have the most to gain from park proximity, even if on average, their homes are less valuable. If parks were made safer, even from displacement, the total value of park proximity could roughly double. Much of the increase would accrue to low-income neighborhoods. Since the beginning of our sample period, the amenity value unlocked through reductions in crime accounts for roughly half of the current value.

Our estimates likely represent a lower bound on the total benefits of unlocked amenity value for two reasons. First, the total value of parks is probably much larger than the benefits of park proximity, as park users include those who live more than a couple of blocks away. Second, Banzhaf (2018) demonstrates that estimates such as ours provide a lower-bound of benefits in settings with preference-based sorting. We find little evidence that resident characteristics change with the park–crime complementarity, though we cannot rule out changes in unobserved characteristics.

This paper addresses parallel, but mostly disparate strands of research in public goods valuation. The first estimates the value of increases (Gamper-Rabindran and Timmins, 2013) and reductions (Currie et al., 2015; Davis, 2004; Muehlenbachs et al., 2015) in environmental amenities. Since the value of leisure-producing environmental amenities such as clean air (Chay and Greenstone, 2005; Currie et al., 2015; Ito and Zhang, 2019; Muehlenbachs et al., 2015), clean water (Keiser and Shapiro, 2018), and climate amenities (Albouy et al., 2016) depends on the overall quality of outdoor experience in any locale, there is reason to believe that their value may depend on levels of public safety. Many authors estimate the value of access to open space — see Brander and Koetse (2011) for a meta-analysis — although their reliance on cross-sectional variation raises concerns about bias from omitted variables.

A second strand estimates the value of public safety through housing prices. This literature dates back to early efforts by Thaler (1978). Recent studies address measurement error and omitted variables concerns to value the social cost of crime (Gibbons, 2004) as well as extensions to the value of policing (Chalfin and McCrary, 2018; Di Tella and Schargrodsky, 2004), targeted public safety and crime prevention programs (Donohue et al., 2013; Draca et al., 2011; McMillen et al., 2019), and the relocation of sex offenders (Linden and Rockoff, 2008). The present study focuses on the benefits of crime reductions that operate through a complement. However, it raises important questions for further research on the benefits of public safety. The tentative evidence suggests that crime exhibits

[2] Through various examples and anecdotes, Jacobs proposes a kind of mechanism whereby parks that become less active facilitate the perception of reduced safety as well as the production of crime (which presumably increase together). Interestingly, Jacobs even suggests that the kind of variation in amenity benefits that she observes might offer opportunities to the empiricist: "Philadelphia affords almost a controlled experiment on this point," and goes on to compare the successful Rittenhouse Square park with the dangerous Washington Square Park. Similar issues are recognized in the urban planning literature (Weiss et al., 2011).

[3] For work on urban revival, see Baum-Snow and Hartley (2017) and Couture and Handbury (2017).

[4] This empirical design is similar to research by Espey et al. (2001) and Anderson and West (2006), although we use multiple cities over long time periods. We show that effects are identified under relatively restrictive assumptions in a base model that is consistent with prior work.

diminishing marginal damages in neighborhoods near parks. Taken literally, this hints that it may be socially beneficial to concentrate crime if the total can be held constant.

In two articles on the topic, we find contradictory estimates of the relationship between parks and crime using purely cross-sectional data. Anderson and West (2006) find crime associated with higher values in Minneapolis, whereas Troy and Grove (2008) find crime associated with lower values in Baltimore.[5] Each of these studies relies on different samples and specifications, making them difficult to reconcile.[6] Besides framing the issue of public good complementarity more generally, our estimates examine data across multiple cities and use variation across time and space, allowing for a rich set of time-varying controls and the IV strategy.[7]

Section 2 presents a theory of complementary public goods in a hedonic setting. Section 3 describes our data. Section 4 examines functional form issues and presents graphical evidence. Section 5 reports supporting regression evidence for a range of specifications. Section 6 considers the roles of sorting and preference heterogeneity. Section 7 considers the distribution of gains from park complementarity over time and by neighborhood income. Section 8 concludes.

2. A simple model of public good complements

In principle, complementarity in public goods, e.g., warm weather and a community pool, is no less important than between private goods, swimming trunks and goggles. An important difference is that local public goods are bought indirectly through housing. This purchase is developed in the model below.

Preferences are represented by a Cobb-Douglas function: the utility of person i in location j is $U_{ij} = Q_{ij} y^{\alpha} x^{1-\alpha}$, where y is the quantity of the housing good consumed, with price v_j, x is a numeraire good, and $\alpha \in (0,1)$ is a fixed parameter. Q_{ij} gives the value of location j to person i, which is log-linear in interacted amenities:

$$\ln Q_{ij} = \left(\theta^P + \theta^{PH} H_j\right) P_j + \theta^H H_j + \ln \xi_j + \epsilon_{ij} \tag{1}$$

where P_j denotes the environmental amenity, H_j denotes the crime level, and ξ_j other commonly-valued amenities. The parameter ϵ_{ij} is an idiosyncratic taste shock for the neighborhood.

The parameters $\theta^P > 0$ and $-\theta^H > 0$ define the base elasticities of willingness-to-pay for the environmental amenity and safety (minus crime), respectively. The interaction parameter θ^{PH} describes the complementarity, which we predict to be negative. Alternatively, safety and parks are complements. These terms may be arranged as $(\theta^H + \theta^{PH} P_j) H_j + \theta^P P_j$ to illustrate how the cost of crime rises when the environmental amenity is higher.[8]

Denote our measure of crime, $\tilde{H}_j = H_j + a_j$, where a_j captures measurement error. This error adds to the unobserved amenity term: $\tilde{\xi}_j = \xi_j + (\theta^H + \theta^{PH} P_j) a_j$. Taking these shifts into account, the indirect utility function is given by:

$$\ln U_{ij} = -\alpha \ln v_j + \left(\theta_j^P + \theta^{PH} H_j\right) P_j + \theta^H \tilde{H}_j + \tilde{\xi}_j + \epsilon_{ij}.$$

Solving for log housing price, and letting $V_{ij} = \ln v_{ij}$, it is natural to separate out the park–crime interaction.

$$\begin{aligned} V_j &= \frac{\theta_j^P}{\alpha} P_j + \frac{\theta^H}{\alpha} \tilde{H}_j + \frac{\theta^{PH}}{\alpha} (P_j \times H_j) + \frac{\tilde{\xi}_j + \epsilon_{ij} - \ln U_{ij}}{\alpha} \\ &\equiv \beta^P P_j + \beta^H \tilde{H}_j + \beta^{PH} \left(P_j \times \tilde{H}_j\right) + \xi_j^* + u_{ij} \end{aligned} \tag{2}$$

where $\beta^k = \theta^k/\alpha, k \in \{P, H, PH\}, \xi_j^* = \tilde{\xi}_j/\alpha$, and $u_{ij} = (\epsilon_{ij} - \ln U_{ij})/\alpha$. This specification predicts that $\beta^P > 0$ and $\beta^H < 0$. If parks and safety are complementary, then $\beta^{PH} < 0$. This linear model predicts that above a certain level of crime, a park becomes a public bad. If

$$\tilde{H}_j \geq -\frac{\beta^P}{\beta^{PH}} = \frac{\theta^P}{\theta^{PH}}, \tag{3}$$

then households will pay to live away from the park.[9] As shown in Banzhaf (2015) for the case of individual amenities, hedonic estimates that exploit exogenous changes in the level of one or both public goods complements may shift an entire hedonic price function and identify a lower bound on the Hicksian equivalent surplus.

3. Data and descriptive statistics

3.1. Housing data

Our data are based on observations of housing transactions that vary in their proximity to urban parks and in crime incidents reported in the neighborhood. We study Chicago, New York, and Philadelphia, as they have a large number of parks as well as geocoded, incident-level crime reports. For Chicago, these reports cover the period 2001–2016 and for New York and Philadelphia, 2006–2016. These constitute the years of our sample.

The data on transaction prices and structural characteristics come from Zillow (2018). From these data, we create controls for dwelling characteristics: log distance to the CBD, age of the dwelling, square footage, number of bedrooms and bathrooms, and dwelling type (i.e. single and multifamily residences).[10]

We match each house with data on the socio-economic composition of residents living in the Census block and block group from the 2000 and 2010 Censuses, complemented by the 2011–15 and 2013–2017 American Community Survey (ACS). These include: population density; percentages of White, Black, and Latino households at the block level; vacant and rented housing units at the block level; and median age and median income at the block group level. We also match homes to the total number of housing units in their census block for later benefit calculations.

Table 1 reports basic descriptive statistics for these variables. Note that areas with predominantly White populations are strongly

[5] Anderson and West (2006) estimate this relationship with a sample of 24,000 housing transactions and the number of “serious crimes,” which includes thefts and assaults. Troy and Grove (2008) use 16,000 transactions. They use a measure of the incidence of robbery and rape. The paper states: “Murder was not chosen because the numbers of these crimes are small,” which is true for a single year. They dismiss the use of assaults asserting that these are often indoors and related to domestic violence.

[6] Bowes and Ihlanfeldt (2001) find that crime can affect property values near rail stations, another urban public good.

[7] This is not the first study to posit the importance of public safety for parks. Anderson and West (2006) and Troy and Grove (2008) are good examples of empirical research that examines crime and open space. Troy and Grove (2008) discuss some elements of the complementarity such as a threshold of public safety that is necessary for positive valuation of urban parks.

[8] Strictly speaking, the marginal value of one amenity increases with respect to the other even without the interaction in a Cobb-Douglas formulation. But this is not due to any kind of complementarity. Focusing on the elasticity of the value makes the complementary relationship more plain. Complementary amenities are also implied by the canonical Tinbergen model, described in Bartik and Smith (1987) and Ekeland et al. (2004), even though they have only rarely been estimated.

[9] The error term does include differences in the preference shock relative to utility. Thus, the framework may be used to motivate a logit estimator based on how many people choose to live in an area based on its proximity to a park and local safety. Such an approach would require a nuanced understanding of local housing supply. Since we find little evidence of sorting or increases in population density, we focus on the hedonic analysis instead.

[10] Dwelling characteristics come from the Assessor's office and correspond to the most recent property assessment.

Table 1
Descriptive statistics: housing transactions and characteristics.

	Chicago	New York	Philadelphia	Sample
Panel A: Park characteristics and Homicide Risk				
Number of parks	571	645	120	1336
Avg park size, square miles	0.02	0.05	0.10	0.04
Avg neighborhood size, square miles	0.64	0.74	0.92	0.71
Avg property-level homicide risk	1.65	1.47	1.39	1.55
	(2.31)	(2.07)	(2.15)	(2.19)
Avg property-level transformed homicide risk	2.06	1.96	1.76	2.0
$\widetilde{H}_{it} = 2\sqrt{H_{it}}\left(\sqrt{(H_{it})}\right)^{-1}$	(1.66)	(1.54)	(1.68)	(1.60)
Panel B: Property transactions				
Within 1/16 mile	68,346	74,192	6000	148,538
From 1/16 to 2/16 miles	73,739	80,760	5461	159,960
From 2/16 to 6/16 miles	170,463	159,680	18,200	348,343
Avg price within 1/16 mile, \$K	298	985	334	643
	(205)	(1031)	(213)	(818)
Avg price 1/16 to 2/16 miles, \$K	302	830	272	568
	(198)	(849)	(170)	(673)
Avg price 2/16 to 6/16 miles, \$K	278	770	248	502
	(181)	(813)	(165)	(618)
Log distance to CBD, miles	2.2	1.3	1.5	2.0
Age of structure	75	76	70	75
Square footage	1457	1078	1442	1403
Number of bedrooms	3.0	1.6	2.8	2.8
Number of bathrooms	1.4	1.4	1.4	1.4
Single Family Residence, percent	44	20	51	33
Multifamily, percent	56	80	49	67
Panel C: Socio-economic characteristics				
Residents per square mile, thousands	40.8	92.1	34.7	65.1
White, percent	52	60	62	56
African-American, percent	19	8	18	14
Latino, percent	19	13	6	15
Median resident age	34.9	39.5	36.0	37.2
Median income, \$K	68.8	91.6	55.3	79.1
Vacant, fraction	0.09	0.11	0.09	0.10
Renter, fraction	0.35	0.42	0.34	0.38
Unemployed, fraction	0.08	0.07	0.08	0.08

Notes: Sample includes transactions within 3/8 of a mile of a park in Chicago (2001–2016), New York (2006–2016), and Philadelphia (2006–2016) from Zillow (2018). White refers to non-Latino White and African-American refers to non-Latino African-American. Neighborhood refers to the 3/8 miles radius around a park. Standard deviations are in parentheses.

represented in all three cities and throughout the study period. While the fraction of owner-occupied, multifamily units may appear high, this is not unusual for the cities in our sample.

3.2. Urban parks and neighborhood definition

We organize the housing transactions into neighborhoods, with each centered on a single park. Parks are defined in our source (openstreetmap.org) as: "open, green area for recreation, usually municipal, and are differentiated from other public/private open spaces such as: golf courses, stadiums, nature reserves (which may not have public access), and marinas."[11] We use all parks larger than 0.6 acres (26,136 ft^2), providing 1336 geo-coded urban parks across all three cities. Our minimum area requirements drop about 13% of all reported park-like units reported in openstreetmap.org. Some of these units may be valued by local residents, although many are not named or managed by local park districts.

Each neighborhood contains the housing transactions within 3/8 miles of its park, aside from transactions that are nearer to other parks. We then subdivide the transactions into bands around the park that are 1/16 of a mile (≈100 m) in width: $P_{ik} \equiv I\left[1/16 \times k \le d_{ij}^{j} < 1/16 \times (k+1)\right]$, where d_{ij}^{j} is the distance between each house i to the closest neighborhood park j. Each of these bands correspond to the width of a typical block. We neatly summarize these bands using the vector $P_i \equiv [P_{i1}, P_{i2}, \ldots, P_{i6}]$.[12] A house within a block may have a view. Within two blocks, the park is still within earshot of loud sounds such as gunfire.

Fig. 1 illustrates our neighborhood definition using a map of transactions within 3/8 miles of the parks in Chicago. The transactions are colored according to their distance interval except that the third through fifth intervals are colored uniformly. This reflects the evidence shown below: houses in P_{i3} through P_{i5} do not appear to benefit from a park premium, while those in P_{i1} do. Houses in P_{i2} seem to benefit weakly, and thus may be taken as secondary evidence or excluded. Each point refers to a single transaction. The insert shows a close-up view of the neighborhood around Marquette Park. While not an affluent part of Chicago, the neighborhood still contains a large number of transactions. In total, our final data-set contains 656,841 transactions. Chicago and New York have almost

[11] See https://wiki.openstreetmap.org/wiki/Key:leisure. We subdivide some of the largest parks, such as Central Park in New York, Lincoln Park in Chicago and Fairmount Park in Philadelphia, in order to capture the effects of crime in particular neighborhoods that they span.

[12] In Chicago, most blocks are 1/16 of a mile in length, although many East–West blocks are 1/8 of a mile. In New York, many blocks are approximately 1/2 of a mile north-to-south, and often up to 1/7 of a mile, east-to-west. Central Philadelphia blocks are about 1/13 of a mile.

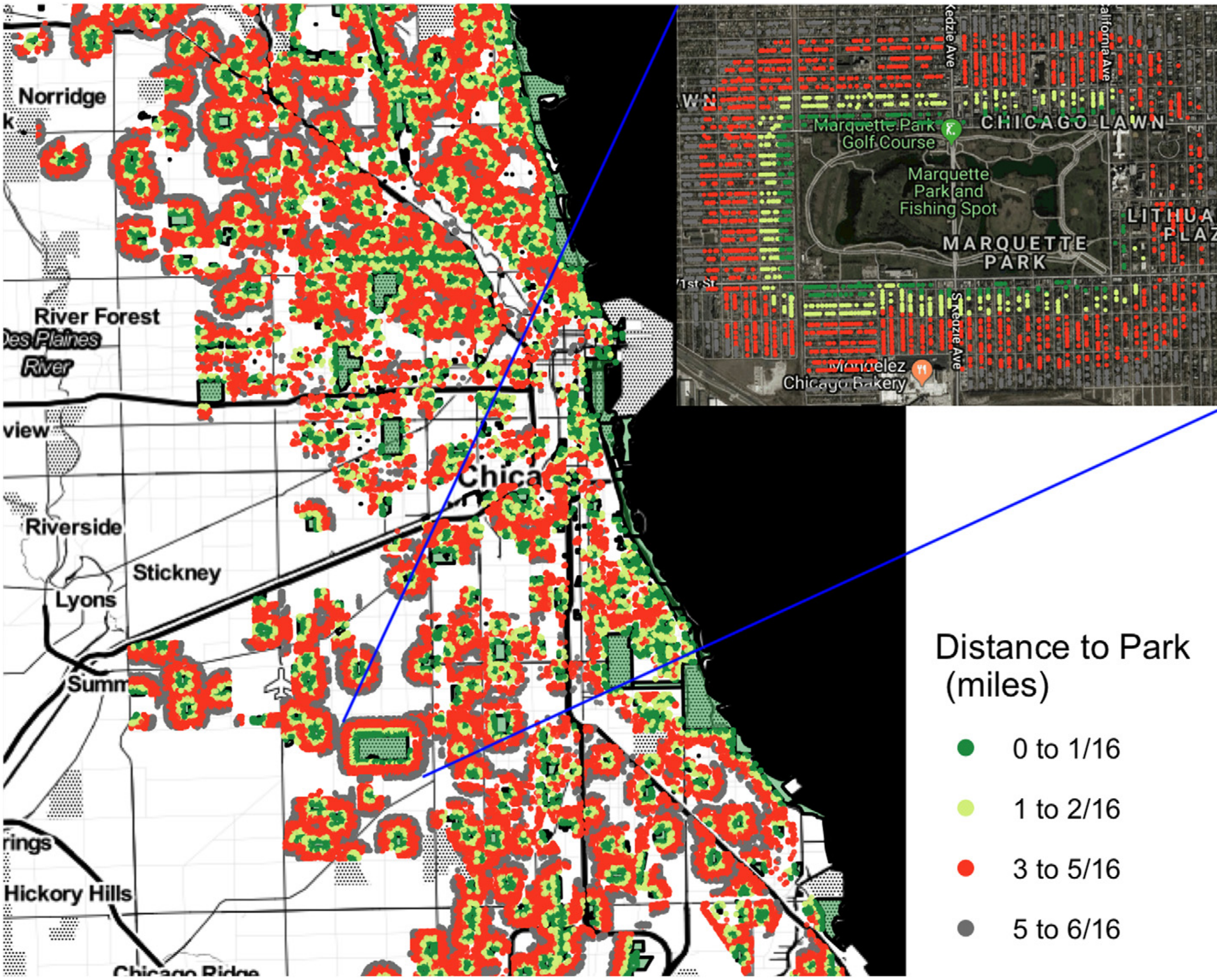


Fig. 1. Housing transactions around parks: neighborhood distance intervals. Notes: The following figure shows transactions within 3/8 miles of the nearest park in Chicago. The zoom in figure represents the 'neighborhood' around Marquette Park. It contains all of the transactions (4623) within three-eights of a mile that are not closer to another a park. Colors correspond to different distance intervals or 'bands' around the park. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

equal numbers, while Philadelphia has far fewer.[13] As seen in Table 1, the raw transaction prices of homes within 2/16 of a mile of a park are higher than those slightly farther away.

