The acute effects of gun violence on the local housing market

EVIDENCE OF FATAL AND NON-FATAL SHOOTINGS IN NORTHERN STOCKHOLM

SAYFA BUNPUCKDEE

NASER QADIRI

Bachelor Thesis

Stockholm School of Economics

2022

The acute effects of gun violence on the local housing market: evidence of fatal and non-fatal shootings in northern Stockholm

Abstract

This study estimates the effects of gun violence on local housing markets in northern Stockholm, divided by fatal, injurious and non-harmful shootings using data on apartment sales between 2018-2021. A significant effect at the 5% level is found for non-harmful shootings, with the occurrence of such shootings increasing apartment prices by 10,9%, and a significant effect at the 10% level is found for the occurrence of fatal shootings, increasing apartment prices by 1.3%. The estimated impacts on apartment prices are inconclusive in implications for residents' crime aversion. The occurrence of gun violence during the marketing period of apartments is found to be significantly correlated with price concessions and housing liquidity in terms of time on the market.

Keywords

Gun violence, Shootings, Vulnerable areas, Housing market, Apartment sales prices

Authors

Sayfa Bunpuckdee (24456)
Naser Qadiri (24597)

Tutor

Marieke Bos, Deputy Director, Swedish House of Finance Researcher, Swedish House of Finance

Examiner

Adrien d'Avernas, Assistant Professor, Department of Finance

Acknowledgements

We would like to direct our gratitude toward Marieke Bos for guidance and valuable feedback and Hans Flink for providing data on housing prices.

Bachelor Thesis
Bachelor Program in Business and Economics
Stockholm School of Economics

1. Introduction

This study investigates the potential existence of causal effects of gun violence on surrounding housing markets in urban Sweden. Apartment sales prices are set as the dependent variable in a hedonic pricing regression, with apartment sales close to a shooting and sales a bit further away comprising the treatment and control group in a difference-in-differences methodology. Additionally, housing liquidity and price concessions are cross-sectionally studied in local housing markets of areas subjected to shootings. The subject and statistical framework of this paper are inspired and heavily based on the paper "Estimates of the Impact of Crime Risk on Property Values from Megan's Laws" by Leigh Linden and Jonah E. Rockoff (2008).

Over the past twenty years, Sweden has been subject to the largest increase in incidences of gun violence with fatal outcomes of all the European countries, with most of the incidences being related to criminal milieus in socially vulnerable areas (National Council for Crime Prevention, 2021). Gun violence entails serious incidents with common injurious or fatal outcomes that generate a lot of attention in the media and in the political debate. Gun violence affects people and especially residents of crime-dense areas by increasing the fear of being subjected to gun violence but also fear of crime in general. The terms gun violence and shooting are henceforth used to denote shootings of any outcome, fatal, injurious or non-harmful.

1.1 Purpose

Socially vulnerable areas are burdened not only by unemployment, economic marginalisation, and a lower standard of living but also by the presence of criminality and the occurrence of gun violence which sometimes results in the death or injury of individuals unrelated to the gang-related conflict that spurred the incident. We aim to investigate what consequences the occurrence of gun violence might have on residents' already marginalised socioeconomic status through effects on the housing market, for example, lowering the prices of apartments and thereby impacting residents' wealth. Additionally, apartment sales at diminished prices due to shootings can be reflective of individuals' willingness to pay to avoid gun violence and other crime, providing valuable information for politicians and authorities that can influence resource allocation to crime prevention measures. Investigating responses in housing liquidity and price concessions in response to shootings further contributes to the literature on relationships between housing liquidity, price concessions, sale prices, and characteristics of residents, along with describing additional facets of households' behaviour through their reactions to local adverse events.

1.2 Research questions

I. How do incidences of gun violence with fatal, injurious or non-harmful outcomes affect reservation prices of nearby apartments?

II. Do incidences of gun violence affect price concessions of surrounding apartments concurrently on the market?

III. Do incidences of gun violence affect housing liquidity (defined as days-on-market) of surrounding apartments concurrently on the market?

1.3 Methodology

We employ a hedonic pricing regression model for estimating the impact of shootings on apartment sales prices. We construct a cross-sectional regression of the apartment sales price gradient as a function of distance to shootings. Two separate graphs are plotted for the 100 days before and 100 days after the occurrence of the shooting. The purpose of this is to identify whether there is any specific distance where the graphs deviate from each other, thereby signalling the radius that might be useful as a cut-off point between the treatment and control group.

Apartment sales within 300 metres and between 300-800 metres of shooting are assigned to the treatment and control groups respectively. Both groups are then cross-sectionally studied regarding sales price and apartment characteristics to investigate the presence of pre-existing differences between observations in the treatment group and control group.

Similarly, to the method used in "Estimates of the impact of crime on property values, we additionally use a difference-in-differences model using the previously defined treatment and control groups but including only those apartment sales that take place between 100 days before and after a shooting, as this study focuses on the short-term effect of gun violence on the local housing market.

1.4 Main findings

Fatal shootings have a significant effect on property prices in Spånga-Kista, while no significant effect of injurious or non-harmful shootings is found. In Hässelby-Vällingby, a significant effect of non-harmful shootings is found, but no significant effects from fatal or injurious shootings. Furthermore, we have negative correlations between the occurrence of a shooting during the marketing period with both the time on the market and price concessions for apartments. Apartments that were on market for sale during the occurrence of a shooting took, on average, much longer time to sell and seller reservation prices were lowered compared to apartments that were not subject to a shooting during their marketing period.

1.5 Structure of this paper

The rest of the paper reviews specific conditions of the housing market as well as the particularities of socially vulnerable areas and the situation of violent crime and gang-related crime in Sweden with the help of documents from the Swedish Police Authority and the National Council for Crime Prevention (BRÅ), as well as criminological literature focused on Sweden. Extant literature of housing research concerning the functioning of the housing market and households' preferences for areas with different social, economic, and demographic attributes is reviewed, as is the literature studying specific relationships between the presence of crime and the housing market, specifically property values. After that, our data sets are described in section 3, which we then use in conjunction with the extant research in formulating our empirical research models presented in section 4. In section 5 we present the empirical results.

2. Literature and Theoretical Framework

Our theoretical framework builds on extant literature from different areas. Research on changes in property prices commonly uses hedonic analysis to model consumer preferences and study the effects of different factors on property values. Hedonic pricing is also extensively used in the body of literature exploring the societal costs of different types of crime, which this study leans on. Finally, studies from the criminological field together with different reports and documents from different Swedish authorities are taken into consideration to further inform the appropriate choice of empirical methods and aid in the interpretation of the results.

2.1 Hedonic pricing

Hedonic pricing assumes that goods are valued for their set of different utility-bearing attributes and decomposes observed prices of a good into different implicit, "hedonic" prices, for the various attributes of the good (Rosen 1974). Hedonic models are used in the determination of how prices vary with the set of attributes in a good (Epple 1987) and are commonly used in studies of changes in property values because of different treatments, including crime risk.

The assumption that attributes such as number of rooms, storeys, and living areas are implicitly valued into the observed price, allows for fitting individual attributes to a hedonic pricing regression model in which price is regressed against the relevant attributed, and the coefficient for each variable can be interpreted as the marginal willingness to pay for a one-unit increase. Additionally, applying the previous assumption that the price of a specific house or apartment is a function of various observable, quantifiable attributes, together with variables unobserved to the researcher, the obtained residual of the hedonic price regression can be attributed to unobserved variables (Bajari, Fruewirth, Kim & Timmins, 2012). In studying effects on property values, hedonic analysis thus allows for the possibility to control for the contribution of variation in these attributes across observations to the total price, enabling the effect of external events on prices to be observed.

Choice of living in a certain area represents not only the preferences of physical house characteristics but also location-specific amenities, such as school quality, crime and social group, which can be observed from property prices, as shown by Bayer, Ferreira and MacMillian (2007), Boustan (2012), and Black (1999) who demonstrated that one standard deviation in local school test scores increased residents' marginal willingness to pay for the property by 2.1 percent.

In addition to local amenities such as high-quality schools, and the presence of parks and greenery, disamenities such as vandalised property have been shown to play a role in property prices, either directly or through interactions with other amenities. For instance, proximity to amenities such as metro stations (Ihlanfeldt & Bowes 2001) and parks (Troy & Grove 2008), have been found to increase property prices in areas of a low crime risk, though the effect on property prices interacts with local crime risk and instead decreases property prices after certain thresholds of crime prevalence.

2.2. Impact of crime on the housing market

The impact of crime on the housing market has been studied as far back as 1976, when Thaler (1976), using hedonic pricing, found property values to be negatively correlated with property crimes. Cullen and Levitt (1996) utilised instrumental variables to demonstrate a causal relationship between crime and urban depopulation, with higher-income households being more likely to move in response to increases in crime, thus increasing the concentration of poor households and lowering property values due to decreased demand. Lynch and Rasmussen (2001) estimated the cost of violent crime for a three-person household to be 933USD per year, in Jacksonville, Florida. Gibbons (2004) found that burglaries demonstrated no effect on prices, while incidences of criminal damage such as graffiti and vandalism lowered property prices in Inner London. The authors attribute the results to residents perceiving visible criminal damage as signalling higher rates of other crimes. Bishop and Murphy (2011) calculated the willingness to pay to avoid violent crime in California, finding that the average household is willing to pay $471.86 per year to reduce the total amount of violent crime by 10%.

Several studies have studied declines in property values regarding proximity to sex offender residencies in the United States following the enactment of Megan's Law in 1996 which enforces public registries of convicted sex offenders. Sex offenders are required to register their residency to local police and notify neighbours within a small radius of 0.1 miles. In Montgomery County, Ohio, Larsen, Lowrey & Coleman (2003) found that houses within 0.1 miles of a registered sex offender on average sold for 17.4% less than comparable houses located further away, while houses between 0.1-0.2 miles and 0.2-0.3 miles sold for 10.2% and 9.3% less, respectively. Pope (2008) found that house prices were reduced by 2.3% after a sex offender moves in within one-tenth of a mile (161 metres) in Hillsborough County, Florida. Additionally, Pope found that prices recovered shortly following the departure of an offender from the area, supporting the notion of a causal relationship. This negative effect is corroborated in a paper by Caudill, Affuso and Yang (2015) that investigates the relationship between house prices and the concentration of offender residencies in addition to their distance to sold properties. The results demonstrate a value loss of 14% for houses within 0.1 miles of a registered sex offender and 7% for houses within a one-mile radius. While the concentration of sex offenders is likely to increase with decreased distance to an offender, the authors find that the concentration effect is larger than the distance effect.

The replicated paper by Linden and E. Rockoff (2008) exploits the above-mentioned legislation combined with data regarding sex offender residencies and surrounding property sales to study the impact of sex offender residencies on property values in Mecklenburg County, North Carolina, USA. Using a difference-in-differences approach to manage the endogeneity problem of crime (in this case presence of sex offender residency) being correlated with other variables that lower property values (e.g., sex offenders tending to move into areas with lower quality, lower-priced housing). Using house sales within 0.1 miles of a residency that a sex offender moves into comprising the treatment group and house sales between 0.1-0.3 miles in the control group, the authors found that homeowners within the treatment zone sell their houses for $5500 less relative to what the amount they could have received had the offender not moved in. Likewise, Wentland, Waller and Brastow (2013) study the same relationship in Virginia, USA, but with the addition of studying the effects of proximity to sex offender residencies on housing liquidity. The authors address the issue present in previous studies of sale prices being observable only for houses that sell, while homes that are marketed but fail to sell are omitted from the sample which causes selection bias (ibid). Research has been conducted also in the Swedish setting, which investigates the effect of crime on the housing market. Ceccato and Wilhelmsson (2011) find that apartment prices were negatively affected by crime levels at the specific location but also by crime levels in the surrounding areas. Of the studied crime types, residential burglaries were found to have the largest negative impact on apartment prices using a data set of 9000 apartments in Stockholm, Sweden. Additional studies by the same authors have demonstrated vandalism and fear of crime in neighbourhoods affect house prices (Ceccato and Wilhelmsson, 2012), as have crime hotspots (Ceccato and Wilhelmsson, 2020)

Another potential variable of interest is the housing liquidity defined as time on the market or days-on-market. Larsen et al. (2003) emphasise the need for investigating liquidity when studying changes in sale prices, arguing that added time on the market lowers the present value of the selling price. If a substantial proportion of sellers wait long enough for an undiscounted offer, then no effect on selling prices will be detected in response to crime risk, even though sellers face consequences in the form of a long time on the market for the property.

Wentland et. al., (2013) argue for the importance of studying the effect on liquidity because of the possible consequences for seller holding costs and reservation prices since crime risk might reduce the pool of potential offers and thus lengthen the marketing period and eventually influence the final selling price. In addition, selling prices and days-on-market are argued to be jointly determined, thus requiring a model that encompasses this to not induce biased results (ibid).

The relationship between the selling price and liquidity is not clear cut, with various studies producing different, sometimes contradictory results. Asabere and Huffman (1993) find evidence of a positive relationship between days-on-market and sales price, attributing this to the increased probability of realising a higher selling price as the property stays on the market longer. Though, properties that are priced too high but fail to sell early face the risk of becoming stigmatised and lingering on the market unless the price is lowered. Angling, Rutherford and Springer (2003) find that higher property initial list prices increase days-on-market, with this effect being greater among those types of property with little variation in list prices.

Glower, Haurin and Henderschott (1998) explore the extent to which seller motivation influences days-on-market and list prices using survival analysis, defining the degree of seller motivation as supposed urgency to sell quickly, based on the seller's moving date and date of new employment elsewhere. They find evidence that motivated sellers sell their houses more quickly than unmotivated sellers but fail to confirm whether this is due to motivated sellers reducing days-on-market through setting lower initial list prices.

In addition to studying changes in price, we explore the relationship between gun violence and housing liquidity defined as days-on-market for apartments that are exposed to a nearby shooting during their marketing period, compared to apartments that do not.

2.3 Crime perception and crime occurrence in Sweden

2.3.1 Socially vulnerable areas

"Socially vulnerable areas" is a term the Swedish police authority uses to classify areas with low social and economic status that are also affected by criminality to the point that residents feel unsafe and are less willing to collaborate in legal trials. Residents are directly affected through threats or extortion, or indirectly by local criminals displaying violence and openly selling drugs.

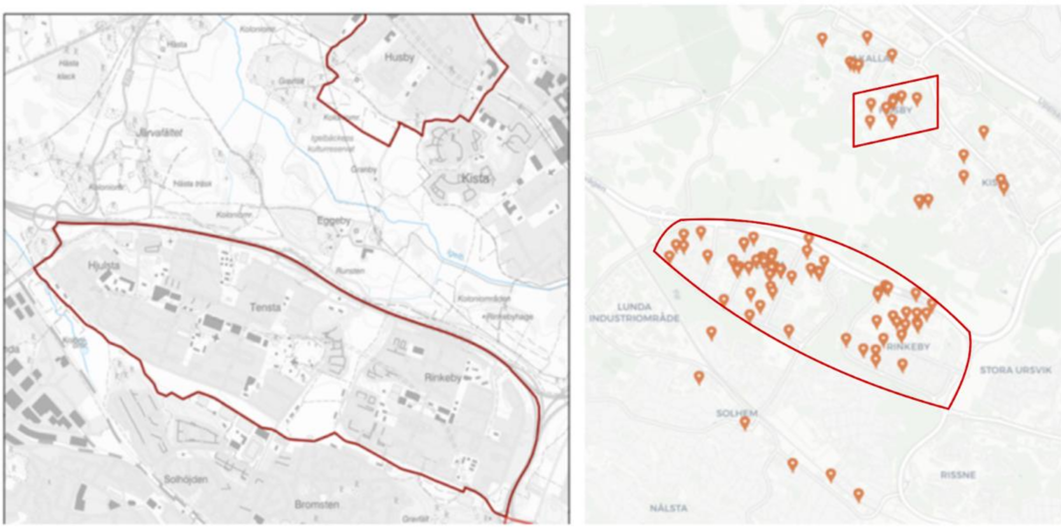
The Swedish police have in total three categories of socially vulnerable areas, ordered by the severity of problems "vulnerable areas ", "risk areas" and finally "especially socially vulnerable areas". *Risk areas* denote vulnerable areas that are at risk of developing into *Especially socially vulnerable*, the latter of which denoting vulnerable areas with more prominent problems of criminality, political or religious extremism and elements of parallel societies. These areas have increased in number over the past years, and as of 2021, the Swedish Police list 61 socially vulnerable areas of any category in Sweden (Police authority, NOA 2021).

Table 1. Number of socially vulnerable areas in Sweden in the last 5 years, Police authority, NOA (2021)

	2014	**2015**	**2017**	**2019**	**2021**
Number of vulnerable ares	55	53	61	60	61

In the districts Spånga-Tensta and Rinkeby-Kista located in the north-western parts of Stockholm municipality, there have over the past four years (2018-2021) been fourteen confirmed shootings with fatal outcomes and thirty-six non-fatal shootings with one or more people being injured. (Sveriges television) These two districts contain two of the areas classified as especially socially vulnerable, Rinkeby-Tensta and Husby (Figure 1). Which neighbourhoods to include and the geographical boundaries of these especially socially vulnerable areas are decided by the police authority. The especially socially vulnerable area Rinkeby-Tensta includes not only the neighbourhoods Rinkeby and Tensta but also Hjulsta.

Figure 1: Fatal and non-fatal Shootings in Rinkeby-Tensta and Husby 2018-2021



Note: Boundaries of some of the especially socially vulnerable areas drawn by the police authority (the left map, (Swedish Police Authority 2021)) and the occurrence of shootings in these areas (the right map) in 2018 -2021.

Table 2. Number of shootings and its proportion in the country (Police Authority, NOA 2021)

Year	2018		2019		2020	
	Number	%	Number	%	Number	%
Vulnerable areas	119	38.9	96	28.8	130	39.7

Note: For instance, in 2018, there were 119 shootings in socially vulnerable areas. These shootings accounted for 38.9% of total shootings in the country.

2.3.2 Crime occurrence and fear of crime

A report by the Swedish national council for crime prevention illustrates differences in levels of lethal violence between Sweden and other European countries. In most West European countries, lethal violence has been declining since the 1990s. However, this declining trend has been followed by an increase in lethal violence in Sweden since 2013 which stands out from other European countries. While levels of most sources of lethal violence have been declining, the increasing levels of lethal violence can be attributed to the increases in gun violence since the early 2000s. Moreover, this increase is specifically related to criminal settings, with lethal gun violence in Sweden being related to criminal settings in eight out of ten cases as of 2021, as compared to three out of ten cases in the early 2000s (National Council for Crime Prevention, 2021).

The annual report of perceived safety by the national council for crime prevention, using survey data, shows that eighty percent of the population between sixteen to eighty-four years of age believe that crime has increased by any or by a large margin over the past three years. This proportion has since 2015 remained constant, however. Forty-five percent of the population worry about crime in society to a large extent. Thirty-five percent of people often or very often worry about themselves or a relative being victimised. However, the actual rates of victimisation do not show any definite increase, ranging between 20.8%, 22.4%, 23.1%, 22.6%, and 20.2% over the years 2016 to 2020. (National Council for Crime Prevention, 2021:11). Additionally, Stockholm municipality has its own dedicated survey on perceived unsafety and crime victimisation. On the question of whether residents have been worried about being subjected to crime of any kind during the last 12 months, 14% of respondents in Hässelby-Vällingby, 13% in Spånga-Tensta and 11% in Rinkeby-Kista answered “yes”. 9% of all respondents in Stockholm municipality answered “yes” to the same question (Stockholm municipality, 2020). Evidently, fear of crime is not irrelevant in contemporary Sweden, and this provides further motivation for the study of perceptions and reactions to gun violence.

2.4 Contribution

The study contributes by expanding the knowledge of what preferences of households and what matters for individuals’ choice of residency. We explore the impact of local incidences of violent crime on the local housing market's behaviour in terms of prices and liquidity, providing insight into how to study and model these variables when researching housing markets in similarly disadvantaged areas in Sweden and elsewhere. With previous research having explored the influence of seller motivation (Glower et.al., 1998) and perceived crime risk due to nearby sex offender residencies (Wentland et.al., 2013) on housing liquidity, there is an opportunity to examine the effects on housing liquidity incidences of violent crime such as gun violence.

Moreover, as in the replicated article by Linden and E. Rockoff (2008), the results provide information useful in computing individuals' estimated willingness to pay to avoid crime. While surveys show that inhabitants in Sweden and especially socially vulnerable areas experience unsafety, this study functions to provide quantitative, monetary estimates of how large sums of public spending on crime prevention inhabitants might tolerate, in Sweden and elsewhere.