3.3. Mapping crime risk at the transaction level

Our crime measure is based on crime incident reports. These data come from city police departments, provided by their Open Data Portal.[14] We use these geo-located reports to calculate a measure of crime risk at all locations for every city and year in the study period.

For clarity and comparability over space and time, we focus on homicide risk. Prior research suggests that property and other types of crime are subject to greater reporting biases. Furthermore, property crime in particular can occur more frequently in neighborhoods with greater amenities and wealth.[15] Hence, we use "crime" and "homicides" interchangeably throughout the paper.

Fig. 2 illustrates the estimated homicide risk for Chicago. Darker-shaded areas indicate higher homicide risk. To calculate homicide

[13] Fig. 1 and Appendix Figs. A1 and A2 illustrate the parks, bin definitions, and housing transactions in each of the three cities.

[14] For the City of Chicago, the data are extracted from the Chicago Police Department's CLEAR (Citizen Law Enforcement Analysis and Reporting) system and available through the Chicago Data Portal at https://goo.gl/D8Vm82, New York City data from the New York City Police Department (NYPD) and available through NYC Open Data portal at https://goo.gl/zGp8Z2. Philadelphia crime incidents come from the Philadelphia Police Department and are available through Open Data Philly at https://goo.gl/gYR96r.

[15] We analyze the robustness of our estimates to measures that include all crimes and discuss them with our main results. Prior research illustrates substantial heterogeneity in the perception and valuation of different types of crime and ambiguous effects of property crimes on housing prices. For example, Thaler (1978) finds that property crime reduces housing prices, but Gibbons (2004) finds no effect of burglaries. Ihlanfeldt and Mayock (2010) discuss the drawbacks of using total crimes as a crime risk measure. Using total crime implicitly assigns the same weight to all crimes, putting too much weight on low-value crimes. As an alternative to homicide, we use willingness-to-pay estimates from Chalfin and McCrary (2018) to construct a unitary measure of homicide-equivalents. Homicide risk appears to provide a better signal of what areas are truly dangerous. These estimates are much less precise, though they suggest qualitatively similar findings.

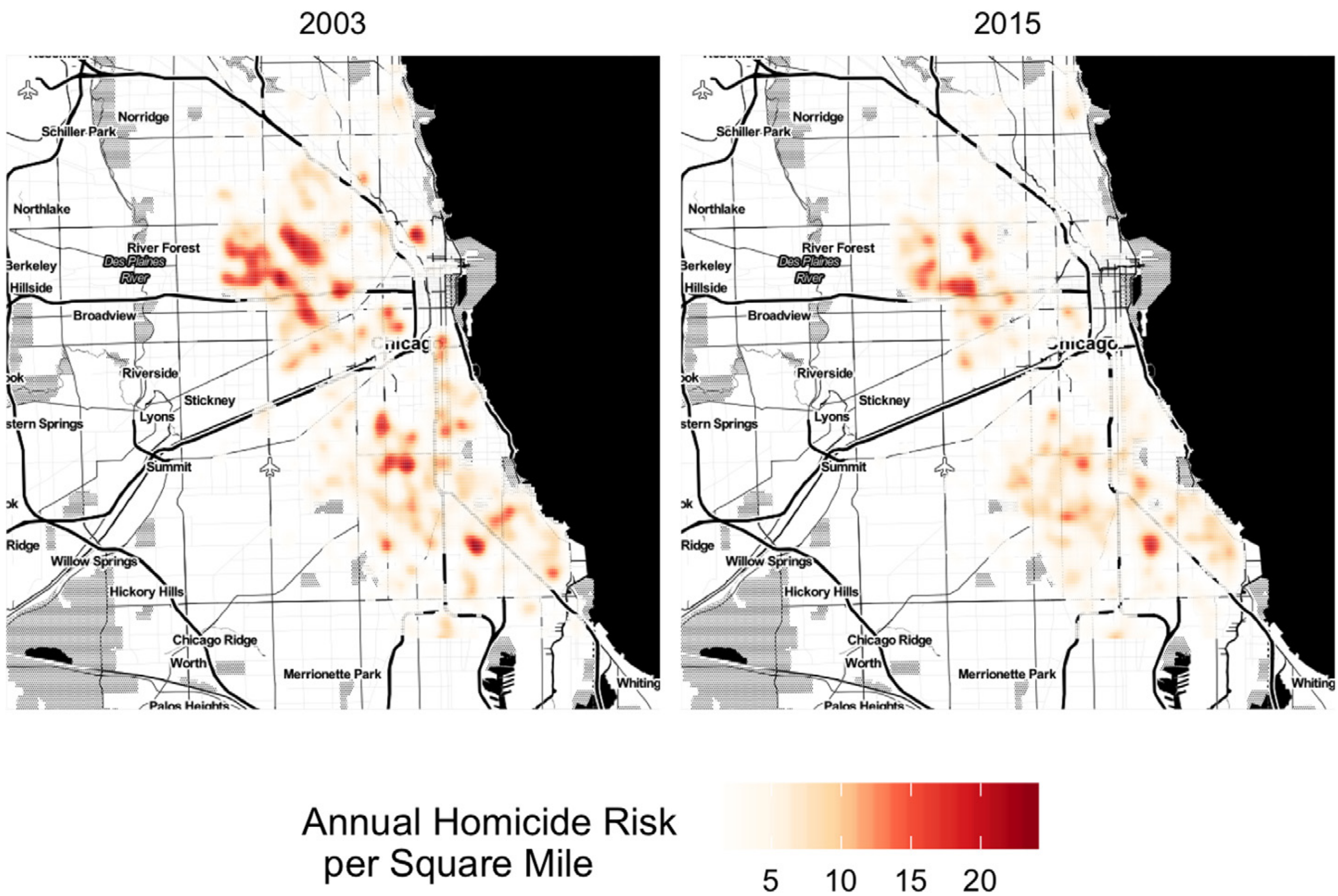


Fig. 2. Homicide risk heat map for Chicago (3-year moving average). Notes: Shades represent levels of homicide risk, which we measure as the expected number of homicides per square mile. Estimates are based geolocated crime data for years 2001–2003 and 2013–2015. We use a bivariate Gaussian kernel with a bandwidth of 1/4 of a mile on a 1/8 mile city grid to estimate the likelihood per square mile of a homicide in each grid. We then calculate the expected number of homicides using the total number of city homicides.

risk, we estimate the likelihood of a homicide at a given property based on the crime incident reports. This likelihood is estimated using a bivariate Gaussian kernel with a bandwidth of 2/8 of a mile on a 1/8 mile city grid, and normalized to give the probability of a homicide per square mile. We use a three-year rolling window to help smooth out short-term fluctuations. The narrow bandwidth and grid allow for fine distinctions in crime rates even within neighborhoods. Taking into account the total number of homicides in the city, H_t^c, our measure of homicide risk at property i in neighborhood j in year t is then given by:

$$\text{(Linear) Homicide Risk}: h_{ijt} = E(h_{ijt}) = p_{ijt} H_t^c \quad (4)$$

where p_{ijt} is the estimated probability of homicide at property i in year t. This yields the expected number of homicides per square mile in year t at property i, $E(h_{ijt}) = h_{ijt}$.[16] Table 1 reports that average property-level homicide risk is similar across the cities, ranging from 1.39 in Philadelphia to 1.65 in Chicago.

Fig. 3 plots trends in homicide rates per square mile for each of the cities during the study period. All of the three cities have experienced substantial (>30%) declines in homicide rates up to 2015, although Chicago's rate shot up in 2016. The declines within cities were not uniform across space. Examining both panels in Fig. 2, it is possible to see that while most areas in the city became safer, some became more dangerous.

Each transaction is matched to the measure of homicide risk that corresponds to its precise location and time. Fig. 4 plots the ratio of homicide risk within 1/8 of a mile of the park to that in the rest of the neighborhood, from 1/8 to 3/8 of a mile. Most neighborhoods have a density of homicide risk of less than 2 per year per square mile. In these neighborhoods, the ratio is close to one. In more dangerous neighborhoods, crime risk is slightly higher near parks when looking at the average across the study period.

Crime risk changes over the period were distributed rather uniformly across space. Homicide risk did become slightly more concentrated in the three cities, though not by much. In the base year, we find that 80% of the homicide risk was located in 12.6% of the land area. In the last year of the sample, that fraction fell to 12.5% of the land area.

4. Visual evidence and functional form

Below, we provide visual evidence of the relationship between parks, crime, and transaction prices. This evidence guides our choices of functional form of measures defined in Section 2. Visually, we

[16] In Table 5, we consider measures that use the average homicide rate for the entire neighborhood around the park as well as measures that exclude incidents in or within 1/8 of a mile of the park. Note that the incident data does not measure crimes within parks, but at a street address on the perimeter of the park, usually at the closest location to the incident. We also try different weighting schemes to construct our homicide risk measure. Results are robust to alternative weights.

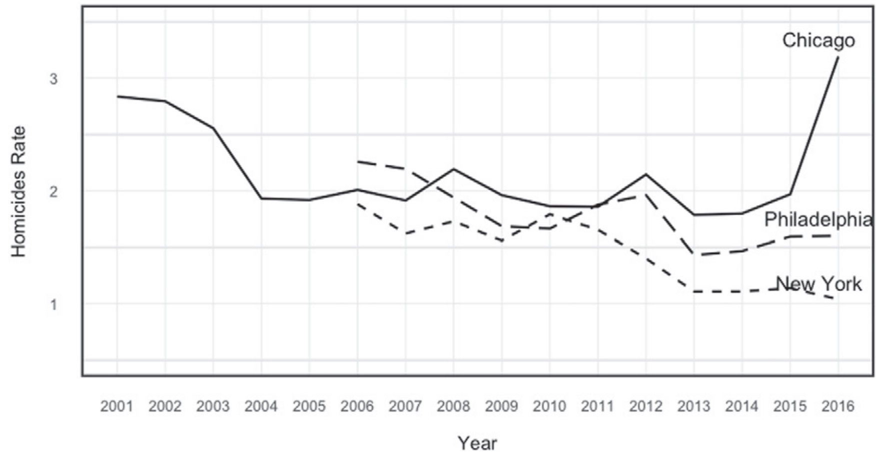


Fig. 3. Annual homicide rate per square mile by city. Notes: Homicide rate is defined as the number of homicides per square mile for Chicago (2001–2016), New York (2006–2016), and Philadelphia (2006–2016).

examine prices by proximity to park, homicide risk, and their interaction by plotting estimates from the following regression equation:

$$V_{ijt} = \mathbf{P_i}\beta^P + \mathbf{H_{ijt}}\beta^H + \sum_k P_{ik}\mathbf{H_{ijt}}\beta_k^{PH} + D_i\beta^D + \gamma_j + \zeta_t^c + u_{ijt}. \quad (5)$$

For park proximity, we use P_i, the park-distance bins mentioned above, which distinguish properties that are typically a block apart. We use the estimates from this model to test for evidence of a decaying park premium in our sample and also to determine whether properties farther away from parks can be sensibly combined in a single comparison bin.

For homicide risk, we examine data grouped by bins of homicides, H_{ijt}. However, interactions between distance bins and homicide risk bins are low in power and impractical for regression tables. This motivates us to consider a continuous measure of homicide risk. We use a non-linear (square root) transformation of risk for two reasons. First, the data generating process for crime can be thought of as a continuous Poisson process. Least squares regressions are best suited to Gaussian processes. Brown et al. (2013) show that if H follows a Poisson distribution with mean λ, then 2 times the square root, $\tilde{H} = 2\sqrt{H}$, is approximated by a Gaussian distribution. We normalize this by the mean so the coefficients can be interpreted as elasticities:

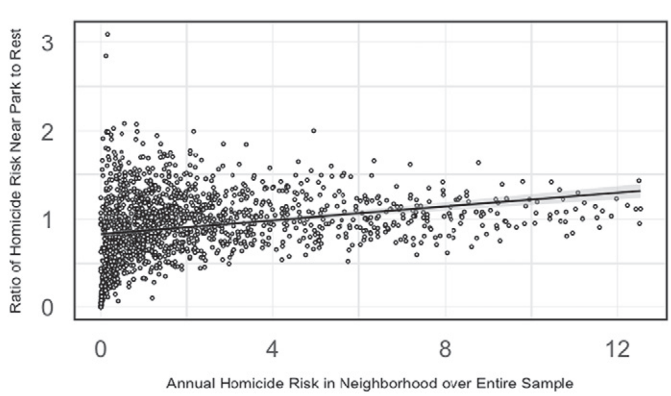


Fig. 4. Homicide risk by neighborhood: relative risk near parks vs overall risk. Notes: The vertical axis denotes the ratio between the average homicide risk per square mile within 1/8 mile of a park and the rest of the neighborhood (2–3/8 of a mile). The horizontal axis measures the average yearly homicide risk in the neighborhood (within 3/8 of a mile). Ratios are computed using averages across the study period.

$\tilde{H} = 2\sqrt{H}/\overline{\sqrt{H}}$.[17] Second, households may experience diminishing returns to safety. Once residents no longer consider their neighborhood safe, they may limit their time outdoors such that additional reductions in safety do relatively little damage.[18]

The model includes a fixed effect for each neighborhood assigned to a park, γ_j. These fixed effects form the basis of the NSD method that helps to control for unobserved factors that vary between neighborhoods. It is based on the idea that properties a few blocks away from each other are likely to be close substitutes. Including these fixed effects requires a large number of observations, which fortunately our data provide. The controls also include 3 sets of time indicators, ζ_t^c, for each city-by-year combination. This takes into account the fact that cities may exhibit different housing price cycles. Finally, the controls include dwelling characteristics, D_i, which do not vary over time due to our source.

Fig. 5 plots variation in housing prices at different levels of homicide risk, comparing homes located within 1/16 of a mile of a park, P_1, to properties 2–5 /16 miles away, P_A. The horizontal axis uses a square root scale. There are two important features to note. First, at the lowest risk levels, transaction prices for homes near parks are roughly 5 percentage points higher than homes farther away from the same park. Second, this park premium disappears at higher levels of homicide risk, supporting the hypothesis that the value of parks falls with crime. The fitted lines come relatively close to the markers, illustrating that a square root transformation fits the data better than other power transformations. As a result, we focus on the transformed measure, $\tilde{H}_{ijt}$, throughout the remainder of the analysis.[19]

Fig. 6 plots distance to park along the horizontal axis using the full 6 bins in $\mathbf{P}_i$. The model interacts these with the transformed measure

17 Taking the differentials on both sides, the 2 cancels out $\frac{dp}{p} = \beta \times \frac{1}{\sqrt{H}}\frac{1}{\overline{\sqrt{H}}}dH$. Thus, β resembles an elasticity, evaluated at $H = \sqrt{H} \times \overline{\sqrt{H}}$.

18 Albouy et al. (2016) make a similar argument for extreme temperature based on Zivin and Neidell (2014).

19 We examine the fit of several linear and non-linear homicide measures in Appendix Table A1. Estimates of the complementarity are not much different across these different forms. Point estimates in all specifications decline rapidly and are not statistically significant after the first interval when the block-level, time-varying controls are added. Appendix Fig. A3 compares the fit of the linear vs square root measures, suggesting that the linear measure may underestimate prices at low levels of homicide risk. Appendix Table A2 compares estimates using the linear measure of homicide risk. The linear estimates imply a smaller park premium and a somewhat smaller percentage point reduction in prices per additional homicide within 1/16th mile of a park. In the Online Appendix C, we provide results using the simpler linear measure.

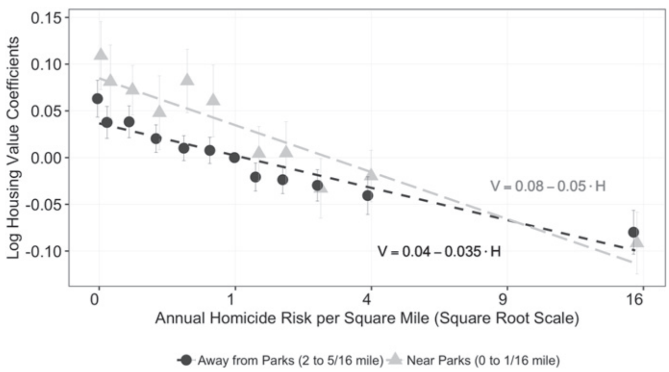


Fig. 5. Transaction prices by homicide risk: near parks versus comparison properties. Notes: Figure plots estimates of transaction prices at different homicide rates using: $V_{ijt} = \mathbf{P_i}\beta^P + \mathbf{H}_{ijt}\beta^H + \sum_k P_{ik}\mathbf{H}_{ijt}\beta_k^{PH} + D_i\beta^D + \gamma_j + \zeta_t^c + u_{ijt}$, where V_{ijt}, is the sales price of house i in neighborhood j, city c, year t. P_{ik} are indicators for distance to park, where the excluded category is 2 –5/16 of a mile. $\mathbf{H}_{ijt}$ are indicators for different levels of homicide risk around property i. D_i are dwelling characteristics which are described in Panel B of Table 1. γ_j are neighborhood fixed effects, and ζ_t^c are city-year interaction indicators. Round markers plot estimates for the comparison group of properties from parks between 2 and 5/16 miles, the excluded category, hence β^H. The triangle markers plot estimates for near parks, $\beta^H + \beta_1^P + \beta_1^{PH}$. Bars denote 90% confidence intervals.