Lastly, evidence of changes to property values and housing liquidity in terms of days-on-market supports the picture provided by the criminological research of Swedish cities regarding the impact of crime on surroundings. Any disruptions in the local housing market that are evoked by incidences of gun violence place further distress on residents. In addition to providing the previously mentioned indications of appropriate public spending amounts, the study broadens the understanding of the impacts of crime on socially vulnerable areas, which is very important in Sweden.

3. Data description

3.1 Apartment sales

We initially intended to retrieve data for apartment sales within Stockholm County, which would include approximately 70 000 observations. The rationale for this was to be able to study the effects within and between different socially vulnerable areas, of which there are several in Stockholm, as well as compare the effects on socially vulnerable or in other ways disadvantaged areas with effects in wealthier areas such as the inner city. Due to the relative scarcity of gun violence incidence in such areas, a large dataset would be important to accomplish a large enough treatment group. This was however impossible in practice due to funding issues. Luckily, we were allowed free access to up to 5000 objects of apartment sales by Svensk Mäklarstatistik. We requested and were granted data for the districts of Spånga-Kista and Hässelby-Vällingby, both of which contain socially vulnerable areas of interest to our study and neighbourhoods built as part of the Swedish Million programme, containing large amounts of housing with distinctively similar characteristics between both apartments and neighbourhoods. The limited dataset decreases the power of any observed effects. However, we still find it valuable to contribute and build upon previous research in a new setting.

Data on apartment transactions consists of 6085 sales between 2018 and 2021 in Hässelby-Vällingby (3040 observation) and Spånga-Kista (3045 observation) with the following associated apartment characteristics: Initial ad asking price, contract price, price per square metre, contract date, living area, number of rooms, monthly fee, build year, latitude and longitude for all transactions. Approximately 95% of all housing sales that go through real estate agents are reported to Svensk Mäklarstatistik (Mäklarstatistik.se). The data on property sales cover the area within the red line (see below) which is in the northern part of the Stockholm Municipality.

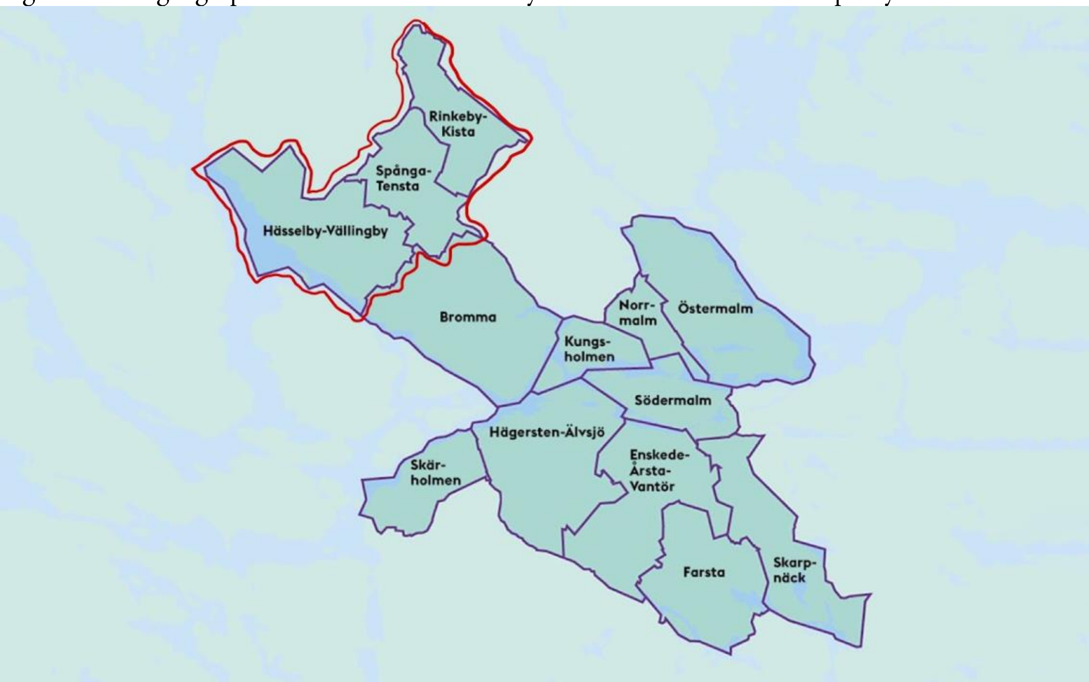
3.2 Shootings

Data on gun violence consist of 109 observations from the same area over the same years with the following information: date, type of shooting, latitude and longitude for every shooting. Request for exact coordinates from Polisen NJU was denied due to confidentiality. Hence, we collected coordinates from the SVT map for 2019-2021. For 2018, we had to use maps from both SVT and DN to manually pinpoint the coordinates in google-maps, which lessens the accuracy further. The greatest challenge here is nevertheless the possibility that the news outlets have purposefully obscured the exact locations to not reveal sensitive information. A full list of shootings can be found in the appendices.

Table 3. Shootings, 2018-2021

Spånga-Kista	Freq.	Percent	Cum.	*Hässelby-Vällingby*	Freq.	Percent	Cum.
Fatal	16	18.18	18.18	Fatal	7	33.33	33.33
Injurious	33	37.50	55.68	Injurious	8	38.10	71.43
Non-harmful	39	44.32	100.0	Non-harmful	6	28.57	100.00
Total	88	100.00		Total	21	100.00	

Figure 2. The geographical location of this study within Stockholm municipality



Note: 109 shootings have been used to analyse the acute effect of gun violence on 6085 observations of apartment sales in 2018-2021.

3.3 variables

Having the longitude and latitude of each apartment and each shooting, we can make a pairwise combination of every sale with every shooting using STATA. We upload the data on apartment sales and then use the cross command to upload data on shooting. To find the distances between each of the paired-up data, we use the geodist command: geodist aprmt_lat aprmt_lon shoot_lat shoot_lon, distance. This command generated the distance between each paired combination in a new variable.

3.3.1 Dependent variables

The following variables are used as dependent variables: *Log of sale price, each characteristic of the apartment, days-on-market* and *the difference between the sale price and asking price*. The logarithmic form of sales price changes the case from a unit change to percent changes. In the cross-sectional difference estimator, we use each characteristic of the apartment to measure the difference in characteristics of apartments in the treatment group compared to the control group. The variable *days-on-market* is obtained simply by taking the difference between the contract date and the ad publication date.

3.3.2 Independent variables

We control for apartment characteristics which consist of the attributes mentioned above. We have created dummies, *post* which takes the value of 1 if the apartment sale has occurred after a shooting, *treat* which takes the value of 1 if the apartment is in the treatment group and *post_treat* which takes the value of 1 if the apartment sale has occurred after a shooting and is in the treatment group. Furthermore, we created three other variables. One for apartments that were put on sale before the shooting but that sold after the shooting, one for apartments that were put on sale before but sold after and one for apartments that were put on sale after and sold after. For the placebo-controlled study, the following dummy variables were created: *false_post* and *treat_false_post.*

4. Empirical Methodology

Mäklarstatistik.se states that approximately 95% of sales are reported to them, which produces a selection effect if the tendency of realtor agents to report a sale depends on the characteristics of the sold property. In addition, our data set provides data only for the apartments that have been put up for sale, giving rise to a selection bias, with apartments included in the sample potentially not being representative of the apartments within the studied areas in general, as the treatment effect on houses that are marketed but fail to sell cannot be estimated. Wentland et al. (2013) have approached this issue using data containing information about both sold and unsold homes and additional statistical methods. Moreover, households that sell their properties might have different properties and be more risk-averse than the general population, as noted in the replicated paper by Linden and Rockoff (2008). Applying analogous reasoning to our study setting, risk-averse households might be overrepresented amongst households that move in conjunction with a shooting. Such households might additionally be willing to pay more to avoid crime and set lower reservation prices, leading to overestimations of the average willingness. Conversely, households that are willing to buy the apartments after a shooting might be less risk-averse and thus not demand as low discount prices, which would increase observed sale prices.

Control variables are necessary to reduce omitted variable bias. Potential issues regarding omitted variables are the failure to control for other crimes and variables that may affect property values, such as crime levels in the surrounding area as the occurrence of other types of crime is common in socially vulnerable areas (Police authority, NOA 2021). Additionally, apartment prices are likely to be influenced by proximity and connection to (dis)amenities that we have not included in the regressions.

4.1 Method

We follow the methodology of the article Estimates of the impact of crime on property values by Linden and E. Rockoff (2008) in which they used specific data consisting of locations of sex offender residencies to compare the values of home sales within small areas in which the housing stock was homogenous. In addition, we estimate the effects of gun violence on housing liquidity defined as days-on-market or time on the market as well as price concessions, within the studied areas.

4.1.1 Hedonic pricing model of apartment sales prices.

We employ a hedonic pricing model to estimate the impact of shootings on apartment sales prices. The dependent variable in our study is the log of apartment sales price, which is regressed against the set of apartment characteristics provided to us for each apartment by Mäklarstatistik.se. We log transform the sales price in the hedonic pricing regression, as done in the original paper by Linden and E. Rockoff (2008), and as is commonly done when the relationship between the sales prices and the set of attributes is presumed to be non-linear (Troy & Grove 2008). The coefficients for our control variables can thus be interpreted as the approximate marginal percentual change in sale price following a one-unit change in the quantity of the apartment attribute.

A caveat to this approach lies in the difficulty of identifying a correct hedonic function when amenities and disamenities may be correlated with unobservable variables. (Epple 1987) Provided that no relevant variables are omitted, and the characteristics included in the model accurately reflect the implicit, or hedonic prices, we can thus separate the effect of the treatment from changes in sales pricing resulting merely from differences in characteristics of sold apartments.

An overview of the average characteristics of apartments in the studied areas is presented further below in Table 4. Characteristics Apartments Sold, 2018 -2021, for all the characteristics used in the hedonic price regression model.

4.1.2 Treatment and control group

We use coordinates of shootings retrieved from SVT and DN to compare the values of home sales within small, relatively homogenous areas. Incidences of gun violence in the form of fatal, injurious and non-harmful shootings between 2018-2021 serve as the treatment.

When studying the impact of crime on the housing market, there is always the problem of endogeneity to be dealt with, in terms of higher rates of crime being correlated with other variables that decrease house prices. Wilhelmson et al. (2021) suggest that the exact time and location of shootings can be treated as random events despite not comprising a completely random phenomenon. As research has found fatal gun violence to be mainly related to criminal settings in socially vulnerable areas (National Council for Crime Prevention 2021), incidences of gun violence cannot be said to be evenly or randomly spread across different areas since they tend to concentrate in socially vulnerable or disadvantaged neighbourhoods. These areas are more likely to contain disamenities that other areas might not, which may negatively affect property prices. The non-random distribution of shootings hence gives rise to an endogeneity problem, as the treatment is correlated with attributes, some of which are unobservable, that by themselves affect the dependent variable.

If we were to simply compare areas with and without shootings it would require separating the effects of shootings from other crimes, in addition to the added difficulties of constructing a hedonic model that accurately reflects the implicit prices for two heterogeneous areas. Gerell, Sturup, Magnusson, Nilvall, Khoshnoodet & Rostami, (2021) also found that the probability of follow-up shootings is higher in socially vulnerable areas, especially in conjunction with open-drug markets within those areas, while open drug markets in wealthy areas are not correlated with follow-up shootings at all.

A large proportion of shootings in the country occur in a very small part of the country's geographical area. In 2020, 39.7% of all shootings occurred within the 60 socially vulnerable areas, which constituted only 0.02% of the country's total area. (Police authority, NOA 2021). Additionally, gun violence in Sweden is linked to the criminal settings connected to socially vulnerable areas (National Council for Crime Prevention, 2021:8) As for our data set alone, we see that 16 out of 44 fatal shootings and 88 of the 213 (SVT) total shootings that have occurred since 2018 in Stockholm municipality, took place in Spånga-Kista.

With most shootings taking place in neighbourhoods classified as socially vulnerable areas and their vicinity, there is a challenge in identifying the effect of shootings on property values using traditional methods. Ideally, there would be a treatment group and control group consisting of randomly assigned, comparable observations. However, with shootings being concentrated in socially vulnerable areas, it would be difficult to identify the effect of shootings on property values by comparing socially vulnerable areas to other unaffected areas, as the two groups would differ in many other ways than just being subjected to the treatment. Even when controlling for several apartment characteristics, there could still be other factors that impact apartment sales prices within the areas.

The districts of Spånga-Kista and Hässelby-Vällingby are divided, and the statistical analyses are conducted separately for both districts. As previously mentioned, the validity of our results is dependent on the similarity between the control and treatment groups but also the similarity of apartments within respective groups. Both the districts Spånga-Kista and Hässelby-Vällingby are situated in the north-western parts of Stockholm municipality, each containing socially vulnerable areas. However, as crime levels have been shown to interact with local amenities (Troy & Grove, 2008), (Ihlanfeldt

& Bowes, 2001), the two districts are split up. We assume that amenities vary less within the districts than between the districts, which if true, gives further accuracy to the hedonic price estimations. Also, if amenities that interact with a crime are more different between districts than within districts, then the treatment effects are more easily discerned.

Table 4 shows that apartments in areas in which shootings occur tend to have lower sales prices, on average. For instance, an apartment closer to the shooting location sold for 175855 SEK less, on average, compared to the whole area in Spånga-Kista. This raises the question of whether shootings are more likely to occur within small areas with radiuses lower than 800 metres, that are lower priced than the surroundings even within socially vulnerable areas and districts, or if a higher incidence of shootings might have discounted these small areas over time. No conclusions can be drawn simply from these descriptive statistics, however.

Table 4. Characteristics of Apartments Sold, 2018 -2021

	All		Within 800 metres of shooting			
	Spånga-Kista	Hässelby Vällingby	Spånga-Kista		Hässelby Vällingby	
	Mean *(SD)*	Mean *(SD)*	Mean *(SD)*	Marginal effect in price regression[1]	Mean *(SD)*	Marginal effect in price regression
Sale price	2202698.9	2423700.4	2026843.6		2376697.4	
	(608027.53)	(641658.11)	(529499.85)		(619562.3)	
Asking price	2108640.7	2272317.6	1950642.6		2224478.9	
	(605186.93)	(616710.77)	(534062.77)		(597368.13)	
Price/sqm	34453.676	41621.514	31605.606		40121.588	
	(9001.082)	(10034.181)	(9400.749)		(9820.631)	
Days on market	71.979	77.632	70.117		60.023	
	(818.556)	(842.776)	(740.85)		(598.953)	
Sale –Ask (in price)	93139.732	145666.28	75744.337		148245.89	
	(151926.58)	(182022.05)	(136144.16)		(185103.85)	
Build year	1984.403	1989.812	1982.086	0.00395***	1986.514	0.00181***
	(18.697)	(28.27)	(17.868)	(0.000506)	(27.782)	(0.000106)
Building storeys	6.692	6.745	6.513	0.00264*	6.89	0.000898
	(7.564)	(4.249)	(6.98)	(0.000844)	(4.192)	(0.00118)
Aprmt floor	3.449	3.821	3.334	0.00681**	3.838	0.00607**
	(4.431)	(4.383)	(4.071)	(0.00142)	(4.352)	(0.00122)
Living area	67.497	61.543	68.647	0.0109***	62.451	0.0131***
	(21.13)	(20.43)	(21.256)	(0.000652)	(19.914)	(0.000469)
Rooms	2.651	2.434	2.687	0.0672**	2.487	0.0646***
	(.978)	(.98)	(.987)	(0.0116)	(.968)	(0.00698)
Monthly fee	4078.173	3578.195	4257.08	-0.000101***	3724.078	-0.000139***
	(1221.436)	(1177.868)	(1298.556)	(5.13e-06)	(1146.029)	(3.73e-06)

Note: [1] Estimated for apartment sold within 800m of shooting by regression log(sale price) on listed variables by year fixed effects. E.g., one extra square metre in living area increases the price by 1.09% in Spånga-Kista. (Significance: *10 percent level, **5 percent level, ***1 percent level)

Figure 3A. The surrounding area within 300m and 800m of a shooting in Spånga-Kista (2020-12-29, Fatal), (Mapsdirections.info)

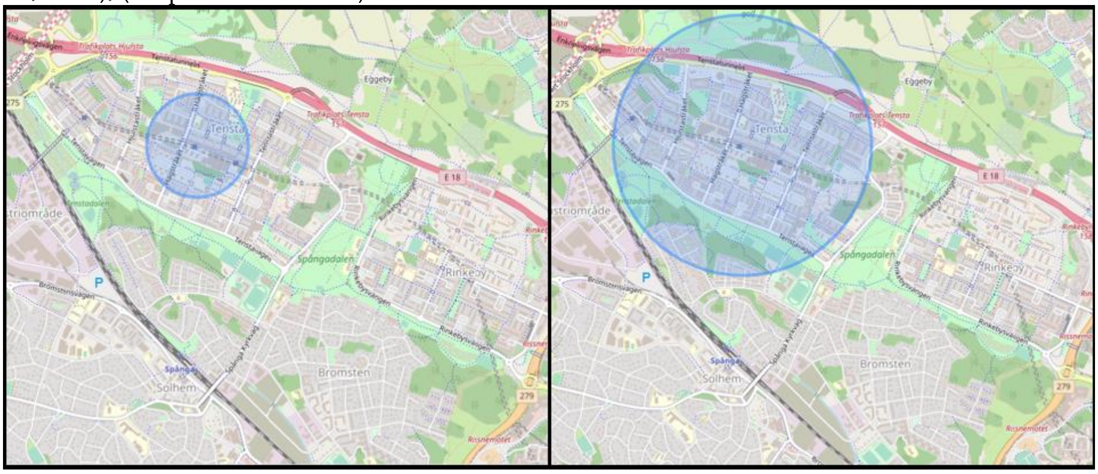


Figure 3B. The surrounding area within 300m and 800m of a shooting in Hässelby-Vällingby (2020-03-01, Injurious), (Mapsdirections.info)

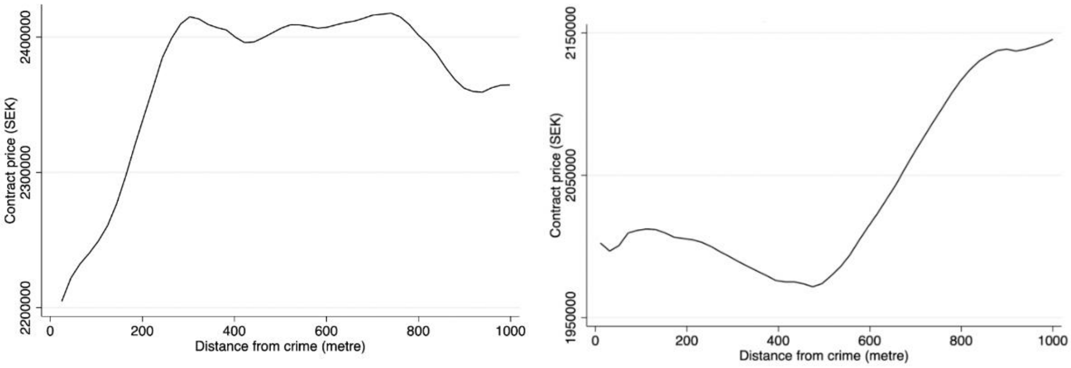
Using a long-time span for the statistical analysis entails several problems. In the article by Linden and Rockoff, a time span of two years prior and post is used. However, the nature of crimes studied in their article and our thesis differ. Individuals cannot be expected to have the same perception of living near a sex offender as discrete events of shootings. A singular incident or even a fatal shooting is not comparable to the constant proximity to an individual that households perceive as prone to commit a crime. The effects of the shooting are unlikely to stand out over a whole year. Because of this and the concentration of shootings within certain areas, we set a time limit of 100 days prior to the shooting and 100 days after the shooting, to prevent the treatment group from being subjected to repeated treatments within the studied period. If we included the whole four years of data, we have access to, there is a possibility that some observations might be within 300 metres of several shootings but setting a time limit reduces the probability of this. If observations within the treatment group are subjected to repeated treatments within the studied period, this would overestimate the effect within our model. The time limit of 100 days is set regarding the average days on market for apartment sales. In addition, Wilhelmson, Ceccato & Gerell (2021) found that shootings influenced the local prices of apartments over 100 to 200 days. The length of the limit of 100 days is thus also more likely to capture all the hypothesised effects.