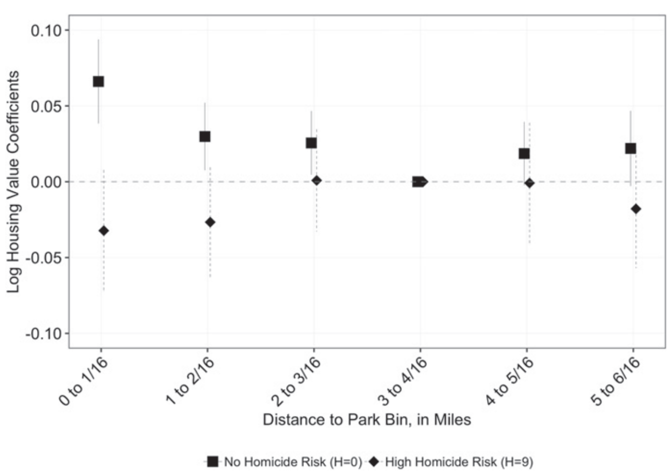


Fig. 6. Transaction prices by distance to park. Notes: Figure plots estimates of prices by distance from parks, using: $V_{ijt} = \mathbf{P_i}\beta^P + \tilde{H}_{ijt}\beta^H + \sum_k P_{ik}\tilde{H}_{ijt}\beta_k^{PH} + \gamma_j + \zeta_t^c + u_{ijt}$, where V_{ijt}, is the sales price of house i in neighborhood j, city c, year t. P_{ik} are indicators for distance to park at intervals of 1/16 of a mile (roughly one block) and $\tilde{H}_{ijt}$ is the square root of homicide risk per square mile around property i. γ_j are neighborhood fixed effects, and ζ_t^c are city-year interaction indicators. Square markers for no homicide risk illustrate β_k^P, while diamond markers for high homicide risk illustrate $\beta_k^P + \sqrt{9}\beta_k^{PH}$. Bars denote 90% confidence intervals.

of homicide risk. The plot illustrates differences in the park premium in areas with no homicide risk versus areas with high homicide risk (i.e. 9 annual homicides per square mile). With no risk, the model estimates a park premium that decays with distance to the park. When risk is high, we see evidence of a park discount that also decays with distance to the park.[20] Furthermore, the effects of proximity to a neighborhood park disappear after the 2nd distance interval, which justifies our choice to collapse the 3rd, 4th, and 5th bins into a single comparison group, P_{iA}. Fig. 1 shows P_{iA} in red for Marquette Park. This is equivalent to constraining 3rd, 4th, and 5th bins to have the same coefficient, or using $\tilde{\mathbf{P}}_\mathbf{i} \equiv [P_{i1}, P_{i2}, P_{iA}, P_{i6}]$, where $P_{iA} = P_{i3} + P_{i4} + P_{i5}$. Transactions in the outer bin, P_{i6}, behave somewhat differently. In some cases, certain transactions in bin P_{i6} are assigned to a separate park, which raises a selection issue. Rather than include it in our comparison sample, we separately estimate P_{i6} as a nuisance parameter in our models.

5. Regression analysis of park-safety complementarity

5.1. Identifying the park premium and park-safety complementarity

In this section, we specify our hypotheses and identification strategies using a regression equation that builds on the NSD method introduced in Eq. (5). This equation uses the abbreviated park-distance bins, $\tilde{\mathbf{P}}_\mathbf{i}$, the (square root) transformation of homicide risk, $\tilde{H}_{ijt}$, and introduces time-varying socio-economic controls for race, income, tenure, unemployment, and population density X_{ijt}, as listed in Panel C of Table 1. Importantly, we include interactions between these and the park and homicide variables:

$$V_{ijt} = \tilde{\mathbf{P}}_\mathbf{i}\beta^P + \tilde{H}_{ijt}\beta^H + \tilde{\mathbf{P}}_\mathbf{i}\tilde{H}_{ijt}\beta_k^{PH} + D_i\beta^D + X_{ijt}\beta^X + \sum_k \tilde{P}_{ki}X_{ijt}\beta_k^{XP} + \tilde{H}_{ijt}X_{ijt}\beta^{XH} + \gamma_j + \gamma_j^T \times t + \zeta_t^c + u_{ijt}. \quad (6)$$

In addition, we control for neighborhood-specific time trends, $\gamma_j^T \times t$.

Formally, the empirical design is centered around two hypotheses. The primary hypothesis is that parks and safety are complements. This implies that $\beta_1^{PH} < 0$ and that $\beta_k^{PH} > \beta_{k+1}^{PH}$. In words, the negative park–crime interaction attenuates with distance from the park. The secondary hypothesis is that safe parks are goods. This means that $\beta_1^P > 0$ and that $\beta_k^P \geq \beta_{k+1}^P$. At zero homicide risk, the park premium for bin k is given by β_k^P; with any positive level of homicide risk, it becomes $\beta_k^P + \beta_k^{PH}\tilde{H}_{ijt}$. If the value of parks can become negative at high enough levels of crime, then at that threshold $\tilde{H}'_{ijt}$, it becomes $\beta_1^P + \beta_1^{PH}\tilde{H}_{ijt} = 0$. This is the threshold above which park access within a neighborhood does not confer a premium. It is important to note that through the effects of complementarity, park value can be "unlocked" both above and below $\tilde{H}'_{ijt}$. We later provide empirical estimates of "unlocked" value in high-crime and low-crime neighborhoods in our sample. Neither hypothesis requires estimating the causal effect of crime on property values outside of the interaction.

In order to obtain unbiased estimates of the park–crime interaction, β^{PH}, the error term must be orthogonal to the park–crime interaction, *conditional* on the control variables, the main effects of parks and crime, and their interactions with the controls. To state this precisely with the interaction terms, let L denote the linear projection[21], and omit obvious subscripts and tildes to write:

$$L\left(u|PH, P, H, X, PX, HX, W\right) = L\left(u|P, H, X, PX, HX, W\right) \quad (7)$$

where $W = \left[D, \gamma_j, \gamma_j^T \times t, \zeta_t^c\right]$ denotes the control variables not interacted with parks or crime. This condition allows all of the regressors in Eq. (6) to have biased coefficient estimates, except for the park–crime interaction. The interactions of the controls with park proximity, and separately with crime, help to control for other sources of complementarity or interactions that may influence property prices.[22] For example, as X includes household income, the interactions control for whether parks are less valuable in areas with

[20] The discount is not strongly significant when we estimate individual effects for each distance bin. We provide formal tests with greater statistical power in later sections.

[21] This requires a standard linearity in parameters assumption, as in Wooldridge (2010).

[22] In practice, the X variables are demeaned so as not to change the estimated coefficient under this condition.

poorer residents. Since the socio-economic variables, X, are potentially endogenous in the sense that they could also be outcomes of changes in crime, it is important to see estimates that exclude as well as include these variables. We will show that estimates of β^{PH} and β^{P} are not sensitive to their inclusion.

For estimates of the park premium, a parallel condition applies:

$$L\left(u|P, H, X, HX, W\right) = L\left(u|H, X, HX, W\right). \tag{8}$$

Homicide risk, socio-economic characteristics, their interaction, and the other non-park variables must absorb any additional variation due to unobserved factors that might bias the estimate of the main effect of parks. Bias in β^{P} results from price-influencing unobservables that are correlated with parks and are orthogonal to homicide risk. For example, unobserved property-level characteristics could result in differences in the price premium. The identification assumption for β^{P} is likely to be more difficult to satisfy than for β^{PH}. The set of unobservables that may be correlated with the interaction of park proximity and crime seems smaller than those that may be correlated with park-proximity in general.

As we substantiate below, the condition for identifying the park premium may be less demanding than a similar condition for the direct effect of crime. This could be due to the fact that parks are distributed more evenly than homicide risk. Visually, this can be seen by comparing parks shown in Fig. 1 and the homicide measure in Chicago seen in Fig. 2. As mentioned earlier, 80% of the homicide risk we measure is concentrated on just 13% of land.

The identifying assumption for β^{PH} is violated by confounds that affect the value of park proximity within a few blocks and simultaneously vary with homicide risk. We distinguish between three types of omitted variables concerns by decomposing the error term in Eq. (5) into:

$$u_{ijt} = \underset{(1)}{\upsilon_{jt}} + \underset{(2)}{\sigma_i} + \underset{(3)}{\chi_{ijt}} + \mu_{ijt}$$

where the first three terms include unobservables that are correlated with homicide risk and affect park premium: (1) υ_{jt} includes time-varying unobservables *between* neighborhoods; (2) σ_i includes time-invariant, property-level characteristics, within neighborhoods; (3) χ_{ijt} includes all other time-varying unobservables that may produce bias. μ_{ijt} is idiosyncratic and unrelated to the interaction term after conditioning on the controls.[23] Accordingly, we report estimates from the core NSD model and then evaluate their robustness to possible biases. We include neighborhood-specific time trends and neighborhood-by-year fixed effects to help address *between-neighborhood* differences (1). We address time-invariant property characteristics (2) using a true repeat-sales estimator, based on a small sample, and a "matching" repeat-sales estimator based on a larger sample. We address time-varying unobservables that may operate within neighborhoods (3) using the socio-economic controls and their interaction. We also consider an IV that uses city-level variation in crime rates, which identifies the complementarity assuming that city-level fluctuations are exogenous to the differential changes that occur *within* a tight perimeter around parks.

5.2. Neighborhood spatial differences

Table 2 reports estimates from model (6), with successive levels of controls. We report effects for the closest 0–1/16th distance-to-park interval, β_1^P, as well as the second, 1–2/16ths mile interval, β_2^P, since the latter also shows some evidence of a park premium. The reference category in this specification is the 2–5/16ths mile distance interval.

Column 1 reports estimates from a specification that ignores the interaction between park access and homicide risk, which the remaining columns include. This coefficient on park proximity, β^P, refers to a mean effect across all risk levels. All specifications include neighborhood fixed effects, city-by-year fixed effects, and observable dwelling characteristics. Specification 3 adds time-varying socioeconomic controls, 4 adds neighborhood time trends, and 5 adds the socio-economic control interactions.

Column 1 suggests that homes within roughly one block of parks sell for approximately 3 percentage points more than those farther away. When the interaction is included, the premium, now for *safe* parks, rises to roughly 5 percentage points. This relates directly to the main result in the paper, which is seen in the fourth row: the park premium falls with crime, $\beta^{PH} < 0$. Across specifications, the magnitude is 1.2 to 1.3 percentage points for each increase in the transformed risk measure. With high enough risk, $-\beta^P/\beta^{PH} \approx 4.1$ (standard error $= 1.0$), the park premium becomes zero. De-transforming the risk variable, this corresponds to almost 4 homicides per square mile annually. Failure to account for the park–crime complementarity would underestimate the premium for *safe* parks by 35% while overestimating the premium for unsafe parks. It is also worth noting that the interaction is negative for the second park bin, albeit smaller and imprecise. While this estimate must be tempered by its imprecision, it further supports the hypothesis of complementarity.

The core estimates are largely stable across columns 2 through 5. As alluded to above, the main park and park–crime interaction effects change far less than the coefficient on crime. Moving from column 2 to column 3, we see that the latter is roughly halved with the addition of socio-economic controls, while the interaction term hardly changes and the main park effect falls by one-tenth. Neighborhood time trends added in column 4 further reduce the crime estimate, but slightly increase the park and park–crime estimates. Column 5, which saturates the model with the socio-economic interactions, hardly moves the estimates any further. While the time-varying controls could introduce simultaneity since they are potential outcomes of crime changes, we find that including them does not affect our main results.

The estimates in Table 2 may still be subject to omitted variables that vary *within* neighborhoods. For instance, reductions in local crime could coincide with capital improvements made in nearby parks. To consider the potential effects of omitted variables on our NSD estimates, we adopt the method developed by Altonji et al. (2005) and Oster (2017).[24] In particular, Oster (2017) introduces the coefficient of proportionality, δ. This statistic captures the effect of additional control variables on the coefficient of multiple correlation, R^2. If the main estimates are not sensitive to including observed variables that explain variance in the outcome, then the logic is that they are unlikely to be sensitive to remaining unobserved variables. $\delta \geq 1$ indicates that unobserved variables would need to be stronger confounders than the controls we consider in order to drive our estimate to zero.

The tests proposed in Oster (2017) depend on an assumption about the maximum possible R-squared that could be achieved in a model that accounts for the remaining unobservables, $R_{\max}$. It is unlikely that $R_{\max} = 1$ in our model, since our housing price data contain a great deal of variation unrelated to the underlying value of neighborhood amenities. Oster suggests as a rule of thumb using $R_{\max} = 1.3\tilde{R}^2$, where $\tilde{R}^2$ is the R-squared from the model with a full

[23] In Appendix B, we derive the conditions that identify the complementarity β^{PH} and β^{P}, and we refer to the unobservables that are correlated with homicide risk and affect the park premium as $\phi_{ijt} = \upsilon_{jt} + \sigma_i + \chi_{ijt}$.

[24] These estimates also rely upon the functional form that we have defined for the amenity value of parks as a function of distance.

Table 2
Price effects of the complementarity between park proximity and homicide risk: neighborhood spatial differences (NSD) estimates.

	Dependent variable: ln *Housing transaction price*				
Estimator	Neighborhood spatial differences (NSD)				
	(1)	(2)	(3)	(4)	(5)
Park within 1/16 mile	0.0320***	0.0546***	0.0488***	0.0515***	0.0500***
	(0.0106)	(0.0143)	(0.0142)	(0.0139)	(0.0155)
Park 1 to 2/16 mile	0.0033	0.0165	0.0128	0.0113	0.0135
	(0.0087)	(0.0117)	(0.0112)	(0.0111)	(0.0109)
Homicide Risk	−0.0272***	−0.0233***	−0.0115***	−0.0098***	−0.0091***
	(0.0029)	(0.0030)	(0.0028)	(0.0030)	(0.0034)
Park 1/16 mile × Homicide Risk		−0.0116***	−0.0119***	−0.0129***	−0.0132**
		(0.0042)	(0.0041)	(0.0040)	(0.0057)
Park 1 to 2/16 mile × Homicide Risk		−0.0065*	−0.0057	−0.0050	−0.0056
		(0.0036)	(0.0037)	(0.0036)	(0.0034)
Neighborhood Fixed Effects	Yes	Yes	Yes	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Dwelling characteristics	Yes	Yes	Yes	Yes	Yes
Time-var Socio-economic Controls			Yes	Yes	Yes
Neighborhood Time Trends				Yes	Yes
Park 1/16 × Time-var Socio-economic Controls					Yes
Hom. × Time-var Socio-economic Controls					Yes
R^2	0.2668	0.2670	0.2844	0.3020	0.3043
Observations	656,841	656,841	656,841	656,841	656,841

Notes: Sample includes transactions within 3/8 of a mile of a park for Chicago (2001–2016), New York (2006–2016), and Philadelphia (2006–2016) from Zillow (2018). Dependent variable is ln*(Housing transaction price)*, *Park within 1/16mile* is an indicator for sales within 1/16 mile of a park, *Park 1 to 2/16mile* is an indicator for sales between 1/16 mile and 2/16 mile of a park, *Homicides Risk* denotes the yearly number of expected homicides per square mile at the *Property level*. This variable is defined using the following transformation: $\left(\tilde{H} = 2\sqrt{H}\left(\sqrt{(H)}\right)^{-1}\right)$. The reference category in this specification includes properties between 2 and 6/16ths miles away. Dwelling characteristics include: log distance to the CBD, age of the dwelling and its square, square footage, number of bedrooms and bathrooms, and dwelling type (i.e. Single, Multi Family Residence). Specifications also include indicators for dwellings with missing characteristics. Results are robust to restricting the sample to dwellings with complete data. Socio-economic controls include the following census block and block group socio-economic variables linearly interpolated yearly from the 2000 and 2010 Censuses, and the 2011–2015 ACS: population density, proportion of Blacks, proportion of Latinos, proportion of vacant housing units and of rented units at the block level; and median age, median income and unemployment rate at the block group level. Neighborhood is defined using a 3/8 mile radius around a park. Standard errors clustered at the neighborhood level are in parentheses.
* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

set of controls.[25] This rule may be too demanding given that housing transaction data may have considerable noise and idiosyncrasies.

In Table 3, we report δ for a range of values for $R_{\max}$ using all of the control variables introduced in columns 2–5. We find that the δ values for the park–crime interaction are at least 8 times higher than the main effect of crime. In other words, the former is only one-eighth as sensitive to additional controls as the latter. The values of δ for the main effect of parks are nearly as high as the values for the interaction. In absolute magnitude, the interaction coefficient meets the condition $\delta > 1$ even for the highest value of $R_{\max} = 1.3$, while that for parks is right at the cusp. Indeed, this criterion is demanding: Oster (2017) finds that 55% of non-experimental findings published in top journals would not meet it. Nevertheless, the overall robustness of both the main park and park–crime interaction suggests that the spatial differencing method is quite effective at controlling for unobservables related to parks, if not for crime (Druckenmiller and Hsiang, 2018).

5.3. Robustness checks over space and time

Table 4 reports estimates with even more flexible controls for neighborhood differences over time. Column 1 replicates column 4 from above, which includes 1336 neighborhood-specific time trends. Column 2 allows for time trends to differ for properties near parks versus farther away. This can account for differing trends in park premia over local booms and busts. Column 3 introduces neighborhood-by-year fixed effects, controlling flexibly for time-varying differences between neighborhoods. Neither of these specifications weakens the park–crime interaction.

The checks in Table 5 use measures of homicide risk averaged across the entire neighborhood or over the entire sample period. The final measure excludes crime incidents that occur within 1/8 mile of parks. Not surprisingly, the longer time-average in column 2 produces slightly larger interaction effects. This could result from smoothing annual variation in homicides.

With the neighborhood-level measure, the estimates exclude variation in homicide risk across properties within neighborhoods. The coefficient on the park–crime interaction in column 3 is smaller than in 1, likely reflecting attenuation from increased measurement error, but is not significantly different from those reported in Table 2. The measure in column 4 uses only time-invariant variation in homicide rates between neighborhoods. Thus, it cannot estimate the main effect of homicides while including neighborhood fixed effects. In

Table 3
Price effects of the complementarity between park proximity and homicide risk: robustness to omitted variable bias.

$R_{\max}$	$1.1\tilde{R}^2$	$1.2\tilde{R}^2$	$1.3\tilde{R}^2$
Park within 1/16 mile	2.9477	1.4782	0.9864
Homicide Risk	0.5157	0.2583	0.1723
Park 1/16 × Homicide Risk	4.7035	2.3555	1.5711

Notes: This table reports the Oster (2017) proportionality coefficient. Coefficients from Table 2 Column (2) are compared with our full set of controls shown in Column (5). $\tilde{R}^2$ corresponds to the R-squared from Table 2 Column (5).

[25] The rule-of-thumb $R_{\max} = 1.3\tilde{R}^2$ is derived from her analysis of 65 results from published papers. The 1.3 cutoff is a value that allows 90% of the experimental results examined to 'survive', meaning that the unobservables explain less of the variation in the outcome than the observables.

Table 4
Price effects of the complementarity between park proximity and homicide risk: neighborhood spatial differences (NSD) estimates.

	Dependent variable: ln *Housing transaction price*		
Estimator	Neighborhood spatial differences (NSD)		
	(1)	(2)	(3)
Park within 1/16 mile	0.0515***		0.0522***
	(0.0139)		(0.0138)
Park 1 to 2/16 mile	0.0113**		0.0097*
	(0.0111)		(0.0112)
Homicide Risk	−0.0098***	−0.0079***	−0.0158***
	(0.0030)	(0.0029)	(0.0041)
Park 1/16 mile × Homicide Risk	−0.0129***	−0.0144***	−0.0131***
	(0.0040)	(0.0054)	(0.0041)
Park 1 to 2/16 mile × Homicide Risk	−0.0050	−0.0047	−0.0048
	(0.0036)	(0.0033)	(0.0037)
Neighborhood Fixed Effects	Yes	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes	Yes
Dwelling characteristics	Yes	Yes	Yes
Time-Var Socio-Economic Controls	Yes	Yes	Yes
Neighborhood Time Trends	Yes		
Near-Far-Park Time Trends		Yes	
Neighborhood by Year Fixed Effects			Yes
R^2	0.3020	0.3210	0.3461
Observations	656,841	656,841	656,841

Notes: Sample and variables are the same as described in Table 2, which includes transactions within 3/8 of a mile of a park for Chicago (2001–2016), New York (2006–2016), and Philadelphia (2006–2016) from Zillow (2018). Neighborhood is defined using a 3/8 miles radius around a park. Standard errors clustered at the neighborhood level are in parentheses.
* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

contrast, the repeat-sales estimator below relies exclusively on time variation and therefore cannot estimate the time-invariant effect of park proximity. The result in column 5 is about as strong as the estimate in column 3, suggesting that the estimates are not driven by homicides occurring within parks.

5.4. Repeat-sales estimators

One concern with the estimates above is that properties in high-crime areas could possess unobserved characteristics that differ from those farther away. For instance, houses near safe parks may have

Table 5
Price effects of the complementarity between park proximity and homicide risk: neighborhood spatial differences (NSD) estimates property versus neighborhood-level homicide risk, time-varying and fixed averages.

	Dependent variable: ln *Housing transaction price*				
Homicide risk level	Property	Property	Neighborhood	Neighborhood	Neighborhood excluding risk within 1/16 miles of park
Estimator	Neighborhood spatial differences (1)	Sample time average crime (2)	Neighborhood spatial differences (3)	Sample time average crime (4)	Neighborhood spatial differences (5)
Park within 1/16 mile	0.0488***	0.0590***	0.0472***	0.0630***	0.0473***
	(0.0142)	(0.0174)	(0.0166)	(0.0202)	(0.0167)
Park within 2/16 mile	0.0128	0.0196	0.0087	0.0212	0.0090
	(0.0112)	(0.0141)	(0.0126)	(0.0152)	(0.0125)
Homicide Risk	−0.0115***	−0.0281***	−0.0137***		−0.0145***
	(0.0028)	(0.0078)	(0.0032)		(0.0033)
Park within 1/16 mile × Homicide Risk	−0.0119***	−0.0185***	−0.0087**	−0.0179**	−0.0091**
	(0.0041)	(0.0063)	(0.0044)	(0.0070)	(0.0046)
Park within 2/16 mile × Homicide Risk	−0.0057	−0.0095*	−0.0029	−0.0096*	−0.0031
	(0.0037)	(0.0051)	(0.0034)	(0.0055)	(0.0035)
Neighborhood Fixed Effects	Yes	Yes	Yes	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Dwelling characteristics	Yes	Yes	Yes	Yes	Yes
Time-Var Socio-Economic Controls	Yes	Yes	Yes	Yes	Yes
Observations	656,841	656,841	656,841	656,841	656,841

Notes: Sample and variables are the same as described in Table 2, which includes transactions within 3/8 of a mile of a park for Chicago (2001–2016), New York (2006–2016), and Philadelphia (2006–2016) from Zillow (2018). Columns vary in the level of Homicide Risk we use, as described at the top of each column. Neighborhood is defined using a 3/8 miles radius around a park. Standard errors clustered at the neighborhood level are in parentheses.
* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

Table 6
Price effects of the complementarity between park proximity and homicide risk: NSD + repeat-sales estimates.

Estimator	*Dependent variable:* ln *Housing transaction price*		
	Neighborhood spatial differences (NSD)		
	NSD (1)	NSD + Matching Repeat Sales (2)	NSD + True Repeat Sales (3)
Park within 1/16 mile	0.0488***		
	(0.0142)		
Park 1 to 2/16 miles	0.0128		
	(0.0112)		
Homicide Risk	−0.0115***	−0.0143***	−0.0024**
	(0.0028)	(0.0028)	(0.0010)
Park within 1/16 mile × Homicide Risk	−0.0119***	−0.0109*	−0.0056***
	(0.0041)	(0.0056)	(0.0019)
Park 1 to 2/16 mile × Homicide Risk	−0.0057	−0.0050	−0.0015
	(0.0037)	(0.0037)	(0.0027)
Neighborhood Fixed Effects	Yes	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes	Yes
Dwelling characteristics	Yes	Yes	
Time-Var Socio-Economic Controls	Yes	Yes	
Observations	656,841	543,256	172,399

Notes: Sample and variables are the same as described in Table 2, which includes transactions within 3/8 of a mile of a park for Chicago (2001–2016), New York (2006–2016), and Philadelphia (2006–2016) from Zillow (2018). Neighborhood is defined using a 3/8 miles radius around a park. Samples for the repeat sales as a matching estimator and the true repeat sales were constructed using the McSpatial R package (McMillen, 2013). Standard errors clustered at the neighborhood level are in parentheses.
* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

larger windows, while those in dangerous areas may not. If so, then the estimates in Table 2 could be confounded by complementarities between parks and certain housing characteristics. To address the effects of unobserved fixed property characteristics (σ_i), Table 6 compares the estimate in column 3 from Table 2 with specifications that rely upon repeat-sales estimation within the NSD framework.

Column 2 reports estimates from a repeat-sales-as-matching estimator developed by McMillen (2012). It generates a counterfactual by matching home sales in the first year of the sample to properties in each subsequent year.[26] However, the interaction effect remains significant at almost the same magnitude, though less precisely estimated.

Column 3 provides estimates from a standard repeat-sales model that uses a much smaller sample (about one quarter) of properties that sell more than once during the study period. These estimates rely completely on time variation in prices, such that the value of park proximity cannot be estimated. In contrast to the estimates from the matched model, the main estimated effect of homicide risk is closer to zero, possibly due to reduced variation in the sub-sample. Despite this reduction in variation, the park–crime interaction effect remains precise and significant.[27]

5.5. City-level crime instrument

A remaining concern regarding the estimates reported in Tables 2 and 6 is that neighborhood-level dynamics may cause the park premium to rise as crime falls, for reasons that the controls do not account for. For instance, nearby public housing demolitions could act as a confounding variable by lowering local crime while improving the view of park-side properties.[28]

To address this, we develop an IV strategy that uses changes in crime at the city-year level to estimate effects on the park-safety complementarity within neighborhoods. This IV predicts local crime levels using changes in the crime rate at the city level, allocating those changes in proportion to the share of crimes observed at the beginning of the sample. It is similar to the shift-share IV estimator developed by Bradbury et al. (1982) and Bartik (1991) for non-crime measures and examined by Goldsmith-Pinkham et al. (2018) and Borusyak et al. (2018). The city-year changes that motivate this instrument are seen in Fig. 3. Much of the variation in local homicide risk can be attributed to declines in aggregate homicide rates in these cities. Indeed, Pope and Pope (2012) argue that much of the variation in city-level homicide in the U.S. is explained by aggregate shifts occurring across the country rather than by local socio-economic changes.

We define the instrument formally by denoting the total annual homicides in a city in year t as H_t^c. Altering slightly our previous notation, we define p_{ij0} as the probability of a homicide in property i in neighborhood j in the base year 0. Thus, the predicted expected number of homicides at each location i is

$$H_{ijt}^{IV} = p_{ij0} H_{-jt}^{c}. \tag{9}$$

To calculate the base probabilities, we use the first two years of the sample, which excludes them from the regression analysis.[29] To avoid any mechanical upward bias in the instrument, the measure H_{-jt}^{c} omits homicides occurring in the same neighborhood j.

Using two-stage least squares (2SLS), we estimate Eq. (6), without interactions, treating $\tilde{H}_{ijt}$ and $\tilde{\mathbf{P}}_{\mathbf{i}}\tilde{H}_{ijt}$ for $i = 1, 2$ as endogenous. To do so, we use a three-equation first stage where the projected homicide

[26] Samples for the repeat-sales-as-a-matching estimator and the true repeat sales were constructed using the McSpatial R package (McMillen, 2013). Park proximity is an observed characteristic and used as a control, but because the sample is properly balanced, it is not significant. Therefore, that coefficient drops out of this model.

[27] If homeowners expect additional changes in crime, then our estimates may be biased as shown by Bishop and Murphy (2015). However, recent crime trends appear to deviate from historical trends in the last few years. This makes it difficult to construct a forecast that would credibly match expectations of homebuyers.

[28] See Aliprantis and Hartley (2015) and Diamond and McQuade (2019) for related literature.

[29] We use homicide data for 2001–2002 for Chicago, and 2006–2007 for NYC and Philadelphia as our base period.

Table 7
Price effects of the complementarity between park proximity and homicide risk: first stage for city-level crime instrument (omits the first two years of sample).

	Dependent variable:			
	Homicide Risk	Homicide Risk	Park 1/16 × Homicide Risk	Park 1 to 2/16 mile × Homicide Risk
	No Interaction	Interaction		
	(1)	(2)	(3)	(4)
Projected Homicide Risk	0.4506***	0.4514***	−0.0333***	−0.0498***
	(0.0117)	(0.0117)	(0.0053)	(0.0045)
Park 1/16 × Projected Homicide Risk		0.0033	0.6775***	0.0082*
		(0.0097)	(0.0136)	(0.0052)
Park 1 to 2/16 × Projected Homicide Risk		−0.0066	0.0068**	0.6743***
		(0.0066)	(0.0034)	(0.0119)
1^{st} Stage *F*-statistic	1461.88	496.85	848.21	1071.56
Neighborhood Fixed Effects	Yes	Yes	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes	Yes	Yes
Dwelling characteristics	Yes	Yes	Yes	Yes
Time-Var Socio-Economic Controls	Yes	Yes	Yes	Yes
Observations	521,945	521,945	521,945	521,945

Notes: Sample and variables are the same as described in Table 2, without the first two years of the sample for each city, i.e. Chicago (2003–2016), New York (2008–2016), and Philadelphia (2008–2016) from Zillow (2018). We use the first two years to construct the instrument. Kleibergen and Paap (2006) *F*-statistics are reported at the bottom of the table. Standard errors clustered at the neighborhood level are in parentheses.
* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

risk instrument and its interactions with the park indicators enter as separate instruments.

$$H_{ijt} = \tilde{\mathbf{P}}_{\mathbf{i}}\pi_0^P + H_{ijt}^{iv}\pi_0^H + \tilde{\mathbf{P}}_{\mathbf{i}}H_{ijt}^{IV}\pi_0^{PH} + D_i\pi_0^D + X_{ijt}\pi_0^X + \gamma_{j0} + \zeta_{t0}^c + e_{ijt0} \quad (10a)$$

$$P_1H_{ijt} = \tilde{\mathbf{P}}_{\mathbf{i}}\pi_1^P + H_{ijt}^{iv}\pi_1^H + \tilde{\mathbf{P}}_{\mathbf{i}}H_{ijt}^{IV}\pi_1^{PH} + D_i\pi_1^D + X_{ijt}\pi_1^X + \gamma_{j1} + \zeta_{t0}^c + e_{ijt1} \quad (10b)$$

$$P_2H_{ijt} = \tilde{\mathbf{P}}_{\mathbf{i}}\pi_2^P + H_{ijt}^{iv}\pi_2^H + \tilde{\mathbf{P}}_{\mathbf{i}}H_{ijt}^{IV}\pi_2^{PH} + D_i\pi_2^D + X_{ijt}\pi_2^X + \gamma_{j2} + \zeta_{t2}^c + e_{ijt2} \quad (10c)$$

Using a three-equation first stage accounts for possible correlations between $\hat{H}_{ijt}$ and $\tilde{P}_kH_{ijt} - \widehat{\tilde{P}_kH_{ijt}}$, or $\widehat{\tilde{P}_kH_{ijt}}$ and $H_{ijt} - \hat{H}_{ijt}$, and $\tilde{P}_kH_{ijt} - \widehat{\tilde{P}_kH_{ijt}}$ (Angrist and Pischke, 2008).

In this "NSD + IV" framework (Neighborhood Spatial Differencing with Instrumental Variables), changes in property-level crime are instrumented by city-level variation while also conditioning on neighborhood fixed effects and time trends. The exclusion restriction in this model requires that $E\left(P_iH_{ijt}^{IV}u_{ijt}|.\right) = 0$, where "|." denotes conditional on the control variables.[30] By conditioning on neighborhood fixed effects and time trends, the NSD + IV framework mitigates concerns about the endogeneity of initial crime shares (Borusyak et al., 2018; Goldsmith-Pinkham et al., 2018). In identifying the park–crime complementarity, the IV strategy differs from other shift-share approaches by relying on the effects of city-level (i.e., market-level) shifts on within-neighborhood differences.

Table 7 reports the results of an uninteracted model in column 1 and the interacted model in columns 2–4.[31] Consistent with the trends illustrated in Fig. 3, the first stage results of the IV regression in Table 7 indicate that city-level changes in homicide risk predict local changes quite well. For every unit increase in annual homicides at the city level, we find a corresponding 0.45 increase at the neighborhood level. Under simplifying assumptions, this implies that roughly 45% of local homicide variation is driven by city-wide trends.

Table 8 provides estimates from five specifications of the NSD + IV model. The NSD + IV estimates for the safe park premium and the interaction are both similar to corresponding specifications from the NSD model without instruments in Tables 2 and 4, adding various time controls. In fact, Wu-Hausman tests fail to reject differences between comparable specifications of the NSD and IV + NSD models in all five specifications. The IV estimate for the complementarity is larger and less precise when parks and time trends are interacted. While the identifying variation in the IV has its limits, overall, the estimates are stable in specifications 2, 3, and 5. The park–crime complementarity is slightly larger, while the main effect of (safe) parks is slightly smaller. Combining the two, the homicide level at which the park premium reaches zero here at $-\beta^P/\beta^{PH} \approx 3.7$ (s.e. = 0.8), implying roughly 2.3 homicides per square mile.

5.6. Additional checks and mechanisms

The exclusion restriction in our NSD + IV model is not directly testable. It would be violated if the instrument is correlated with changes in other public goods near parks. We examine the relationships between the instrument and block-level unemployment rates in Table B.1 and zip-level restaurant establishments in Table B.3. These tests indicate that the instrument is not correlated with these outcomes.

The results in Table B.2 indicate that city-level changes in crime do not predict different effects on within-neighborhood changes in crime near versus far from parks. This finding reinforces our interpretation of our IV estimates as capturing the price effects of safety changes near parks as opposed to changes in relative safety near parks. In Table B.4, we test the robustness of our IV estimates to lags in the initial shares of homicide risk by omitting the first 5 years of housing transactions in each city.[32] Our estimates become slightly

[30] See Appendix B for a more detailed exposition of the exclusion restriction in the NSD + IV model. The standard relevance condition is also required: $E(P_iH_{ijt}, P_iH_{ijt}^{IV}|.) \neq 0$.

[31] Column 1 reports the results of an uninteracted model, in that case, there is only one endogenous variable and the first stage is reduced to Eq. (10a).

[32] This tests the sensitivity of our estimates to assumptions about the within-neighborhood distribution of initial homicide risk.

Table 8
Price effects of the complementarity between park proximity and homicide risk: NSD + IV estimates (omits the first two years of sample).