While we have already established that shootings are more frequent in certain neighbourhoods, especially those in socially vulnerable areas, there is the possibility that shootings occur frequently also in small areas of similar size to our treatment group. This raises the question of whether the 100-day limits might not be sufficient to exclude the possibility of repeated treatments. Researchers have studied shootings in several large Swedish cities over the years 2011-2015 and identified the presence of near-repeat patterns of shootings, in other words being clustered in time and space. This entails that shooting in a location is likely to shortly thereafter be followed by another shooting relatively in the proximity of the location of the previous shooting. (Sturup, Rostami, Gerell & Sandholm, 2017). However, the researchers point out that even though the risk of a new shooting increases in the aftermath of an actualized shooting, the absolute risks remain low. For good measure, we have studied the data and found that most fatal shootings in our sample are not followed by another fatal shooting within 100 days and 600 metres, although injurious and non-harmful shootings are commonly closely followed by shootings of any category. The 600 metres of distance is set as the point to which the treatment groups of two spatially separate shootings begin to intersect. For Spånga-Kista, only three fatal shootings are followed by another fatal shooting within 100 days and 600 metres.

4.2 Graphical Indications

Prior to conducting our main analysis, we examine whether graphical evidence of apartment sales regressed on distance provides support for our assignment of observations to treatment and control groups. First, we explore if apartments close to a shooting are sold less than apartments further away. Figure 4A shows the price gradient of distance to shootings coordinates during the first 100 days after shooting. The prices in Hässelby-Vällingby are lowest for apartments closest to the shootings, rise with distance until reaching apartments about 300 metres away and then flatten out. In Spånga-Kista, the prices decline with distance until reaching about 500 metres before it rises sharply.

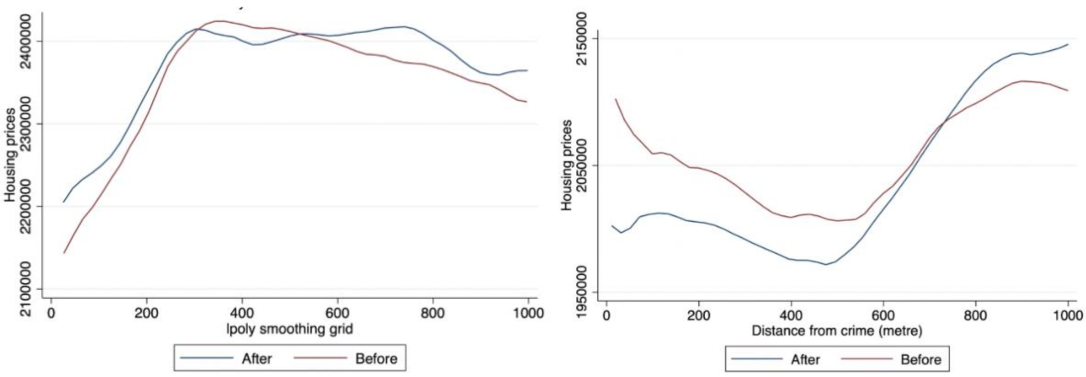
Figure 4A. Price gradient of distance from shooting, first 100 days after shooting



Note: Local polynomial regression (bandwidth=100 metres) of the sale price on distance from shooting. (The left graph refers to sales in Hässelby-Vällingby. The right graph refers to sales in Spånga-Kista)

In figure 4B we include the price gradient of distance to shootings coordinates 100 days before the shootings occurred. The price gradient in Hässelby-Vällingby is largely similar in all distances before and after the occurrence of the shooting. In Spånga-Kista apartments closest to the shooting had higher prices before than after but otherwise followed the same trend.

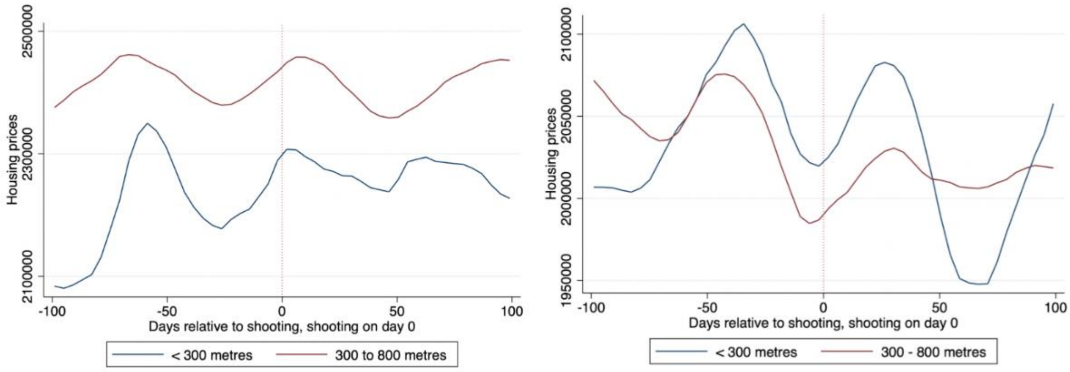
Figure 4B. Price gradient of distance from shooting, 100 days before and 100 days after a shooting



Note: Local polynomial regression (bandwidth=100 metres) of the sale price on distance from shooting. (The left graph refers to sales in Hässelby-Vällingby. The right graph refers to sales in Spånga-Kista)

Figure 4C shows the price gradient with respect to days relative to shooting for apartment prices within 300 metres and between 300 metres and 800 metres of the shooting locations. Homes located in the near vicinity of the shooting are sold for less than homes further away. However, there is no clear decline in prices for homes closest to the shooting as compared to homes further away after the shooting has occurred. Judging by eye, both graphs seem to follow similar trends, albeit at different total sales prices. If so, then a sharp decline in the sales prices of homes in the near vicinity after the shooting that broke the common price trend with homes further away could indicate the presence of treatment effects on adjacent homes and the absence of an effect on homes beyond 300 metres.

Figure 4C. Price trends before and after shooting



Note: Price trends before and after the occurrence of shooting (bandwidth 12 days).
(The left graph refers to sales in Hässelby-Vällingby. The right graph refers to sales in Spånga-Kista)

4.3 Statistical Estimation framework

The empirical estimation models are based on a cross-sectional difference estimator and a difference-in-difference estimator. Following the procedure of Linden and Rockoff, we first plot apartment sales on the Y-axis and the distance to the coordinates of a shooting on the X-axis in a diagram and use local polynomial regressions to construct two separate graphs for the sales prices for the pre and post period of the shooting. The purpose of this was to identify whether there were any clearly discernible cut-off points in distance to the shooting where apartment sales prices noticeably decrease in the post-period compared to the pre-period, as identifying such a cut-off point in the distance could suggest appropriate radiuses to be used for defining the treatment and control groups. For example, if we found that sales prices are noticeably lower after shootings within a certain radius, that would motivate closer statistical inspection as to whether this difference is significant and if it can be attributed to a causal effect of shootings.

Using a bandwidth of 0.075 miles (120.7 metres) for the local polynomial regression plot, Linden and Rockoff found that house sales prices drop in the years after the arrival of a sex offender within 0.1 miles but display no noticeable change between 0.1 and 0.3 miles which then motivated their choice of treatment and control groups. However, using a bandwidth of 100 metres and pre-and-post periods of 100 days before and after the shooting, there are no discernible differences in apartment sales price in the post-period compared to the pre-period for any distance in our data set.

We thus decide in advance to define our treatment group as all apartment sales taking place within 300 metres of shooting and the control group containing the apartment sales taking place between 300-800 metres of a shooting. Again, following Lindén and Rockoff, we use a cross-sectional difference estimator to check for pre-existing differences in the characteristics of apartments located within 300 metres of shooting and those located between 300 and 800 metres of a shooting, corresponding to the apartments belonging to the treatment and control group, respectively. The distances of 300 metres and 300-800 metres differ from the replicated article for several reasons, the first being that we do not possess the exact coordinates of shooting, hence we increase the radiuses of both the treatment and control group. Additionally, shootings are likely to be heard over a large area and might impact properties further away (Wilhelmsson, Ceccato & Gerell, 2021)

4.4.1 Cross-sectional difference estimator of apartment characteristics

The reliability of our study is dependent on the similarity between apartments in the treatment and control groups. We, therefore, start by estimating the differences in characteristics that might be present between homes within 300 metres and homes between 300-800 metres by constructing a cross-sectional difference regression which takes the form presented in equation (1)

(1) $$Log(P_{ijt)} = \beta_0 + \beta_1 * Treat_{ijt} + \alpha_t + \varepsilon_{ijt}$$

The log of the apartment sale price[1] is a function of a measure of distance from the gun violence, a random error term (ε_{ijt}) and a year fixed effect (α_t). The dummy variable "Treat" is an indicator variable which takes the value of 1 if an apartment sale occurs within 300 metres of shooting and allows for β_1 to be interpreted as the difference in sales prices between apartments in the treatment and control group. Again, following Linden and Rockoff, we limit the sample to apartment sales that took place before the occurrence of gun violence.

4.4.2 difference-in-differences estimator

The Difference in Difference (DiD) method is a quasi-experimental method that makes use of data from a treatment group and a control group to obtain an appropriate counterfactual to measure a causal effect (Jeffrey m. Wooldridge, 2018). A natural experiment occurs when some exogenous event changes the environment. In our case, that change is the occurrence of a shooting in an area. In this methodology, there is a control group which is not affected by the change, and a treatment group which in contrast is thought to be affected by the change. The Key assumption for DiD is that the treatment and control groups have parallel (or common trends) in the absence of the change (ibid). In this study, we estimate the effect of gun violence on property values. Ideally, we would want to observe both the property values after exposure to local incidents of gun violence and the counterfactual property values, comparing treated property values with the same values had they not been treated, which is not possible. Neither can we observe reversions of treatment to infer causality as Bishop (2008) did, due to the nature of gun violence. Instead, we use a control group that is like the treated group but is presumed to not experience the change.