	Dependent variable: ln *Housing transaction price*				
Estimator	Neighborhood spatial differences (NSD)				
	NSD + IV (1)	NSD + IV (2)	NSD + IV (3)	NSD + IV (4)	NSD + IV (5)
Park within 1/16 mile	0.0200**	0.0484***	0.0478***		0.0496***
	(0.0099)	(0.0176)	(0.0174)		(0.0175)
Park 1 to 2/16 mile	0.0011	0.0206	0.0170		0.0165
	(0.0079)	(0.0131)	(0.0129)		(0.0131)
Homicide Risk	−0.0302***	−0.0255***	−0.0272***	−0.0203**	−0.0259***
	(0.0098)	(0.0094)	(0.0097)	(0.0092)	(0.0099)
Park 1/16 mile × Homicide Risk		−0.0150**	−0.0145**	−0.0429*	−0.0148**
		(0.0066)	(0.0066)	(0.0254)	(0.0067)
Park 1 to 2/16 mile × Homicide Risk		−0.0099*	−0.0081	−0.0092*	−0.0079
		(0.0054)	(0.0052)	(0.0052)	(0.0054)
Wu-Hausman Test (*p*-values)	0.1367	0.2604	0.2399	0.1973	0.5862
	NSD (1)	NSD (2)	NSD (3)	NSD (4)	NSD (5)
Park within 1/16 mile	0.0218**	0.0455***	0.0471***		0.0460***
	(0.0100)	(0.0146)	(0.0142)		(0.0143)
Park 1 to 2/16 mile	0.0018	0.0104	0.0091		0.0087
	(0.0079)	(0.0110)	(0.0109)		(0.0112)
Homicide Risk	−0.0178***	−0.0142***	−0.0138***	−0.0121***	−0.0203***
	(0.0032)	(0.0032)	(0.0033)	(0.0032)	(0.0044)
Park 1/16 mile × Homicide Risk		−0.0126***	−0.0131***	−0.0100*	−0.0123***
		(0.0044)	(0.0044)	(0.0058)	(0.0045)
Park 1 to 2/16 mile × Homicide Risk		−0.0044	−0.0036	−0.0040	−0.0036
		(0.0035)	(0.0034)	(0.0031)	(0.0035)
Neighborhood Fixed Effects	Yes	Yes	Yes	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Dwelling characteristics	Yes	Yes	Yes	Yes	Yes
Time-Var Socio-Economic Controls	Yes	Yes	Yes	Yes	Yes
Neighborhood Time Trends			Yes		
Near-Far-Park Time Trends				Yes	
Neighborhood by Year FE					Yes
Observations	521,945	521,945	521,945	521,945	521,945

Notes: Sample and variables are the same as described in Table 2, without the first two years of the sample for each city, i.e. Chicago (2003–2016), New York (2008–2016), and Philadelphia (2008–2016) from Zillow (2018). We use the first two years to construct the instrument. Standard errors clustered at the neighborhood level are in parentheses.
* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

larger, likely as a result of the sampling restriction, but are not significantly different from the estimates using the full sample of data. Changes in expenditures on new equipment or community programs could occur simultaneously with reductions in crime and confound estimates of the complementarity. Estimates reported in Appendix Table A4 indicate that estimates for the park premium and the complementarity are robust to controls for changes in annual public expenditures on parks across three regions delineated by the Chicago Parks Department.[33]

We also implement checks that are related to park use. Most parks are frequented less and often closed at night, potentially magnifying the effect of the complementarity with daytime crime. Estimates presented in Appendix Table A5 show that park–crime interaction when using daytime crime is slightly stronger in the NSD + IV specification.[34] We also examine heterogeneity in effects across different sized parks by dividing parks into quartiles of size.[35] The estimates illustrated in Appendix Fig. A4 indicate that the effects of the proximity and the complementarity are greater for large parks. In fact, the park premium for small parks is insignificant, albeit imprecise.[36]

6. Endowment heterogeneity, preferences, and selection

6.1. Disentangling complementarities from taste heterogeneity

The findings reported above indicate that differences in public good endowments may change how households value complementary public goods. These endowments are often correlated

[33] Data come from publicly available annual budget appropriations documentation for the City of Chicago: https://www.chicagoparkdistrict.com/about-us/departments/budget-and-management. They are available for parks in north, central and south regions of the City during the years 2011–2017.
[34] Estimates are not different in the regular NSD model.
[35] Presumably, larger parks are more valuable. Thus, both the park premium and the interaction should be stronger around larger parks. In Chicago, parks in the fourth quartile (above 75th percentile) have a minimum size of 4.83 acres, in New York 4.18 acres, and 10.88 acres in Philadelphia.
[36] We implement two additional robustness tests to evaluate the effects of anomalies in our sample and crime variable as a robustness check. Appendix Table A3 shows that results remain unchanged when we drop 2016 to exclude the spike in homicides in Chicago in that year. Appendix Table A4 reports estimates using the measure of homicide equivalent risk that includes all crime types, weighted by existing estimates of WTP from Chalfin and McCrary (2018) . Effects are less precisely estimated in this model and possibly affected by attenuation bias from misreporting of minor crimes, though the magnitudes cannot be ruled out using the confidence intervals around our main estimates.

Table 9
Price effects of the complementarity between park proximity and homicide risk: disentangling complementarity from taste heterogeneity.

Estimator	*Dependent variable:* ln *Housing transaction price*				
	Neighborhood spatial differences (NSD)				
	(1)	(2)	(3)	(4)	(5)
Park within 1/16 mile	0.0260***	0.0503***	0.0257**	0.0484***	0.0500***
	(0.0101)	(0.0136)	(0.0100)	(0.0133)	(0.0155)
Homicide Risk	−0.0129***	−0.0091***	−0.0139***	−0.0104***	−0.0091***
	(0.0031)	(0.0031)	(0.0030)	(0.0031)	(0.0034)
Park within 1/16 mile × Homicide Risk		−0.0124***		−0.0116***	−0.0132**
		(0.0045)		(0.0040)	(0.0057)
Prop. Af. American	−0.2515***	−0.2531***	−0.2872***	−0.2885***	−0.2248***
	(0.0336)	(0.0335)	(0.0301)	(0.0301)	(0.0354)
Prop. Latino	−0.2435***	−0.2449***	−0.2716***	−0.2705***	−0.2430***
	(0.0244)	(0.0245)	(0.0273)	(0.0272)	(0.0274)
Park 1/16 × Prop. Af. American	−0.0448**	−0.0165			−0.0109
	(0.0214)	(0.0239)			(0.0339)
Homicide Risk × Prop. Af. American	−0.0118**	−0.0133**			−0.0284***
	(0.0057)	(0.0057)			(0.0077)
Park 1/16 × Prop. Latino			−0.0620*	−0.0493	−0.0111
			(0.0344)	(0.0333)	(0.0378)
Homicide Risk × Prop. Latino			0.0187**	0.0169**	−0.0059
			(0.0077)	(0.0076)	(0.0085)
Neighborhood Fixed Effects	Yes	Yes	Yes	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Dwelling characteristics	Yes	Yes	Yes	Yes	Yes
Time-Var Socio-Economic Controls	Yes	Yes	Yes	Yes	Yes
Neighborhood Time Trends	Yes	Yes	Yes	Yes	Yes
Park × Time-Var Socio-Economic Controls					Yes
Hom. × Time-Var Socio-Economic Controls					Yes
Observations	656,834	656,834	656,834	656,834	656,834

Notes: Sample and variables are the same as described in Table 2, which includes transactions within 3/8 of a mile of a park for Chicago (2001–2016), New York (2006–2016), and Philadelphia (2006–2016) from Zillow (2018). Neighborhood is defined using a 3/8 miles radius around a park. Standard errors clustered at the neighborhood level are in parentheses.
* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

with household characteristics. In that case, valuation models could mistake heterogeneity in the effects of public good endowments for differences in household tastes. Indeed, researchers have modeled differences in preferences across demographic groups extensively, e.g., Bayer et al. (2007). In the current setting, researchers could infer that residents in high-crime neighborhoods have weaker tastes for parks, when lack of safety may better explain this difference.[37]

The results presented in Table 9 explore whether the park premium varies by neighborhood demographic composition. These regressions interact the park premium with the share of residents that are either African-American or Latino. We then consider how robust these results are to including park–crime complementarity. Without the complementarity, the results in column 1 suggest little to no park premium in majority African-American neighborhoods. However, when the complementarity is included in column 2, the difference becomes statistically insignificant. A similar pattern emerges for Latino neighborhoods in columns 3 and 4. All interactions are included in column 5, which replicates the same column in Table 2.

These results suggest caution. Heterogeneity that could easily result in inferences about differences in tastes could alternately stem from complementary endowments.[38]

6.2. Household selection and welfare effects

Improvements in neighborhood safety may influence house prices not just by offering greater direct benefits, but also by affecting the composition of the neighborhood. For example, as safety improves, more affluent households may locate near urban parks. So could households with stronger tastes for open space.

Banzhaf (2015) illustrates that if the types of people buying homes change, then hedonic estimates provide the exact willingness to pay for amenities only under a restrictive set of assumptions. When private endowments and preferences of buyers are related to changes in amenities, Banzhaf establishes that hedonic estimates

[37] Other factors, such as housing market discrimination (Christensen and Timmins, 2018), can also contribute to differences in the hedonic estimates for local amenities such as parks.

[38] Economists have long asserted that researchers should, in principle, not look to differences in tastes to explain behavior (Silberberg and Suen, 2000; Stigler and Becker, 1977). Indeed, tastes are difficult to measure, and (behaviorist) choice models are not suited for providing testable predictions along taste lines.

Table 10
Socio-economic changes and the complementarity between park proximity and homicide risk.

Panel A	*Dependent variable:*							
	ln(Population density)		White, fraction		African-American, fraction		Latino, fraction	
Estimator	Neighborhood spatial differences (NSD)							
	(1) NSD	(2) NSD + IV	(3) NSD	(4) NSD + IV	(5) NSD	(6) NSD + IV	(7) NSD	(8) NSD + IV
Park within 1/16 mile	−0.0531**	−0.0504	0.0055	0.0111	0.0059	−0.0006	−0.0083	−0.0046
	(0.0206)	(0.0241)	(0.0061)	(0.0066)	(0.0044)	(0.0052)	(0.0043)	(0.0048)
	[0.0444]	[0.1642]	[0.4049]	[0.2835]	[0.2287]	[0.9698]	[0.1224]	[0.5851]
Homicide Risk	0.0595***	0.1157***	−0.0528***	−0.0996***	0.0377***	0.0679***	0.0147***	0.0318***
	(0.0058)	(0.0129)	(0.0040)	(0.0076)	(0.0042)	(0.0090)	(0.0034)	(0.0064)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Park within 1/16 mile × Homicide Risk	0.0144	0.0167	−0.0012	−0.0073**	0.0023	0.0078**	−0.0022	−0.0030
	(0.0072)	(0.0094)	(0.0021)	(0.0027)	(0.0021)	(0.0028)	(0.0021)	(0.0026)
	[0.3518]	[0.1693]	[0.7322]	[0.0307]	[0.4455]	[0.0307]	[0.4455]	[0.3715]
Observations	364,269	364,269	364,269	364,269	364,269	364,269	364,269	364,269

Panel B	*Dependent variable:* ln(Median income)		Median age		Renter, fraction		Vacant, fraction		Unmployed, fraction	
	NSD	NSD + IV	NSD	NSD + IV	NSD	NSD + IV	NSD	NSD + IV	NSD	NSD + IV
Park within 1/16 mile	0.0014	−0.0005	1.2943***	1.4997***	−0.0126	−0.0135	0.0043**	0.0022	0.0017	0.0012
	(0.0112)	(0.0131)	(0.2584)	(0.3072)	(0.0077)	(0.0091)	(0.0022)	(0.0027)	(0.0012)	(0.0016)
	[0.9011]	[0.9698]	[0.0000]	[0.0000]	[0.1852]	[0.3075]	[0.1224]	[0.5851]	[0.2287]	[0.5851]
Homicide Risk	−0.0783***	−0.1513***	−0.5970***	−1.4391***	0.0240***	0.0488***	0.0069***	0.0131***	0.0089***	0.0148***
	(0.0055)	(0.0112)	(0.0715)	(0.1482)	(0.0028)	(0.0055)	(0.0009)	(0.0017)	(0.0007)	(0.0015)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Park within 1/16 mile × Homicide Risk	−0.0006	−0.0045	−0.1249	−0.2910**	0.0038	0.0059*	−0.0018*	−0.0003	0.0001	0.0008
	(0.0048)	(0.0062)	(0.1039)	(0.1314)	(0.0027)	(0.0035)	(0.0010)	(0.0014)	(0.0007)	(0.0009)
	[0.8934]	[0.5259]	[0.4455]	[0.0805]	[0.4455]	[0.1693]	[0.3518]	[0.8248]	[0.8934]	[0.5158]
Observations	364,269	364,269	364,269	364,269	364,269	364,269	364,269	364,269	322,368	322,368

Notes: Sample includes a interpolated yearly series of socio-economic characteristics from the 2000, 2010 Censuses and the 2011–15 ACS at block level. All specifications include neighborhood fixed effects and year fixed effects. Neighborhood refers to the 3/8 miles radius around a park. Standard errors clustered at the neighborhood level are in parentheses. Benjamini and Hochberg (1995) adjusted *p*-values in brackets.
Specifications also include controls for park proximity between 1 and 2/16th miles and it's interaction with Homicide Risk, which we omit for clarity of exposition as these coefficients are never statistically significant.
* Significant at 10% level; ** significant at 5% level; *** significant at 1% level according to Benjamini and Hochberg (1995) adjusted *p*-values.

identify a lower bound on the Hicksian equivalent surplus associated with the amenity improvement.[39]

In order to examine changes in neighborhood composition, we examine the socio-economic characteristics used before as controls. This involves estimating an equation similar to those in prior sections:

$$X_{bjt} = \tilde{\mathbf{P}}_{\mathbf{i}}\beta^P + H_{bjt}\beta^H + \tilde{\mathbf{P}}_{\mathbf{i}}H_{bjt}\beta^{PH} + \gamma_j + \zeta_t^c + u_{bjt} \tag{11}$$

where X_{bjt} measures the socio-economic characteristic for block (or block group) b, in neighborhood j in year t. The right-hand-side terms are those described in Eq. (6).[40]

Table 10 reports the results of this exercise. As there are eight different characteristics, the significance of each variable must be adjusted for multiple hypotheses tests. Thus, in addition to standard single-hypothesis *p*-values, the table reports *p*-values that control for rates of false discovery (Benjamini and Hochberg, 1995). In this case, the standard NSD estimates suggest no significant change in local household characteristics. This suggests that most of the benefits of the park-safety complementarity accrue to households who resemble prior local buyers.

In the IV specification, two significant park–crime results do emerge. A one-unit decrease in homicide risk is associated with a 0.7 percentage point increase in the share of White households living near parks. We observe a similar decrease in the African-American share. Taking into account the main effect, a one-point decrease in risk raises the percentage of White households by 10.7 points.[41]

Given these magnitudes and the IV estimates from Table 8, we can calculate changes in willingness-to-pay even in the most extreme case where African-American households place zero value on parks regardless of homicide risk. These calculations indicate that increases in the White share could account for up to about one-eighth of the estimates of the park premium and the park-safety

[39] We note that despite the fact that illustrations in Banzhaf (2015) make use of a difference-in-difference setup, this main result generalizes across a large class of empirical models that satisfy a conditional independence assumption. In this class, exogeneous changes in an amenity are identified which could be simultaneous with changes in buyer characteristics.

[40] Our measures of demographic characteristics are annualized by interpolating the matched census blocks and block groups from the 2000, 2010 census, and the 2011–15 ACS. Population and population-by-race are obtained at the block level whereas median income and median age at the block group level, which is the finest geography that they are available.

[41] This estimate conservatively assumes that all changes in the demographic composition recorded by the Census/ACS are homeowners/buyers.

Table 11
Amenity value of park proximity and value "locked-in" by homicide risk.

	Estimated total values of park proximity				Aggregate housing values	Housing units
	No complementarity *($B in 2017 values)*	With complementarity	No crime in parks	Locked-in by crime		(1000s)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Total						
Total	13.9	15.4	25.7	10.3	553.0	811
Panel B: Total and breakdown by city and total						
Chicago	2.6	3.1	4.7	1.7	102.0	284
New York	10.8	11.8	20.0	8.2	429.8	467
Philadelphia	0.5	0.6	1.0	0.4	21.2	60
Panel C: Breakdown by average homicide risk of neighborhood						
Low	7.5	12.1	13.9	1.8	101.1	375
Medium	4.3	4.9	8.8	3.8	135.8	289
High	1.7	−1.6	3.1	4.7	316.1	147
Panel D: Breakdown by neighborhood median income						
Low	2.6	−0.4	4.7	5.1	298.9	205
Medium	3.4	4.0	6.3	2.3	188.1	274
High	7.9	11.7	14.7	3.0	66.0	332
Panel E: Breakdown by majority race/ethnicity of neighborhood						
White	10.2	14.8	18.9	4.1	405.6	495
Af. American	2.2	−0.5	4.1	4.6	88.4	192
Hispanic	1.5	1.1	2.7	1.7	59.0	124

Notes: Estimates of the value of parks for each city are based on the number of units within 1/16 miles of a park and the mean value from the 2013–17 ACS. The first column ignores the park–crime complementarity, while the second column includes it. Coefficients without the complementarity are 0.0257 (0.0103) for within 1/16 of a mile of a park, and −0.0153(0.0027) per homicide. Coefficients with complementarity are reported in Table 2 column (3). Column (3) report the total potential value of park proximity if parks were made perfectly safe, setting expected homicides zero. Column (4) reports the difference with column (3). Panel B breaks down differences by city. Panel C, by average homicide level. We calculate the expected number of homicides in a neighborhood by year and classify them as: Low Homicide Risk: less than one expected homicide by year; Medium Homicide Risk: more than one and less than three expected homicides per year; High Homicide Risk: more than three expected homicides per year. Panel D, by average home value. We divide neighborhoods based on the median neighborhood income from the 2000 Census and classify them using city-specific income terciles. Panel E, by race and ethnicity. We divide neighborhoods by the majority shares based on the 2000 Census.

complementarity.[42] We note that this calculation reflects an upper bound that assumes extreme differences in group-specific valuation, which are not supported by the empirical evidence reported in the prior section. The estimates in Table 9 do not find evidence of differences in the premium for safe parks in neighborhoods with greater shares of African-Americans.

We cannot rule out a change in willingness-to-pay that results from unobserved changes in buyer preferences and therefore interpret our estimates as a lower bound welfare measure. However, we find that observed changes in neighborhood composition are either statistically insignificant or economically small.

7. Valuing park proximity with complementarity

Accounting for complementarity can change estimates of the aggregate benefits of public goods, and even more dramatically, how those benefits are distributed. We stress that these estimates only capture the value of park proximity for nearby residents.

7.1. Implications of complementarities for valuation and unlocking value

To calculate the implied total value of park proximity in our 3 cities, we apply the NSD estimates from (3) of Table 2 to all of the housing units reported in the Census.[43] The calculations sum up property values reported in intervals from the census block group level from the 2013–2017 ACS in 2017 dollars.[44] The first column of Table 11 ignores the park–crime complementarity, while the second column includes it.[45] Column 3 reports the total potential value of park proximity if parks were made perfectly safe, setting expected homicides to zero. This would not require eradicating crime altogether, since crime near parks could be displaced through targeted safety programs. Column 4 reports the difference with column 3. Panel B breaks down differences by city; Panel C, by average homicide level; Panel D, by average home value; Panel E, by race and ethnicity.