In this study, the treatment group consists of apartments located within 300 metres of the shooting and the control group are apartments located between 300-

[1] of observation *i*, in district *j* and year *t*. Districts: Spånga and Kista, Hässelby and Vällingby

800 metres of a shooting. *Treat* is a dummy variable denoting the treatment group, assuming a value of 1 if the sold apartment is located within 300 metres of a shooting location. *Post is a* dummy denoting the time after a shooting has occurred, assuming the value 1 if the *Treat_post* dummy denotes the treated group after the shooting. X represents all observable apartment characteristics and alpha is the year-fixed effect
The difference in differences model is specified as follows:

(2) $$Log(p_{ijt}) = \beta_0 + \beta_1 * Treat_{ijt} + \beta_2 * Post_{ijt} + \beta_3 * Treat\_Post_{ijt} + \beta_4 * X_i + \alpha_i + \varepsilon_{ijt}$$

5. Estimation results

51. Pre-arrival differences in characteristics of sold apartments

We examine possible variations between groups for every apartment characteristic included as control variables. This is accomplished by letting the characteristic of interest take the place of the dependent variable in equation (1). For instance, to examine the variation in build year characteristics we use the following regression:

$$Build\_year_{ijt} = \beta_0 + \beta_1 * Treat_{ijt} + \alpha_t + \varepsilon_{ijt}$$

We find no evidence of any pre-existing difference in the sale price or other characteristics in Spånga-Kista (Table 5). In Hässelby-Vällingby, however, the sale price and building stories differ significantly at the 5% level. Apartments within 300 metres of a future shooting sold for 7.64% less and the apartment building was 1.241 stories shorter on average than apartments between 300-800 metres. The areas compared in Spånga-Kista are very homogenous. In Hässelby-Vällingby it differs in some attributes.

Table 5. Pre-shooting differences in average characteristics of apartments sales sold close (<300m) to shooting compared to the control group (300m<d<800)

Pre- shooting differences								
Spånga-Kista	Log price	Build year	Building storeys	Aprmt floor	Living area	Rooms	Monthly fee	Elevator
Within 300m	-0.00800	2.952	2.902	0.823	-1.051	-0.00863	-11.42	0.142
	(0.0224)	(1.422)	(1.507)	(0.319)	(2.695)	(0.0415)	(50.58)	(0.0966)
Constant	14.46***	1,982***	5.849	3.264	67.53**	2.635**	4,141**	0.628
	(0.0899)	(7.204)	(2.535)	(0.972)	(2.460)	(0.0581)	(268.6)	(0.180)
Observations	2,061	2,061	1,481	1,856	2,061	2,055	2,053	1,975
R-squared	0.013	0.014	0.029	0.008	0.007	0.004	0.008	0.028
Häss.-Väll.	Log price	Build year	Building storeys	Aprmt floor	Living area	Rooms	Monthly fee	Elevator
Within 300m	-0.0764**	-0.634	-1.241**	-0.719	-2.661	-0.171	-32.81	-0.0208
	(0.00540)	(3.781)	(0.0886)	(0.386)	(1.880)	(0.0512)	(124.4)	(0.0721)
Constant	14.59***	1,977***	6.209*	4.344	60.25***	2.311***	3,667**	0.617
	(0.0567)	(14.08)	(0.552)	(1.441)	(0.625)	(0.00271)	(186.3)	(0.141)
Observatios	812	812	575	723	812	801	801	801
R-squared	0.034	0.143	0.122	0.044	0.016	0.020	0.054	0.208

Robust standard (clustered by district) errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: pre-shooting refers to 100 days before the occurrence of the shooting

5.2 Estimation Results

We start by presenting estimates of equation (1) in column 1 of table 6 and 7, including sales of all apartments within 800 metres of a shooting of any category in Spånga-Kista and Hässelby-Vällingby and a year fixed effects but without controlling for other apartment characteristics. The estimate of β_1 from this specification is a measure of the average price difference between apartments within 300 metres and apartments between 300-800 metres of distance to the location of a future shooting. This difference is 1.2% in Spånga-Kista and 5.28% in Hässelby-Vällingby (Column 1 of Table 6 and 7). Apartments in the near vicinity of future shooting locations are sold for less prior to the shootings but this difference is not statistically significant. When apartment characteristics are included as control variables in the equation, the differences in sales prices are reduced to 0.766% and 1.79% respectively (Column 2 of Table 6 and 7) but still not statistically significant. Thus, for the category "All shootings", we find no statistically significant evidence of differences between the treatment and control group in the pre-treatment period in terms of the apartment sale price. Additionally, the reduction in estimated differences seen in Column 2 compared to column 1 when including apartment characteristics indicates that the control variables capture a large amount of the variation, further justifying the inclusion in the regression model. The results of the cross-sectional difference estimation thus support our previous predictions of similarity of sold apartments, upon which the internal validity and explanatory value of our study hinges on.

Column 3 of table 6 and 7 presents a pre-post comparison using equation (2). In addition to including apartment characteristics and year fixed effects, we have also included a time limitation of 100 days before and 100 days after a shooting. Due to this, the pre-existing differences for All shootings are estimated to be -2.38% for the treatment group compared to the control group (Column 3 of Table 6), in contrast to the estimate of 0.766% in absence of the time limitation, although still not reaching statistical significance. Thus, the introduction of the time limitation does not impose on the assumptions of homogenous treatment and control groups.

Table 6. Impact of Shootings on apartment value in Spånga-Kista

	Log (sale price) pre-shooting		Log (sale price), pre -and post-shooting			
			All shooting	Fatal	Injurious	No harm
Spånga-Kista	(1)	(2)	(3)	(4)	(5)	(6)
Within 300m of shooting	-0.0120	-0.00766	-0.0238	0.0883*	0.00351	-0.0825
	(0.0128)	(0.00884)	(0.00435)	(0.0107)	(0.00694)	(0.0297)
Within 300m*post-shooting			0.0106	0.0130*	-0.0138	0.0152
			(0.00563)	(0.00162)	(0.00976)	(0.00554)
Post-shooting			0.00158	-0.0201**	0.0278	-0.00938
			(0.00496)	(0.000873)	(0.00467)	(0.0153)
Aprmt characteristics		✓	✓	✓	✓	✓
Year fixed effect	✓	✓	✓	✓	✓	✓
100 days pre- and post-shooting			✓	✓	✓	✓
Constant	14.49***	5.250	5.014	-0.0722	6.534	6.168
	(0.00223)	(1.419)	(1.897)	(2.196)	(2.679)	(2.261)
Observatios	14,388	9,715	2,873	514	1,166	1,193
R-squared	0.000	0.675	0.660	0.757	0.680	0.649

Standard errors(clustered by district) in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: pre-shooting and post-shooting refer to 100 days before and 100 days after the date of occurrence of a shooting. Equation (1) is for columns 1 and 2. Equation (2) is used for columns 3, 4, 5 and 6.

Table 7. Impact of Shootings on apartment value in Hässelby-Vällingby

	Log (sale price) pre-shooting		Log (sale price), pre -and post-shooting			
			All shooting	Fatal	Injurious	No harm
Hässelby-Vällingby	(1)	(2)	(3)	(4)	(5)	(6)
Within 300m of shooting	-0.0528	-0.0179	-0.0343	-0.0546	0.00428	-0.0821**
	(0.0182)	(0.00755)	(0.0146)	(0.0118)	(0.00967)	(0.00483)
Within 300m*post-shooting			0.00170	0.0200	-0.0791	0.109**
			(0.00834)	(0.0347)	(0.0311)	(0.00440)
Post-shooting			-0.00298**	0.0104	-0.00620	-0.0169
			(0.000159)	(0.00503)	(0.0189)	(0.00333)
Aprmt characteristics		✓	✓	✓	✓	✓
Year fixed effect	✓	✓	✓	✓	✓	✓
100 days pre- and post-shooting			✓	✓	✓	✓
Constant	14.65***	10.66**	10.77**	9.419**	11.30	10.98***
	(0.00308)	(0.262)	(0.267)	(0.374)	(1.939)	(0.0921)
Observatios	7,610	5,594	1,162	418	359	385
R-squared	0.007	0.658	0.552	0.785	0.589	0.435

Standard errors (clusterered by district) in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: pre-shooting and post-shooting refer to 100 days before and 100 days after the date of occurrence of a shooting. Equation (1) is for columns 1 and 2. Equation (2) is used for columns 3, 4, 5 and 6.

Within our framework, the effects of gun violence are estimated by comparing differences in sale prices before and after a shooting, between apartments within 300 metres of a shooting and apartments between 300-800 metres of a shooting, which are assumed to represent the counterfactual change in the treatment group had the shooting not occurred.

Within 100 days prior to shooting, apartments within 300 metres were sold for 2.38% less, on average, than apartments between 300-800 metres in Spånga-Kista (Column 3 of Table 6). The isolated effect of all types of shooting is 0.0106 which means that on average, a nearby shooting contributes to prices of apartments being sold within 100 days after a shooting, increasing by 1.06% compared to if the shooting had not occurred. This effect is not statistically different from zero at any reasonable confidence level.

The individual effects of the three categories of gun violence, fatal, injurious and non-harmful, have been estimated using equation (2). Columns 5 and 6 in table 6 show that injurious shootings and shootings leading to no harm have demonstrated no statistically significant results in Spånga-Kista. Column 4 in table 6 estimates the effect of fatal shootings. First, we see that within 100 days prior to a shooting, the apartments within 300 metres were sold for 8.83% more, on average, than apartments between 300-800 metres in Spånga-Kista which is statistically significant at the 10% level. Second, the isolated effect of fatal shootings is 0.0130 which means that on average, a nearby fatal shooting contributes to a 1.3% increase in the prices of apartments that are sold within 100 days after the event. This effect is statistically significant at the 10% level. However, the statistically significant pre-existing 8.83% difference in sales price contradicts the assumption of homogenous treatment and control group when studying the category of fatal shootings individually. If the control group is too different, then the credibility of interpreting the coefficient Post*Treat as the marginal effect of the treatment is diminished.

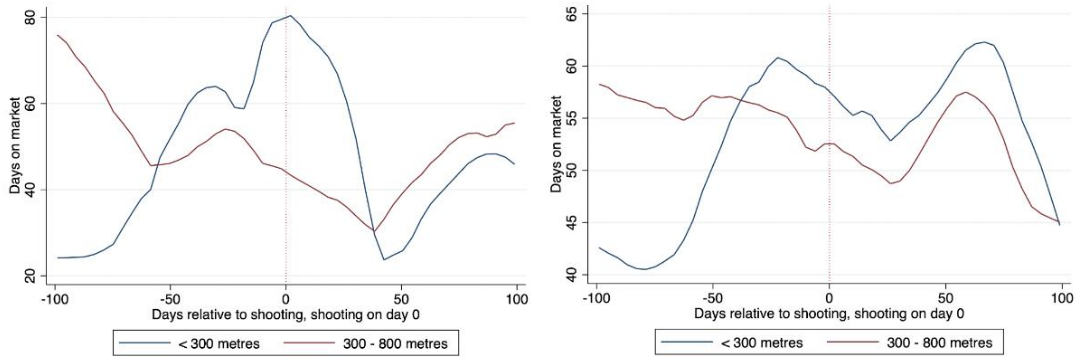
In Hässelby-Vällingby, non-harmful shooting is the only type of gun violence that has a statistically significant effect at a 5% level. Before the shooting, the apartments within 300 metres were sold for 8.21% less, on average, than apartments between 300-800 metres (Column 6 Table 7). The isolated effect of non-harmful shootings is 0.109 which means that on average, a nearby non-harmful shooting contributes to a 10.9 % increase in the prices of apartments that are sold within 300 metres and 100 days after the event. This effect is statistically significant at the 5% level.

5.3 Days on market

We have additionally included a measure of time on the market it takes for an apartment to sell. Figure 5 shows the number of days it took for apartments to sell, on average, on the Y-axis. The X-axis indicates when the sale was put on sale relative to the occurrence of gun violence. In Hässelby-Vällingby, it took, on average, approximately 80 days for a sale that was put for sale on the same as the occurrence of the shooting within 300 metres while it only took circa 45 days between 300-800 metres. Wentland, Waller and Brastow

(2014) argue that failing to include liquidity in the modelling of statistical tests of crime effects on property values results in simultaneity bias, as sales price and days-on-market are jointly determined. In addition, changes in housing liquidity are relevant to the housing market and sellers. Additional time on the market might be negative for sellers who are in the process of buying somewhere else, affecting the holding costs and the subsequent seller reservation prices (Wentland, Waller & Brastow, 2014). According to the different proposed relationships in the literature, nearby gun violence can be thought to affect local housing liquidity in different ways. The cause of nearby gun violence may reduce the number and size of potential buyers and in turn lengthen the time on the market. On the other hand, crime averse sellers eager to move after the shooting might lower their reservation prices noticeably to close a faster sale, having a shortening effect on the apartment's time on the market.

Figure 5. Days on market



Note: this figure shows the days it took for the apartment to be sold. For instance, it took, on average, approximately 80 days for an apartment within 300 metres to be sold in Hässelby-Vällingby when it was put on sale the same day as the occurrence of the shooting. (The left graph refers to sales in Hässelby-Vällingby. The right graph refers to sales in Spånga-Kista)

It is plausible that gun violence may affect housing liquidity, defined as the number of days between the list date (in our data denoted as *initidal_ad_date*) and the contract date. Glower, Laurin & Henderschott (1998) demonstrated that time on the market is reduced for highly motivated sellers who have new employment and are already buying elsewhere. Accordingly, crime averse sellers might be compelled to sell quickly following a shooting.

The occurrence of gun violence may affect the housing liquidity not only in the near vicinity but in the whole neighbourhood because the potential buyers may associate it with the area rather than where it exactly occurred. (Wilhelmsson et.al., 2021) suggest that the effects of gun violence might be more far-reaching than other violent crimes due to the discernible noise together with more extensive media coverage compared to other types of crime. While the risk of bystanders being injured in a shooting is small, shootings could negatively influence the local housing market mainly due to an increased fear of crime in general rather than resulting from market participants estimating their risk of being subject to gun violence specifically. Notably, studies have found acts of criminal damage and vandalism to the property to contribute more to feelings of unsafety than do actual increases in burglaries (Gibbons 2004), with vandalism increasing fear of crime and this impact having synergistic effects in neighbourhoods presenting with signs of decline (Ceccato & Wilhelmsson 2012), something that is distinctive to socially vulnerable areas. Past occurrences of shootings of any category might leave direct traces in the surroundings, which could then potentially induce stronger reactions among residents in socially vulnerable areas. For instance, following a shooting, bullet holes might be present in structures surrounding the location, while locations of fatal shootings might indirectly provide evidence of a past shooting in the form of memorial items intended to honour the victim. Such traces might not persist over longer periods of time but are likely to persist within the studied period of approximately three months after a shooting.

To measure the effect of gun violence on liquidity and price concessions of the local housing market, we create three different dummies. The first dummy denotes the apartments that were put on sale and that were sold before the shooting. The second dummy is for the apartments that were put on sale before the shooting but that were sold after the shooting. Lastly, the third dummy is for the apartments that were put on sale after the shooting (and sold after the shooting). The first and the third dummies do not experience disturbance in terms of shooting, but the second dummy does and hence could be affected.

Filippova and Rehm (2014) find that in buoyant markets, properties that have gone long without selling are attached a stigma due to being perceived as deficient, which is relevant in our research setting, with housing prices having increased in Sweden at a rate that stands out compared to the other Nordic countries (Bergman & Nyberg, 2021). Hence, we attempt to exclude those apartments already subject to stigma and to avoid both negative and extreme values we set time constraints for days-on-market between 0-365 days. The season in which the apartment is listed has been shown to affect time on the market (McGreal et. al, 2009). Therefore, the time constraint encompasses a whole year to average out the differences in time on the market for different observations resulting from seasonality. The apartments that are put on sale before shooting and sell after, stay, on average, much longer on the market (Table 8).

Table 8. Summary characteristics (Days on Market)

Spånga-Kista	Obs	Mean	Std. Dev.	Min	Max
Ad date before, sell date after	6539	115.281	97.05	1.235	360.253
Ad date before, sell date before	113311	41.052	55.778	0	360.253
Ad date after, sell date after	125489	37.394	50.484	0	359.654
Hässelby-Vällingby	Obs	Mean	Std. Dev.	Min	Max
Ad date before, sell date after	1302	103.967	88.038	4	349.563
Ad date before, sell date before	33657	38.993	55.563	0	363.419
Ad date after, sell date after	18379	33.567	43.733	0	347.177

Note: We have limited days-on-market to be between 0 and 365 days to reduce outliers.

We estimate the relationship between days on market with these three explanatory variables using ordinary least square regression (Table 9). Days-on-market for apartments that were on the market during the occurrence of the shooting increased significantly. It took approximately 115 days, on average, in Spånga-Kista and 104 in Hässelby-Vällingby for these apartments to be sold. Apartments that were both put on sale before and sold before the shooting spent, on average, 41/39 days on the market while apartments that were put on sale after spent on average 37/34 days on the market.

Table 9. The impact of the shooting on apartments that were on sale during the shooting

	Spånga-Kista	*Hässelby-Vällingby*
Ad date before, sell date after	65.86***	65.11***
	(4.516)	(8.365)
Ad date before, sell date before	-8.369*	0.134
	(4.357)	(8.007)
Ad date after, sell date after	-12.03***	-5.293
	(4.356)	(8.008)
Constant	49.42***	38.86***
	(4.354)	(8.001)
Observations	245,520	53,403
R-squared	0.049	0.039

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Regression results with days-on-market as the dependent variable.

5.4 Price concessions

Asabere & Huffman (1993), defined price concessions as the list price minus the sales price. We use equation (2) to instead estimate the difference between seller reservation prices and the initial asking price. A negative value thus implies that the seller reservation prices, or closing prices, were on average lower than the initial asking prices or list prices. Here, the post indicates all apartments that were put on sale during the shooting.

In the control group, apartments not on sale during the shooting sold, on average, for 81503 SEK more than the initial asking prices and apartments on sale during the shooting sold for only 33635 SEK more (in Spånga-Kista). In the treatment group, apartments not on sale during the shooting sold, on average, for 77402 SEK more than the initial asking prices while apartments that were put on sale during the shooting sold for only 32107 SEK more. We see that the occurrence of a shooting in the marketing period was correlated with price concessions both in the control group and treatment group. Proximity (within 300 metres) to the shooting was associated with a smaller average price concession in Spånga-Kista but not significantly. In Hässelby-Vällingby in the control group, apartments not on sale during the shooting sold, on average, for 160386 SEK more than the initial asking price and apartments on sale during the shooting sold for 124811 SEK. In the treatment group, apartments not on sale during the shooting sold, on average, for 154112 SEK more while apartments that were put on sale sold, on average, for only 87499 SEK more. Proximity to the shooting was associated with a higher average price concession in Hässelby-Vällingby but not significantly.

Table 10. Differences between seller reservation price and initial asking price after a shooting

	Spånga-kista	*Hässelby-Vällingby*
Ad date before, Sale date after	-47,868*	-35,575
	(5,452)	(18,541)
Ad date before, Sale date after within 300m	2,573	-31,038
	(6,263)	(50,653)
Within 300m of shooting	-4,101	-6,274
	(3,399)	(11,094)
Constant	81,503**	160,386*
	(5,048)	(19,822)
Observations	26,696	10,385
R-squared	0.004	0.001

Robust standard errors (clustered by district) in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The table shows the difference between end price and asking price for apartments that were in the sale between 0 - 365 days of shooting (to reduce outliers)

5.5 Robustness

To assess the robustness of our study, we run placebo tests using equation (2) and the exact same difference-in-differences specification while replacing the dates of each occurrence of shootings of any category with dates 100 days prior to the actual date of the incidence. Linden and Rockoff (2008) used falsification tests to confirm that they did not estimate a false negative impact of the offender's arrival. For instance, if areas near a future offender's location demonstrated a slower growth in housing prices compared to other houses, this could result in a false negative impact of the offender's arrival. We find no evidence of a false effect in Spånga-Kista. In Hässelby-Vällingby apartments close to fatal shootings are selling for 7.97% less compared to 5.46% less from the main result. What is contradictory, however, is that non-harmful shooting has a statistically significant large negative impact. This is likely due to the usage of the false date in the placebo model, in which the 100 days after a non-harmful shooting corresponds to the period of 100 days before a non-harmful shooting in the regular model, in which we found significant pre-existing differences.