The results at the bottom of Panel A, in column 1, indicate that the total value of park proximity is $14 billion, while in column 2 it is slightly higher at just over $15 billion. This second number is

[42] This calculation treats the estimated park premium as a population-weighted average of the valuations of three groups: (non-Latino) Whites, African-Americans, and everyone else: $\beta^P = \beta^P_w s^0_w + \beta^P_b s^0_b + (1 - s^0_w - s^0_b)\,\beta^P$, and that other groups (i.e. Asians and Latinos) exhibit the average willingness-to-pay. Population shares from the Census and reported in Table 1 indicate that $s^w_0 = 0.56$ and $s^b_0 = 0.14$. As an extreme example, we let African-Americans place no value on parks $\beta^P_b = 0$ or no value on homicide reductions near parks $\beta^{PH}_b = 0$. In this scenario, White households must value parks 25% more than the estimated average to balance each term. This yields the following calculations for the willingness-to-pay of White buyers for park proximity and the park–crime interaction: $\beta^P_w = \frac{s^0_w + s^0_b}{s^0_w}\beta^P = (1.25)(0.048) = 0.06$ or $\beta^{PH}_w = \frac{s^0_w + s^0_b}{s^0_w}\beta^{PH} = (1.25)(0.015) = 0.019$. A 10.7% increase in White households would lower the observed willingness-to-pay by a maximum of $(-0.107)(0.06) = -0.006$ for β^P or $(-0.107)(0.019) = -0.002$ for $\beta^P H = 0$.

[43] Appendix Table A6 provides separate estimates for each of the cities in our sample. F-tests suggest no difference in the estimates across cities.

[44] We use the midpoint of each interval and calculate the area of the block group that is within 1/16 miles of a park to compute the proportion of housing units in each census block group affected by the premium.

[45] The coefficients without the complementarity are 0.0257 (0.0103) for within 1/16 of a mile of a park, and −0.0153(0.0027) per homicide.

Table 12
Effect of actual crime reductions on the value of park proximity.

	Neighborhood's change in homicide risk *(Billions of dollars in year 2017 values)*		
	Decrease $\Delta H < -0.01$	Increase $\Delta H > 0.01$	Net
Chicago	1.3	−0.3	1.0
New York	6.6	−0.9	5.8
Philadelphia	0.2	−0.2	0.8
Total	8.1	−1.3	6.8

Notes: Estimates of value of parks for each city are based on the number of units within 1/16 miles of a park and the median value and our estimates, as described in Table 11. Using the expected number of neighborhood homicides, we calculate yearly percent changes using a linear regression by neighborhood. We classify neighborhoods as having experienced a decrease if the average yearly reduction in homicide risk was below 1%. We classify them as having experienced an increase if the neighborhood homicide risk above 1%. All others are classified as having no change.

slightly higher since higher-value properties tend to benefit from the complementarity. Moreover, the potential value of proximity in column 3 is estimated at over \$25 billion, with more than \$10 billion "locked-in" by crime.

To put these numbers in context, we compare these values of park proximity with rough estimates of park costs. In 2017, the operating and capital expenditures on these parks were \$2.3 billion.[46] Capitalizing these values using a discount rate of 4 % (a typical mortgage rate) results in a net present cost of \$58 billion. Thus, a policy that moves all crime just a few blocks away from parks could be worthwhile if it costs a sixth of current park budgets.

A fuller analysis of park costs would also consider the opportunity cost of land. Land used for parks could instead be used for other purposes, such as residential uses. Calculations in Table A8 suggest that the land used for parks would accommodate almost \$110 billion in residential property in all three cities. If land accounts for one-third of that value (e.g. Albouy and Ehrlich, 2018), then the opportunity cost of that land is around \$36 billion. Added to the operating costs above, the total cost of parks is closer to \$94 billion. Therefore, the realized value of simply living close to a park (\$15 billion) is already about one-sixth of this value. Adding in the \$10 billion "locked-in " by crime, it could potentially be over a quarter. Note that the benefits of living close to a park may not be fully captured by changes in home values. Moreover, much if not most of the benefits of parks likely accrue to those who live over a block away from them.

As seen in Panel B, most of the value is realized in New York, as it has the highest valued real estate. Appendix Table A7 shows the value of a few specific parks: Central Park itself contributes over \$1.7 billion in value alone.

Panel C breaks down areas by their level of homicide risk. The value of parks in low-crime areas is higher when we consider complementarity, but somewhat lower in medium-crime neighborhoods, and slightly negative in high-crime areas. This produces a slightly lower overall valuation. If park areas became safe, then formerly high-crime areas would receive almost \$5 billion in park-proximity value. On the whole, more than \$10 billion of value in park proximity could be unlocked. This is roughly equal to the current realized value. Half of this value is "locked" in high-crime or low-income neighborhoods.

[46] \$0.5 billion, \$1.7 billion, and \$0.1 billion, in Chicago, New York, and Philadelphia, respectively,

Panel D breaks down neighborhoods by their median income. Its pattern is similar to that of neighborhoods organized by crime rates, with low-income neighborhoods receiving no value from park proximity and high-income areas receiving the most. Eliminating crime raises the value of park proximity the most in low-income areas, despite their lower-value homes, crime produces such a large percentage-wise reduction in the value of park proximity in those areas. Assessments that ignore crime would greatly over-state how much properties in lower-income neighborhoods benefit from being near parks.

Panel E, which breaks down values by race and ethnicity, reveals a more stark pattern. Homeowners in predominantly White areas benefit disproportionately from parks. While White households are moderately more likely to reside near parks — see Table A9 – they are much more likely to reside near safe parks. Homeowners in predominantly African-American areas, on the other hand, do not seem to benefit on average from the parks near them since they are unsafe.

Another implication of the estimates is that a considerable amount of park value has already been unlocked through the reductions in crime observed across the period of our sample. Table 12 reports these values, separating neighborhoods that became safer from those that became more dangerous. Our results show that \$7 billion, about half of the value park-proximity currently experienced, was unlocked during this period.

8. Conclusion

This study presents possibly the strongest evidence to date on the complementarity between two public goods: urban parks and public safety. Across a wide array of neighborhoods, homebuyers pay more to live near parks when they become safe. This phenomenon is illustrated by comparing homes within the same neighborhood and is robust across a range of empirical models that control for potential neighborhood-level confounds.

Our main findings also imply that while safe parks are public goods, unsafe ones can become public bads. In fact, lack of safety appears to have locked up much of the value of existing urban parks. This finding is important for policymakers and those concerned about how public goods are distributed across households. Based on principles of categorical equity, some might endorse providing equal access to open space in safe and unsafe areas alike. Yet, to those in unsafe areas, such access may provide little benefit. On the other hand, the present estimates imply that the value of reducing crime varies substantially, even within local communities. Targeted investments in public safety through park design, "hot spot policing," "safe passage" programs, or other methods could unlock considerable value simply by displacing crime. We leave it to future work to consider whether the costs of displacement make it optimal to do so. While on average such displacement appears to be beneficial, it could affect other public good complementarities that are not examined in this study. Public-good complementarities of all kinds may need to be considered for optimal investment decisions.

The results on the main effect of crime independent of park proximity are far less conclusive and are peripheral to our thesis. However, our findings do point to several directions for further research. First, the value of other forms of public capital, such as public transit, may be similarly reduced by crime. Second, the finding that crime exhibits diminishing marginal costs deserves further attention, as they imply that concentrating crime geographically may be Kaldor-Hicks efficient. Third, there may be conditions under which parks contribute to or detract from the production of crime. Unsafe areas may benefit more from additional "eyes on the street" from residents inside nearby buildings or targeted safety programs (Jacobs, 1961; McMillen et al., 2019). Open spaces may reduce such protections, particularly at night.

Appendix A. Tables and figures

Table A1
Price effects of the complementarity between park proximity and homicide risk: neighborhood spatial differences (NSD) estimates robustness to homicide risk functional form transformations.

	Dependent variable: ln Housing transaction price				
Transformation	Powers			Linear	
	$H^{1/2}\left(\overline{H^{1/2}}\right)^{-1}$	$H^{2/3}\left(\overline{H^{2/3}}\right)^{-1}$	$H^{3/4}\left(\overline{H^{3/4}}\right)^{-1}$		
	(1)	(2)	(3)	(4)	(5)
Park within 1/16 mile	0.0488***	0.0446***	0.0427***	0.0379***	0.0469***
	(0.0142)	(0.0132)	(0.0129)	(0.0121)	(0.0129)
	[3.4456]	[3.3746]	[3.3189]	[3.1308]	[3.6354]
Park 1 to 2/16 miles	0.0128	0.0115	0.0109	0.0092	0.0092
	[1.1429]	[1.0883]	[1.0571]	[0.9487]	[0.8896]
	(0.0112)	(0.0105)	(0.0103)	(0.0097)	(0.0103)
Homicide Risk	−0.0115***	−0.0088***	−0.0077***	−0.0066***	−0.0078**
	(0.0028)	(0.0021)	(0.0019)	(0.0017)	(0.0034)
	[−4.1470]	[−4.0884]	[−4.0531]	[−3.9196]	[−2.3097]
Homicide Risk squared					0.0001
					(0.0002)
					[0.3903]
Park 1/16 × Homicide Risk	−0.0119***	−0.0097***	−0.0087***	−0.0078***	−0.0200***
	(0.0041)	(0.0032)	(0.0029)	(0.0028)	(0.0055)
	[−2.8844]	[−3.0005]	[−3.0025]	[−2.8265]	[−3.6110]
Park 1 to 2/16 × Homicide Risk	−0.0057	−0.0051*	−0.0047*	−0.0050*	−0.0050
	(0.0037)	(0.0030)	(0.0027)	(0.0027)	(0.0053)
	[−1.5607]	[−1.6800]	[−1.7338]	[−1.8604]	[−0.9370]
Park 1/16 × Homicide Risk squared					0.0013***
					(0.0004)
					[3.0363]
Park 1 to 2/16 × Homicide Risk squared					−0.0000
					(0.0005)
					[−0.0032]
Neighborhood Fixed Effects	Yes	Yes	Yes	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Dwelling characteristics	Yes	Yes	Yes	Yes	Yes
Time-Var Socio-Economic Controls	Yes	Yes	Yes	Yes	Yes
Observations	656,841	656,841	656,841	656,841	656,841

Notes: Sample and variables are the same as described in Table 2, which includes transactions within 3/8 of a mile of a park for Chicago (2001–2016), New York (2006–2016), and Philadelphia (2006–2016) from Zillow (2018). Neighborhood is defined using a 3/8 miles radius around a park. Standard errors clustered at the neighborhood level are in parentheses. *t*-Statistics are in brackets.

* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

Table A2
Price effects of the complementarity between park proximity and homicide risk: neighborhood spatial differences (NSD) estimates.

	Dependent variable: ln *Housing transaction price*					
Homicide risk measure	Transformed $\left(\tilde{H} = 2\sqrt{H}\left(\sqrt{(H)}\right)^{-1}\right)$			Linear		
	No Interac.	Neigh. Fixed Effects	+ Time-Varying Controls	No Interac.	Neigh. Fixed Effects	+ Time-Varying Controls
	(1)	(2)	(3)	(4)	(5)	(6)
Park within 1/16 mile	0.0368***	0.0731***	0.0653***	0.0374***	0.0626***	0.0531***
	(0.0122)	(0.0170)	(0.0166)	(0.0122)	(0.0147)	(0.0142)
Park 1 to 2/16 miles	0.0123	0.0348**	0.0292**	0.0124	0.0297**	0.0243**
	(0.0099)	(0.0141)	(0.0134)	(0.0099)	(0.0121)	(0.0115)
Park 2 to 3/16 miles	0.0179*	0.0311**	0.0252**	0.0179*	0.0286**	0.0234**
	(0.0096)	(0.0133)	(0.0127)	(0.0096)	(0.0118)	(0.0112)
Park 4 to 5/16 miles	0.0125	0.0174	0.0184	0.0125	0.0155	0.0172
	(0.0122)	(0.0132)	(0.0126)	(0.0122)	(0.0138)	(0.0133)
Park 5 to 6/16 miles	0.0099	0.0182	0.0217	0.0102	0.0160	0.0186
	(0.0122)	(0.0158)	(0.0149)	(0.0122)	(0.0145)	(0.0139)
Homicide Risk	−0.0153***	−0.0202***	−0.0087**	−0.0092***	−0.0113***	−0.0038*
	(0.0027)	(0.0037)	(0.0035)	(0.0016)	(0.0024)	(0.0023)
Park 1/16 × Homicide Risk		−0.0147***	−0.0147***		−0.0115***	−0.0106***
		(0.0050)	(0.0049)		(0.0033)	(0.0032)
Park 1 to 2/16 × Homicide Risk		−0.0095**	−0.0085**		−0.0090***	−0.0078**
		(0.0043)	(0.0043)		(0.0030)	(0.0030)
Park 2 to 3/16 × Homicide Risk		−0.0047	−0.0037		−0.0045*	−0.0037
		(0.0039)	(0.0040)		(0.0027)	(0.0027)
Park 4 to 5/16 × Homicide Risk		−0.0035	−0.0029		−0.0033	−0.0030
		(0.0038)	(0.0038)		(0.0023)	(0.0024)
Park 5 to 6/16 × Homicide Risk		−0.0046	−0.0060		−0.0043	−0.0055*
		(0.0045)	(0.0043)		(0.0030)	(0.0028)
Neighborhood Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Dwelling characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Time-Var Socio-Economic Controls	Yes		Yes	Yes		Yes
Observations	656,841	656,841	656,841	656,841	656,841	656,841

Notes: Sample and variables are the same as described in Table 2, which includes transactions within 3/8 of a mile of a park for Chicago (2001–2016), New York (2006–2016), and Philadelphia (2006–2016) from Zillow (2018). The reference category in this specifications are properties sold between 3 and 4/16ths of a mile away. Neighborhood is defined using a 3/8 miles radius around a park. Standard errors clustered at the neighborhood level are in parentheses.

* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

Table A3
Price effects of the complementarity between park proximity and homicide risk: NSD + IV estimates (omits the first two years of sample and last year (2016)).

Estimator	*Dependent variable:* ln *Housing transaction price* Neighborhood spatial differences (NSD) NSD (1)	 NSD + IV (2)
Park within 1/16 mile	0.0461***	0.0486***
	(0.0150)	(0.0180)
Park 1 to 2/16 mile	0.0096	0.0203
	(0.0111)	(0.0132)
Homicide Risk	−0.0149***	−0.0231**
	(0.0032)	(0.0091)
Park 1/16 mile × Homicide Risk	−0.0128***	−0.0148**
	(0.0046)	(0.0066)
Park 1 to 2/16 mile × Homicide Risk	−0.0042	−0.0099*
	(0.0035)	(0.0054)
Neighborhood Fixed Effects	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes
Dwelling characteristics	Yes	Yes
Time-Var Socio-Economic Controls	Yes	Yes
Observations	491,710	491,710

Notes: Sample and variables are the same as described in Table 2, without the first two years of the sample for each city, i.e. Chicago (2003–2016), New York (2008–2016), and Philadelphia (2008–2016) from Zillow (2018). We use the first two years to construct the instrument. We drop 2016 to isolate the spike in homicides in Chicago in that year. Standard errors clustered at the neighborhood level are in parentheses.
* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

Table A4
Price effects of the complementarity between park proximity and homicide equivalent risk: NSD + IV estimates (omits the first two years of sample).

Estimator	*Dependent variable:* ln *Housing transaction price* Neighborhood spatial differences (NSD) NSD (1)	 NSD + IV (2)
Park within 1/16 mile	0.0330***	0.0307**
	(0.0127)	(0.0139)
Park 1 to 2/16 mile	0.0126	0.0168*
	(0.0094)	(0.0101)
Homicide Equiv. Risk	−0.0111***	−0.0138***
	(0.0021)	(0.0039)
Park 1/16 mile × Homicide Equiv. Risk	−0.0049**	−0.0044
	(0.0025)	(0.0032)
Park 1 to 2/16 mile × Homicide Equiv. Risk	−0.0042*	−0.0058**
	(0.0024)	(0.0029)
Neighborhood Fixed Effects	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes
Dwelling characteristics	Yes	Yes
Time-Var Socio-Economic Controls	Yes	Yes
Observations	521,945	521,945

Notes: Sample and variables are the same as described in Table 2, without the first two years of the sample for each city, i.e. Chicago (2003–2016), New York (2008–2016), and Philadelphia (2008–2016) from Zillow (2018). We use the first two years to construct the instrument. The specifications are the same as in Table 8. *Homicide Risk* is replaced by *Homicide Equivalent Risk*, which is constructed using willingness-to-pay estimates from Chalfin and McCrary (2018) to construct a unitary measure of homicide-equivalent crimes. Standard errors clustered at the neighborhood level are in parentheses.
* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

Table A5
Price effects of the complementarity between park proximity and daytime homicide risk: NSD + IV estimates (omits the first two years of sample).

Estimator	*Dependent variable:* ln *Housing transaction price*	
	Neighborhood spatial differences (NSD)	
	NSD (1)	NSD + IV (2)
Park within 1/16 mile	0.0462***	0.0571***
	(0.0140)	(0.0177)
Park 1 to 2/16 mile	0.0101	0.0215**
	(0.0100)	(0.0131)
Homicide Risk Daytime	−0.0088***	−0.0365***
	(0.0022)	(0.0103)
Park 1/16 mile × Homicide Risk Daytime	−0.0120***	−0.0195***
	(0.0036)	(0.0070)
Park 1 to 2/16 mile × Homicide Risk Daytime	−0.0040	−0.0103*
	(0.0029)	(0.0057)
Neighborhood Fixed Effects	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes
Dwelling characteristics	Yes	Yes
Time-Var Socio-Economic Controls	Yes	Yes
Observations	521,945	521,945

Notes: Sample and variables are the same as described in Table 2, without the first two years of the sample for each city, i.e. Chicago (2003–2016), New York (2008–2016), and Philadelphia (2008–2016) from Zillow (2018). We use the first two years to construct the instrument. *Homicide Risk* is replaced by *Daytime Homicide Risk* where we exclude homicides that took place at night time (8 pm to 8 am). Standard errors clustered at the neighborhood level are in parentheses.
* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

Table A6
Price effects of the complementarity between park proximity and homicide risk: NSD + IV estimates heterogeneity by city (omits the first two years of sample).

Estimator	*Dependent variable:* ln *Housing transaction price*	
	Park-Neighborhood spatial differences (NSD)	
	NSD (1)	NSD + IV (2)
Park 1/16 mile × Chicago	0.0329	0.0337
	(0.0218)	(0.0257)
Park 1/16 mile × NYC	0.0610***	0.0644**
	(0.0205)	(0.0270)
Park 1/16 mile × Philadelphia	0.0679	0.1133**
	(0.0447)	(0.0533)
Homicide Risk × Chicago	−0.0132***	−0.0086
	(0.0039)	(0.0098)
Homicide Risk × NYC	−0.0111**	−0.0365**
	(0.0056)	(0.0164)
Homicide Risk × Philadelphia	−0.0536***	−0.1105***
	(0.0142)	(0.0419)
Park 1/16 × Homicide Risk × Chicago	−0.0146**	−0.0145*
	(0.0063)	(0.0085)
Park 1/16 × Homicide Risk × NYC	−0.0129*	−0.0169
	(0.0072)	(0.0117)
Park 1/16 × Homicide Risk × Philadelphia	−0.0239	−0.0619**
	(0.0186)	(0.0285)
p-Value *F*-test equality of coefficients		
Park within 1/16 mile	0.580	0.368
Homicide Risk	0.0171	0.0262
Park within 1/16 mile × Homicide Risk	0.859	0.281
Park-Neighborhood Fixed Effects	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes
Dwelling characteristics	Yes	Yes
Time-Var Socio-Economic Controls	Yes	Yes
Observations	521,945	521,945

Notes: Sample and variables are the same as described in Table 2, without the first two years of the sample for each city, i.e. Chicago (2003–2016), New York (2008–2016), and Philadelphia (2008–2016) from Zillow (2018). We use the first two years to construct the instrument. Standard errors clustered at the neighborhood level are in parentheses.
* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

Table A7
Value of selected parks.

City	Park	Realized park value			
		No complementarity	With complementarity	No crime in parks	Locked-in by crime
(1)	(2)	(3)	(4)	(5)	(6)
New York	Central Park	1750	1758	2626	868
Chicago	Lincoln Park	366	484	549	65
New York	Prospect Park	284	161	426	265
Chicago	Grant Park	79	109	119	10
Philadelphia	Fairmount Park	94	109	141	32

Notes: Estimates of the value of parks for each city are based on the number of units within 1/16 miles of a park and the mean value from the 2013–17 ACS and our estimates from Table 2, column 3.

Table A8
Simple estimates of the opportunity cost of land in parks if park land would be residential and one-third of total value is land.

City	Housing			Area			Parks	
	Units *1000s*	Total value *$ B*	Average value *$K*	Park *1000s*	Residential *1000s*	Home val. per acre *1000s*	Home value *$B*	Land value *$B*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Chicago	471.8	$153.1	$ 324.5	9.09	131.82	$1161	$10.6	$ 3.52
New York	1025.3	$745.9	$ 727.5	21.51	171.43	$4351	$93.6	$31.19
Philadelphia	308.7	$ 62.1	$ 201.3	6.07	79.8	$ 779	$4.7	$ 1.58
Total	1805.8	$961.1		36.67	383.05		$ 108.9	$36.29

Notes: Estimates are our own calculations and data from the 2013–17 ACS. Land-share estimate of one-third of housing values is estimated in Albouy and Ehrlich (2018).

Table A9
Exposure to park proximity by race/ethnicity.

City	Proximity to park	Population shares		
		White	African-American	Latino
Chicago	$< 1/8th$ mile	0.40	0.35	0.29
Chicago	$> 1/8th$ mile	0.60	0.65	0.71
New York	$< 1/8th$ mile	0.35	0.29	0.34
New York	$> 1/8th$ mile	0.65	0.71	0.66
Philadelphia	$< 1/8th$ mile	0.19	0.13	0.13
Philadelphia	$> 1/8th$ mile	0.81	0.87	0.87

Notes: Share are calculated based on population counts from the 2010 Census.

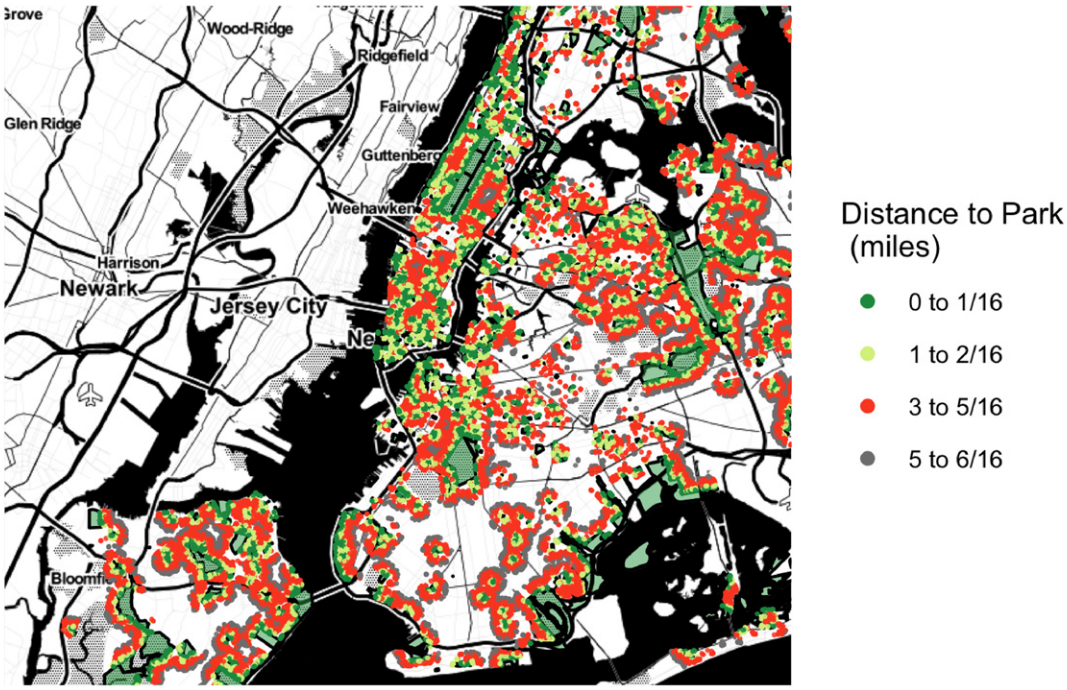


Fig. A1. Housing transactions within 3/8 miles of the nearest park, New York. Notes: Points represent transactions within 3/8 miles of the nearest park. Different shades denote proximity to the park.

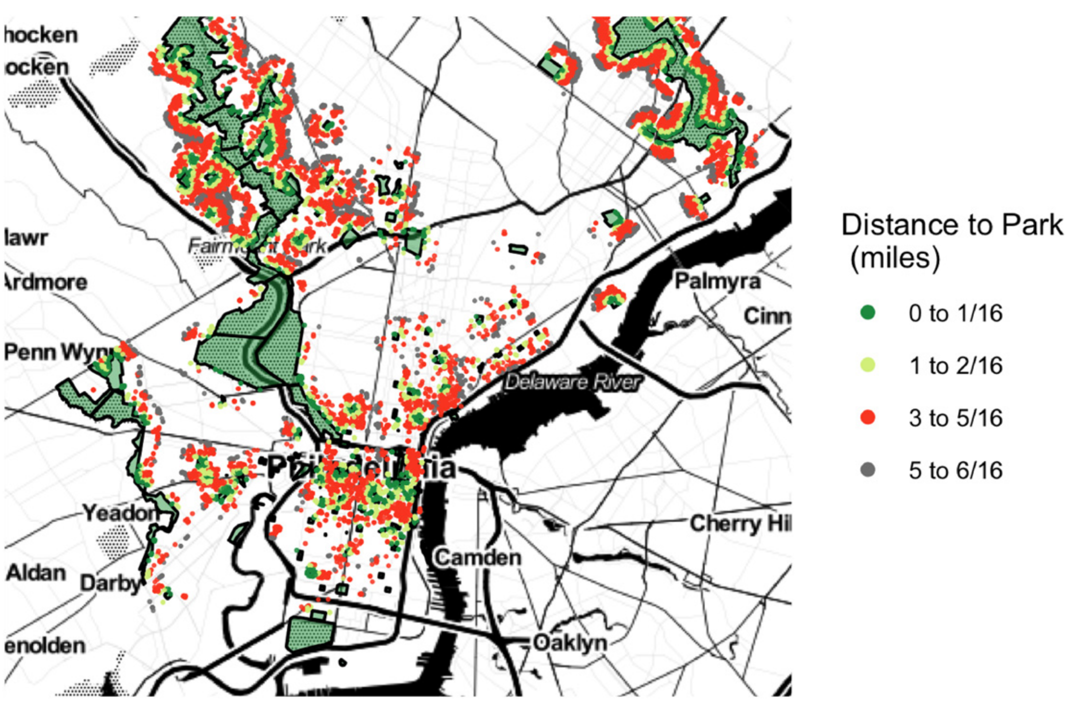
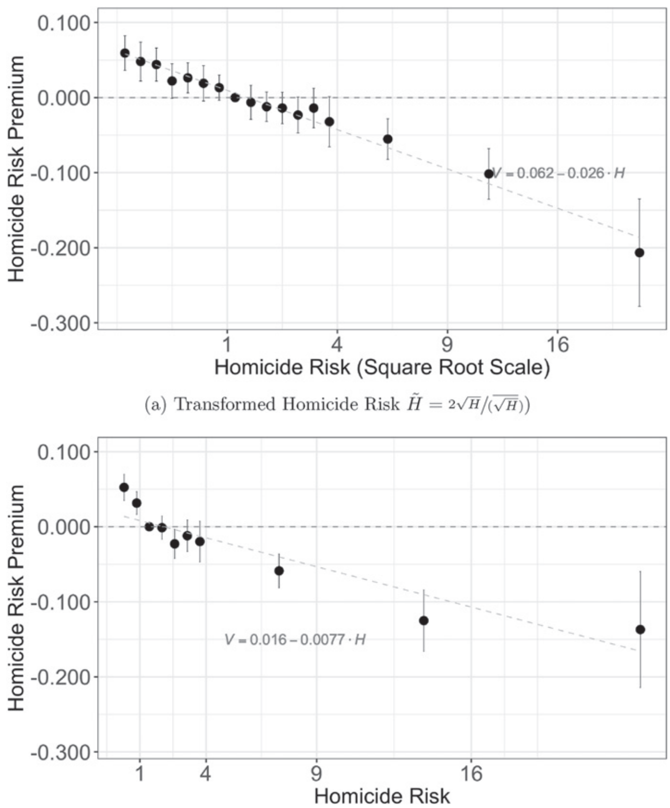


Fig. A2. Housing transactions within 3/8 miles of the nearest park, Philadelphia. Notes: Points represent transactions within 3/8 miles of the nearest park. Different shades denote proximity to the park.



(a) Transformed Homicide Risk $\tilde{H} = 2\sqrt{H}/(\overline{\sqrt{H}})$

(b) Linear Homicide Risk

Fig. A3. Prices and homicide risk. Notes: Figure shows how price varies by distance and homicide risk. Solid dots represent coefficients from this equation: $V_{ijt} = \beta^H \mathbf{H}_{ijt} + \gamma_j + \zeta_t^c + u_{ijt}$, where V_{ijt}, the sales price of house i in neighborhood j, city c, year t. $\mathbf{H}_{ijt}$ are indicators for different levels of homicide risk around property i. γ_j is the park-neighborhood fixed effect that controls for the fixed unobservables shared within a neighborhood. ζ_t^c is a fixed effect for transaction city and year to control for city-specific trends. u_{ijt} is an error term. Bars denote 95% confidence intervals. Dashed line and the equation show the fitted line on the coefficients.

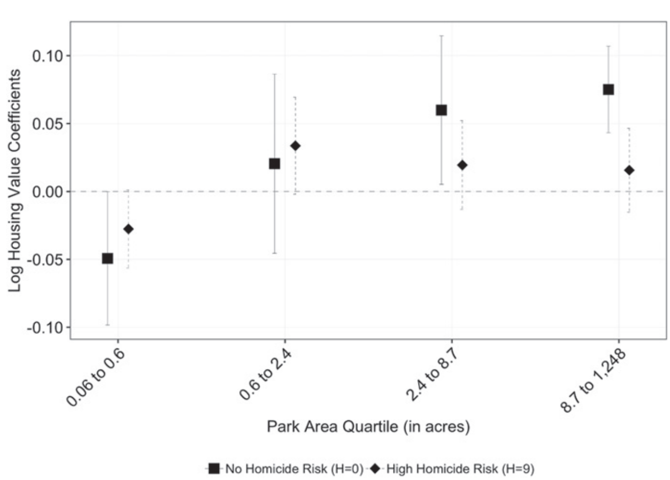


Fig. A4. Park premia by area of park and homicide risk, neighborhood spatial difference estimates. Notes: Figure shows how park proximity premium varies by quartile of the area of the park. Solid markers represent coefficients from Eq. (6) interacted with indicators for the park area quartile in which the property is closest to. *No Homicide Risk* illustrates β_k^P which is the proximity premium for properties with 0 homicides. *High Homicide Risk* illustrates $\beta_k^P + \beta_k^{PH}\tilde{H}_{ijt}^*$ where $H_{ijt}^* = 9$, i.e., the park proximity premium for properties with nine expected homicides per square mile. Bars denote 90% confidence intervals.

Appendix B. Identification in the NSD and NSD + IV frameworks

As discussed in Section 5.5, the identification of estimates presented in Table 2 is threatened if changes in the price effects of parks are correlated with unobservables. We formalize the discussion in this section by examining the effect of different possible types of omitted variables and examine the primary identification assumptions in each of our models. For simplicity, we begin with a simplified version of Eq. (5):

$$V_{ijt} = P_i\beta^P + H_{ijt}\beta^H + P_iH_{ijt}\beta^{PH} + \gamma_j + \zeta_t^c + u_{ijt} \tag{B.1}$$

where V_{ijt} is the sales price of house i in neighborhood j, city c, year t. P_i denotes properties within 1/16 miles of a park and H_{ijt} is our homicide risk measure. P_iH_{ijt} measures homicide risk near parks. γ_j is the neighborhood fixed effect,ζ_t^c is a fixed effect for transaction city and year, and u_{ijt} is an error term.

B.1. Identifying β^{PH} using neighborhood differences

We consider an augmented version of the model above following Wooldridge (2010) notation that includes omitted variables correlated only with park proximity and not with homicides (q_{ijt}), those that are correlated only with homicide risk and not with parks (ν_{ijt}), and those that are correlated only with the interaction (ϕ_{ijt}). Projecting the error term u_{ijt} that include omitted variables into the observables

$$L(u_{ijt}|P,H,PH) = \underbrace{\eta^P P_i}_{q_{itj}} + \underbrace{\eta^H H_{ijt}}_{\nu_{ijt}} + \underbrace{\eta^{PH} P_i H_{ijt}}_{\phi_{ijt}} \tag{B.2}$$

where the η^l for $l = P, H, PH$ measure the relationship between the unobservables and the observables that they affect. Plugging these into Eq. (B.1):

$$V_{ijt} = P_i(\beta^P + \eta^P) + H_{ijt}(\beta^H + \eta^H) + P_iH_{ijt}(\beta^{PH} + \eta^{PH}) + \gamma_j + \zeta_t^c + \tilde{\mu}_{ijt} \tag{B.3}$$

where the error term, $\tilde{\mu}_{ijt}$, has mean zero and is uncorrelated with each regressor.[47] If the exclusion restriction stated in Eq. (7) is valid, then $\eta^{PH} = 0$ and β^{PH} is identified.

Using a potential outcomes formulation, we can also see that if assumption (7) doesn't hold, then the interaction term (β^{PH}) contains an unobserved variable that is not differenced out:

$$E(V_{ijt}|.) = \begin{cases} Y_{00}, & \text{if } P_i = 0, H_{ijt} = 0 \\ Y_{10}, & \text{if } P_i = 1, H_{ijt} = 0 \\ Y_{0h}, & \text{if } P_i = 0, H_{ijt} = h \\ Y_{1h}, & \text{if } P_i = 1, H_{ijt} = h \end{cases}. \tag{B.4}$$

We note that H_{ij} can be a fully continuous treatment.

$$Y_{00} = \gamma_j + \zeta_t^c \tag{B.5a}$$

$$Y_{10} = \gamma_j + \zeta_t^c + \beta^P + \eta^P \tag{B.5b}$$

$$Y_{0h} = \gamma_j + \zeta_t^c + \beta^H \cdot h + \eta^H \cdot h \tag{B.5c}$$

$$Y_{1h} = \gamma_j + \zeta_t^c + \beta^P + \beta^H \cdot h + \beta^{PH} \cdot h + \eta^P + \eta^H \cdot h + \eta^{PH} \cdot h \tag{B.5d}$$

$$\beta^{PH} \cdot h = (Y_{1h} - Y_{0h}) - (Y_{10} - Y_{00}) - \eta^{PH} \cdot h \tag{B.6}$$

[47] Note: the constants get absorbed in the γ_j.

B.2. Threats to identification in the NSD model

In the paper, we examine three types of omitted variables that could be included in ϕ_{ijt} and would violate the identifying assumption in our NSD model: (1) time-varying unobservables *between* neighborhoods may differentially affect price near parks (υ_{jt}), (2) unobserved property characteristics may result in price differences near parks, even within neighborhoods (σ_i), (3) time-varying unobservables *within* neighborhoods may affect price near parks versus farther away (χ_{ijt}).

$$\phi_{ijt} = \underset{(1)}{\upsilon_{jt}} + \underset{(2)}{\sigma_i} + \underset{(3)}{\chi_{ijt}}. \tag{B.7}$$

As discussed throughout Section 5, the assumption that these terms are zero may be too restrictive in our setting. We relax them and address possible violations in a variety of ways. We address (1) υ_{jt} in Table 2 by adding neighborhood-specific time trends and even more flexibly in Table 4 with park-neighborhood-by-year fixed effects. We address (2) σ_i in Table 6 using repeat-sales estimates in the NSD design. We address (3) χ_{ijt} in Table 2 by adding time-varying socio-economic controls that are interacted with our park indicator and with homicides. We alternately relax (3) in Table 4 by introducing controls for separate neighborhood-specific trends within 1/16th mile vs 2–16ths mile from a park.

B.3. Identifying β^{PH} using IV + NSD

We go on to more comprehensively address (3) (χ_{ijt}) with an instrumental variables strategy. This IV assumes that city-level shocks to homicide rates will affect local homicide risk but are not correlated with within-neighborhood differences in price changes near parks. The second stage of the IV strategy then takes the form:

$$\begin{aligned} V_{ijt} &= \beta^P P_i + \beta^H \hat{H}_{ijt} + \beta^{PH} \widehat{P_i H_{ijt}} \\ &\quad + P_i q_{ijt} + \hat{H}_{ijt} \nu_{ijt} + \widehat{P_i H_{ijt}} \cdot \phi_{ijt} + \gamma_j + \zeta_t^c + u_{ijt}. \end{aligned} \tag{B.8}$$

In order to identify the complementarity coefficient β^{PH}, the instrument (H_{ijt}^{iv}) needs only to be orthogonal to ϕ_{ijt}. In other words, the exclusion restriction of this model requires that city-level shocks to the homicide rate affect within-neighborhood changes in housing price through effects on local homicide risk and not through other channels. If this is true, then $Cov(\hat{H}_{ijt}, \phi_{ijt}) = 0$. Importantly, identification of the complementarity (β^{PH}) in Eq. B.8 does not require that the instrument separately identifies the direct effect of crime on prices (β^H). To illustrate this point, we assume that the effect of homicides on prices are not identified by the IV: $Cov(\hat{H}_{ijt}, \nu_{ijt}) \neq 0$. If this is the case, then ν_{ijt} will be differenced out as illustrated in Eqs. (B.5) and (B.6).

B.4. Validity of the instrumental variables strategy

Although the exclusion restriction cannot be tested directly, we provide a number of indirect tests. In particular, we test whether the instrument is correlated with changes in:

(i) other public goods near parks
(ii) homicide incidence near parks
(iii) bar and restaurant establishments

We then examine robustness of estimates to:

(iv) possible endogeneity of initial risk within neighborhood
(v) controls for zone-specific park expenditures

For (i) and (ii), we estimate the following equation:

$$T_{bjt} = \tau^P \tilde{\mathbf{P}}_\mathbf{i} + \tau^H H_{bjt}^{IV} + \tau^{PH} \tilde{\mathbf{P}}_\mathbf{i} H_{bjt}^{IV} + \gamma_j + \zeta_t^c + u_{bjt} \tag{B.9}$$

where T_{bjt} measures the unemployment rate for block group b, in neighborhood j in year t. H_{lt}^{IV} is the instrument: the projected number of homicides in year t in block b, as described in Eq. (9). γ_j are park-neighborhood fixed effects and ζ_j^c are city-year effects.[48]

[48] Our measures of demographic characteristics are annualized by interpolating the matched census blocks and block groups from the 2000, 2010 census, and the 2011–15 ACS. Population and population-by-race are obtained at the block level whereas median income and median age at the block group level, which is the finest geography that they are available.

Table B.1
Effects on unemployment of the complementarity between park proximity and homicide risk: NSD + IV estimates (omits the first two years of sample).

Estimator	*Dependent variable: Unemployment rate*	
	Neighborhood spatial differences (NSD) + IV	
	(1)	(2)
Park within 1/16 mile	0.0010	0.0005
	(0.0024)	(0.0025)
Park 1 to 2/16 mile	0.0009	0.0010
	(0.0017)	(0.0017)
Homicide Risk	0.0020	0.0021
	(0.0014)	(0.0014)
Park 1/16 mile × Homicide Risk	0.0002	0.0003
	(0.0010)	(0.0010)
Park 1 to 2/16 mile × Homicide Risk	0.0007	0.0007
	(0.0007)	(0.0007)
Neighborhood Fixed Effects	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes
Time-Var Socio-Economic Controls	Yes	Yes
Neighborhood by Year FE		Yes
Observations	521,945	521,945

Notes: Sample and variables are the same as described in Table 2, without the first two years of the sample for each city, i.e. Chicago (2003–2016), New York (2008–2016), and Philadelphia (2008–2016) from Zillow (2018). Number or restaurants come from the Census Zip Codes Business Patterns. We use the first two years to construct the instrument. Standard errors clustered at the neighborhood level are in parentheses.

While limited, this test provides some evidence that the instrument does not predict changes in local labor market activity that could reflect changes in public goods provision or other amenity changes.

Next, we examine if the instruments predicts the relative incidence of homicides near parks using a similar specification. This helps to separate possible quantity effects (amount of crime) from price effects (value per crime), the focus of our hypothesis. To do so, we estimate the following equation:

$$H_{ijt} = \tilde{\mathbf{P}}_{\mathbf{i}}\vartheta^P + \vartheta^H H_t^{-jc} + \tilde{\mathbf{P}}_{\mathbf{i}} H_t^{-jc}\vartheta^{PH} + D_i\vartheta^D + X_{ijt}\vartheta^X + \gamma_j + \zeta_t^c + u_{ijt} \tag{B.10}$$

Table B.2
Differential effects of city-level homicides on local homicides.

	Dependent variable:	
	Homicide Risk	
	(1)	(2)
Park within 1/16 mile	0.0186*	0.0270**
	(0.1087)	(0.1074)
Park 1 to 2/16 miles	−0.0212	−0.0135
	(0.0793)	(0.0784)
City-Level Homicides (H_t^{-jc})	0.0028***	
	(0.0002)	
Park 1/16 × City-Level Homicides (H_t^{-jc})	−0.0004	−0.0004
	(0.0002)	(0.0002)
Park 1 to 2/16 × City-Level Homicides (H_t^{-jc})	−0.0001	−0.0001
	(0.0002)	(0.0002)
Neighborhood Fixed Effects	Yes	
City-by-Year Fixed Effects	Yes	
Neighborhood by Year FE		Yes
Time-Var Socio-Economic Controls	Yes	Yes
Observations	521,945	521,945

Notes: Sample and variables are the same as described in Table 2, without the first two years of the sample for each city, i.e. Chicago (2003–2016), New York (2008–2016), and Philadelphia (2008–2016) from Zillow (2018). We use the first two years to construct the instrument. City-Level Homicides $\left(H_t^{-jc}\right)$ are the city-level homicides that exclude the specific neighborhood homicides. Standard errors clustered at the neighborhood level are in parentheses.

* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

where H_{ijt} is the property-level homicide risk and H_t^c is the number of homicides in city c, remaining controls are those described for Eq. (6). Table B.2 reports results from two different specifications, the one described in Eq. (B.10) and a more restrictive specification where we include neighborhood-by-year fixed effects. These tests indicate that shocks to homicides at the city level predict homicides at the property level, but there is no evidence of differential effects on the incidence of homicides near parks relative to our comparison zones. This evidence suggests that changes in the city-level homicide rate affect the prices of homes near parks through the differential value of crime reduction near parks rather than through differential effects on homicide risk near parks.

Next, we examine relationship between homicide risk and bars and restaurants using data at the 5-digit zip level, normalized by area. The aggregation of publicly available data at the 5-digit zip code reduces our ability to examine within-neighborhood differences. All estimates suggest a smaller number of establishments near parks, which may reflect the smaller area within the 0–1 /16th distance bin. We find no evidence of changes in the number of neighborhood-level restaurants near parks as a function of changes in homicide risk. In Panel B, we report estimates for the sample of zips that are smaller than at least one associated park, which provide the strongest possible test by measuring changes at a more disaggregate level with variation within neighborhoods. The evidence is consistent with that provided in Panel A.

Table B.3
Effects on restaurants of the complementarity between park proximity and homicide risk: NSD + IV estimates (omits the first two years of sample).

	Dependent variable: Restaurants per Zip sq mile	
Estimator	Neighborhood spatial differences (NSD)	
	(1)	(2)
Park within 1/16 mile	−17.3606***,*	−18.2753***
	(4.9534)	(4.8835)
Park 1 to 2/16 mile	−0.6533	−1.9367
	(3.5816)	(3.5127)
Homicide Risk	−1.6579	−2.1265
	(1.1034)	(1.7661)
Park 1/16 mile × Homicide Risk	1.7744	2.3291*
	(1.3152)	(1.4092)
Park 1 to 2/16 mile × Homicide Risk	0.3121	1.0793
	(1.2183)	(1.3003)
Estimator	Neighborhood spatial differences (NSD) + IV	
Park within 1/16 mile	−17.6509***	−17.5658***
	(6.1130)	(6.0064)
Park 1 to 2/16 mile	−0.6700	−1.6085
	(4.4120)	(4.4458)
Homicide Risk	−2.0803**	−0.8826
	(4.7391)	(5.1063)
Park 1/16 mile × Homicide Risk	1.8989	2.0393
	(2.3880)	(2.4829)
Park 1 to 2/16 mile × Homicide Risk	0.3093	0.9425
	(2.1513)	(2.3033)
Wu-Hausman Test (p-Values)	0.9993	0.9875
Observations	521,945	521,945
	Restricted Sample	
Estimator	Neighborhood spatial differences (NSD)	
	(1)	(2)
Park within 1/16 mile	−29.9445***	−30.5305***
	(8.0875)	(7.7622)
Park 1 to 2/16 mile	−1.7516	−2.7216
	(6.4029)	(6.1542)
Homicide Risk	−2.9713	−3.8246
	(1.8926)	(2.7253)
Park 1/16 mile × Homicide Risk	3.2888	3.7983
	(2.3838)	(2.4562)
Park 1 to 2/16 mile × Homicide Risk	1.2585	1.9259
	(2.4262)	(2.4815)
Estimator	Neighborhood spatial differences (NSD) + IV	
Park within 1/16 mile	−59.3339***	−59.3082***
	(19.7256)	(19.8768)
Park 1 to 2/16 mile	−8.4042	−10.8307
	(16.3984)	(16.5655)
Homicide Risk	−19.4648	−18.7444
	(14.6213)	(15.6716)
Park 1/16 mile × Homicide Risk	7.1162	7.1797
	(9.2428)	(9.6769)
Park 1 to 2/16 mile × Homicide Risk	−2.1504	−0.6513
	(7.6918)	(8.3160)
Wu-Hausman Test (p-Values)	0.6527	0.7436
Observations	521,945	521,945

(*continued on next page*)

Table B.3 (*continued*)

Neighborhood Fixed Effects	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes
Time-Var Socio-Economic Controls	Yes	Yes
Neighborhood by Year FE		Yes

Notes: Explanatory variables are the same as described in Tables 2 and 8. The number of restaurants comes from Census Zip Codes Business Patterns: https://www.census.gov/data/developers/data-sets/cbp-nonemp-zbp/zbp-api.html. In the *Restricted Sample*, panel data are restricted to zip codes that are smaller than at least 1 associated park and merged with property-level data. We use the first two years of data to construct the instrument. Standard errors clustered at the neighborhood level are in parentheses.
* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

We then test the robustness of our results using a lagged version of our IV + NSD model. The instrument is constructed using the same initial shares, but 1st stage estimates are generated using a truncated dataset that drops the first 5 years of the sample. This lagged version of the IV tests the robustness of our results to the possible endogeneity of initial shares of homicide risk within neighborhoods. We note that endogeneity in the distribution of initial homicide risk between neighborhoods is addressed using flexible neighborhood-level fixed effects in the IV + NSD model. The results reported in Table B.4 indicate that our estimates are robust to the lagged version of the IV.

Table B.4
Housing prices, park proximity, and homicide risk: lagged city-level crime instrument (omits the first five years of sample).

	Dependent variable: ln *Housing transaction price*	
Estimator	Neighborhood spatial differences (NSD)	
	NSD	NSD + IV
	(1)	(2)
Park within 1/16 mile	0.0359***	0.0397**
	(0.0130)	(0.0157)
Park 1 to 2/16 mile	0.0043	0.0110
	(0.0098)	(0.0110)
Homicide Risk	−0.0086***	−0.0175*
	(0.0027)	(0.0090)
Park 1/16 mile × Homicide Risk	−0.0129***	−0.0173***
	(0.0037)	(0.0066)
Park 1 to 2/16 mile × Homicide Risk	−0.0035	−0.0094*
	(0.0029)	(0.0048)
Neighborhood Fixed Effects	Yes	Yes
City-by-Year Fixed Effects	Yes	Yes
Dwelling characteristics	Yes	Yes
Time-Var Socio-Economic Controls	Yes	Yes
Observations	328,968	328,968

Notes: Sample and variables are the same as described in Table 2, without the first five years of the sample for each city, i.e. Chicago (2006–2016), New York (2011–2016), and Philadelphia (2011–2016) from Zillow (2018). We use the first two years to construct the instrument. Standard errors clustered at the neighborhood level are in parentheses.
* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.

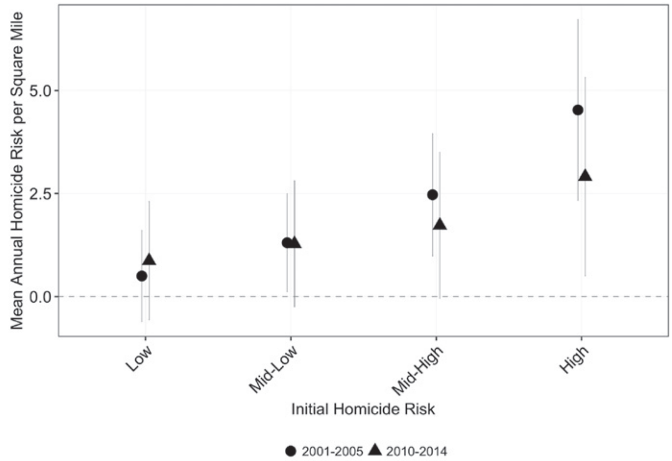
Finally, we test the robustness of our estimates to changes in park-specific expenditures across three zones in Chicago. If improvements in park equipment, facilities, programming or management occur simultaneously with crime reductions, then they could confound estimates of the complementarity. City budget appropriations data are available for the northern, central, and southern zones of Chicago for the years 2011–2017. As a result, this test involves a small subset of the total transactions in the study ($N = 21{,}752$). While less precise, base estimates of the park premium and the complementarity reported in columns 1 and 3 are consistent with the main findings of the paper. Column 2 adds controls for concurrent zone-specific park expenditures. A comparison of column 1 to column 2 indicates that estimates are not sensitive to this control. Column 3 adds (1 year) lagged expenditures. A comparison of column 3 to column 4 indicates that estimates are similarly robust to controls for lagged expenditures.

Table B.5
Housing prices, park proximity, and homicide risk: park expenditures.

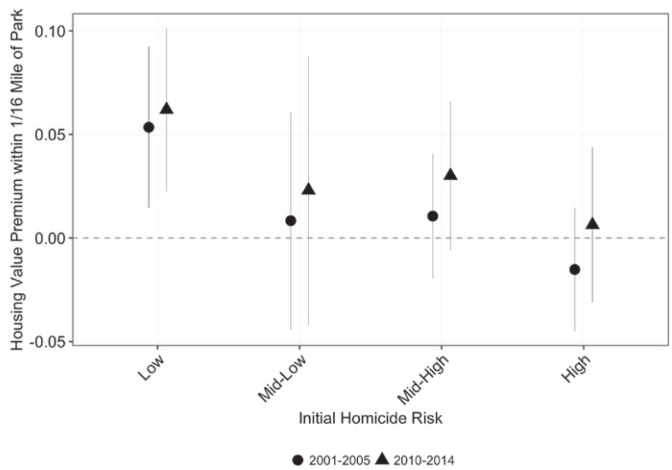
	Dependent variable: ln *Housing transaction price*			
	(1)	(2)	(3)	(4)
Estimator	Neighborhood spatial differences (NSD)			
	Chicago 2011–2016		Chicago 2012–2016	
Park within 1/16 mile	0.0418**	0.0416**	0.0398**	0.0400**
	(0.0176)	(0.0177)	(0.0179)	(0.0179)
Park 1 to 2/16 mile	0.0022	0.0022	−0.0020	−0.0020
	(0.0166)	(0.0166)	(0.0188)	(0.0188)
Homicide Risk	−0.0025	−0.0026	−0.0074	−0.0070
	(0.0067)	(0.0067)	(0.0072)	(0.0071)
Park within 1/16 mile × Homicide Risk	−0.0146**	−0.0145**	−0.0134*	−0.0135*
	(0.0074)	(0.0074)	(0.0079)	(0.0079)
Park 1 to 2/16 mile × Homicide Risk	−0.0003	−0.0002	−0.0000	0.0001
	(0.0064)	(0.0064)	(0.0073)	(0.0072)
Estimator	Neighborhood spatial differences (NSD) + IV			
Park within 1/16 mile	0.0567*	0.0580**	0.0638**	0.0656**
	(0.0293)	(0.0293)	(0.0294)	(0.0293)
Park 1 to 2/16 mile	−0.0090	−0.0088	−0.0075	−0.0065
	(0.0276)	(0.0276)	(0.0308)	(0.0306)
Homicide Risk	−0.0364	−0.0390	−0.0454***	−0.0449
	(0.0541)	(0.0547)	(0.0587)	(0.0588)
Park within 1/16 mile × Homicide Risk	−0.0257	−0.0267	−0.0312*	−0.0322*
	(0.0181)	(0.0183)	(0.0189)	(0.0189)
Park 1 to 2/16 mile × Homicide Risk	0.0054	0.0052	0.0019	0.0014
	(0.0176)	(0.0176)	(0.0199)	(0.0198)
Neighborhood Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Dwelling characteristics	Yes	Yes	Yes	Yes
Time-Var Socio-Economic Controls	Yes	Yes	Yes	Yes
Total Concurrent Expenditures	No	Yes	No	No
Total Lagged Expenditures	No	No	No	Yes
Observations	21,752	21,752	16,831	16,831

Notes: Sample and variables are same as described in Tables 2 and 8. The sample is matched to expenditure data for Chicago (2011–2016). Expenditure data were acquired and digitized using publicly available annual budget appropriations documentation for the city of Chicago: https://www.chicagoparkdistrict.com/about-us/departments/budget-and-management. They are available for parks in north, central and south regions of the City during the years 2011–2016. Standard errors clustered at the census tract level are in parentheses.

* Significant at 10% level.
** significant at 5% level.
*** significant at 1% level.



(a) Mean Homicide Risk and Initial Shares



(b) Park Premium and Initial Shares

Fig. B.1. Park proximity premium, homicide risk and initial crime shares. Note: Panel a plots estimates of mean annual homicide risk by quartile of initial homicide risk using the first four years (2001–2005) and the last four years (2010–2014) of the data panel in Chicago. Panel b plots estimates of the housing price premium (within 1/16th mile) using the same Chicago data. Standard errors are clustered at the neighborhood level.

Appendix C. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpubeco.2019.104110.

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