Table 11. Manipulating the data to believe that the crime occurred 100 days before the actual day

	Log (sale price), pre -and post-shooting			
Spånga-Kista	All shooting	Fatal	Injurious	No harm
Within 300m of shooting	0.00567	0.116**	0.00310	-0.0392
	(0.00559)	(0.00347)	(0.00934)	(0.00877)
Within 300m*post-shooting	-0.0314	-0.0269	-0.00773	-0.0398
	(0.0137)	(0.0103)	(0.0120)	(0.0179)
Post-shooting	0.0112*	-0.00552	0.00988	0.00932
	(0.000961)	(0.0169)	(0.00673)	(0.00645)
Aprmt characteristics	✓	✓	✓	✓
Year fixed effect	✓	✓	✓	✓
100 days pre- and post-shooting	✓	✓	✓	✓
Constant	5.199	-0.748	8.007	5.869
	(2.649)	(1.864)	(5.273)	(2.207)
Observatios	2,652	462	1,071	1,119
R-squared	0.676	0.755	0.681	0.680

Standard errors(clustered by district) in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: pre-shooting and post-shooting refer to 100 days before and 100 days after the date of occurrence of the shooting

Table 12. Manipulating the data to believe that the crime occurred 100 days before the actual day

	Log (sale price), pre -and post-shooting			
Hässelby-Vällinby	All shooting	Fatal	Injurious	No harm
Within 300m of shooting	-0.00162	-0.0797**	0.0137	0.00902
	(0.0106)	(0.00603)	(0.00563)	(0.00598)
Within 300m*post-shooting	-0.0352	0.0416	-0.00223	-0.103***
	(0.00669)	(0.00896)	(0.000452)	(0.000340)
Post-shooting	-0.00404	-0.00191	0.00273	-0.0235*
	(0.0108)	(0.00296)	(0.0192)	(0.00210)
Aprmt characteristics	✓	✓	✓	✓
Year fixed effect	✓	✓	✓	✓
100 days pre- and post-shooting	✓	✓	✓	✓
Constant	10.51**	9.132**	10.71**	10.14**
	(0.264)	(0.170)	(0.681)	(0.197)
Observatios	1,224	493	380	351
R-squared	0.644	0.854	0.807	0.454

Standard errors(clustered by district) in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: pre-shooting and post-shooting refer to 100 days before and 100 days after the date of occurrence of the shooting

6. Analysis

We find no conclusive evidence of gun violence negatively affecting local property prices, although results in Hässelby-Vällingby indicate that non-harmful gun violence positively impacts property prices. Additionally, results in Spånga-Kista indicate a positive effect of fatal shootings on property prices, though these are only significant at the 10% level. If we first assume that these results accurately represent reality, then one potential reason for the positive effect of shootings on prices could be that there are unobserved disamenities, such as crime occurrence, within the treated areas which already discount the apartment prices. We reasoned in section 5.2 that socially vulnerable areas are likely to be particularly sensitive to increases in fear of crime in response to gun violence. However, the converse could also be true, with apartment prices within treated areas already being discounted due to fear of crime, with the marginal effect of shooting being lower than for areas with lower amounts of crime.

Another reason could be the higher time on the market for apartments in Hässelby-Vällingby that were being marketed when a shooting occurred. Since time on the market is significantly higher for those apartments compared to apartments that are both listed and sold before any shooting, it could be that the longer time on the market enabled the appearance of a dedicated buyer. Suppose that in the counterfactual state, residents who are already marketing their homes at the time of the shooting are already in the process of buying and/or moving elsewhere. In that scenario, they might be more willing to accept lower reservation prices to reduce the time on the market instead of waiting for a dedicated buyer with a higher willingness to pay for the specific apartment. However, if shootings reduce the pool of potential buyers, then the time on the market is increased even for sellers who are concurrently buying elsewhere. The seller reservation prices might then increase, due to increased time on the market, while altogether this is not necessarily something positive for sellers.

Nevertheless, there are limitations to our study, further discussed below, which contradict the notion of our observed results being representative of the actual relationships between gun violence and housing markets, invalidating the above reasoning.

6.1 Limitations

The degree to which our difference in differences model accurately predicts a causal effect depends among other things on how much the changes in the control groups represent the contrafactual changes in the treatment group. In studies that employ hedonic models to estimate the effect of a treatment on changes in property sales prices in a cross-sectional sample, an omitted variable problem arises if all relevant property attributes fail to be included as control variables. Local amenities and disamenities are likely to also influence sale prices and can be included in the regression model to some extent, although it remains difficult to include every relevant attribute. In the context of sex offender registries Linden and Rockoff (2008) suggest the possibility that in addition to sex offenders tending to move into areas with lower quality housing in terms of living area and number of rooms, offenders might also move into residences that are discounted due to some factors that are unobserved by the researcher, such as "moving in next to the local 'eyesore'". The discounted price might reflect not the quality of the property per se, but rather a distaste due to factors unobservable to the researcher. For example, as shootings have been demonstrated to be spatio-temporally tied to open drug markets (Gerell et al., 2021), one would have to discern the individual effect of both phenomena on property prices before drawing conclusions on the effect of shootings. Even within areas with frequent shootings, property discounts in proximity to shooting locations could be better explained by distaste for the presence of the drug market rather than a reaction to occurred shootings.

The difference-in-differences model employed in this study manages this problem by comparing differences in sales before and after shootings between the treatment and control groups. Thus, any observed effect is unlikely to be due to local (dis)amenities, unless (dis)amenities tend to emerge in conjunction with the treatment. Following the example above, if eyesore properties are not commonly emerging close by following the arrival of an offender, the decline in prices can be attributed to the arrival itself.

While shootings are related to open drug markets, these can be considered stable over several years (Gerell et al., 2021). It is thus unlikely that any observed difference-in-differences is due to the emergence of open drug markets, although these disamenities pose a problem in disrupting the assumed homogeneity between the treatment and control group. If open drug markets are commonly present in adjacent locations of future shootings and these are stable over time, it might be that apartment prices near shootings are already discounted due to the presence of criminality. While shootings might still affect these prices, it is also possible that the effect of higher crime concentration is diminishing, that is, the apartments within the treatment group are not as sensitive to gun violence as other apartments, which would underestimate the effect of gun violence.

Among the strengths of this study is both the treatment and control groups belonging to the same neighbourhoods to a large extent. The results thus accurately represent the effects within neighbourhoods. However, we have not considered that homes that sell might have different attributes than homes in general (Linden, Rockoff 2008). The aversion towards living in the close vicinity of the location of a past shooting is thus not necessarily reflected in the sales price drops, since it might just reflect the preferences of the households that move. These might be different from other households. Also, the households living in *"bostadsrätter,"* a type of owner-occupied apartment, might have different attitudes towards the risk of crime than local tenants. Residencies in socially vulnerable areas in general and the neighbourhoods included in the areas of our study are typically rental apartments. Households that own their apartments might not estimate the risk of being subjected to crime the same way that rental tenants do.

7. Conclusion

Hedonic estimation methodology is fittingly used to estimate the short-term effects of gun violence on property values in socially vulnerable areas in Stockholm municipality. Using a dataset consisting of 6000 apartment sales and using a difference-in-differences framework inspired by Linden and Rockoff (2008) to test the effects of incidences of gun violence on the local property market for two separate areas, Spånga-Kista and Hässelby-Vällingby in Stockholm, Sweden, yields unexpected results. We interweave the observed impact on different housing market metrics from our study and the extant literature explore possible explanations for these, but the implications regarding residents's crime aversion and the cost of crime remain inconclusive. The advantages of studying treatment effects within small areas of specific socioeconomically disadvantaged neighbourhoods have to be weighed against the difficulties in constructing a statistical model that both includes enough observations and is internally valid by virtue of the similarity between treatment and control groups.

8. Further research

Further research employing similar methods might seek to expand the radius of both the treatment and control group in relation to the occurred shooting or crime. In Spånga-Kista, the *Post* dummy was significant for the difference-in-differences estimation of the effect of fatal shootings, meaning that prices of apartments within 800 metres significantly declined after the occurrence of a shooting. While a causal effect of fatal shootings on the whole area within 800 metres cannot be inferred, the observed significant result suggests that future research using similar methods could expand the radiuses of the treatment and control group. This should be possible with access to a larger dataset that optimally contains information on sold apartments from socially vulnerable areas in all of Sweden's larger cities, as well as provides data for all existing residences in the studied areas to combat one of the sources of selection bias that arises from only studying apartments that sell.

Since several studies have demonstrated the influence of local levels of different types of crime on property prices, such disamenities need to be included within the hedonic pricing models for greater accuracy, both in terms of the marginal effects of the levels of a different crime, but also their interaction with the treatment variable. Finally, more robust tests for the effects on housing liquidity and price concessions are needed, as our models do not allow any causal claims and the research on the relationship between these aspects of the housing market and crime is scarce. If housing liquidity is thought to decrease following an event and this is hypothesised to be partly the result of a reduced pool of buyers, it would be interesting to explore whether this effect is moderated by media articles or the number of google searches of the specific event.

Bibliography

Anglin, M. Paul., Rutherford, Ronald & Thomas M. Springer, (2003), The trade-off between the Selling price of residential properties and time-on-the-market: The impact of price setting, *Journal of Real Estate Finance and Economics*, 26(1): 95-111

Asabere, K. Paul & Forrest E. Huffman, (1993), Price concessions, time on the market, and the actual sale price of homes, *Journal of Real Estate Finance and Economics*, 6:167-174

Bajari, Patrick., Fruehwirth Cooley, Jane., Il, Kyoo, Kim & Christopher Timmins, (2012), A rational expectations approach to hedonic price regressions with time-varying unobserved product attributes: The price of pollution, *American Economic Review*, 102(5): 1898–1926

Bayer, Patrick., Ferreira, Fernando & Robert McMIllan, (2007), A unified framework for measuring preferences for schools and neighbourhoods, *Journal of political economy*, 2007, 115:4, 566-638.

Bergman, A. Mats & Sten Nyberg, (2021), Konkurrens och prisbildning på den svenska bostadsmarknaden, *SNS Analys*, 81

Bishop, C. Kelly & Alvin, D. Murphy, (2011), Estimating the willingness to pay to avoid violent crime: A dynamic approach, *American Economic Review*, 101(3): 625-629.

Black, E. Sandra, (1999), Do better schools' matter? Parental valuation of elementary education, The *Quarterly Journal of Economics*, 114(2): 577-599.

Boustan, Platt Leah, (2012), School desegregation and Urban Change: Evidence from City Boundaries, *American Economic Journal: Applied Economics*, 4(1): 85–108

Bowes, R. David & Ihlanfeldt, R. Keith, (2001), Identifying the Impacts of Rail Transit Stations on Residential Property Values, *Journal of Urban Economics*, 50(1): 1-25.

Caudill, B. Steven., Affuso, Ermanno & Ming Yang (2015), Registered sex offenders and house prices: A hedonic analysis, *Urban studies,* 52(13): 2425-2440.

Ceccato, Vânia & Mats Wilhelmsson, (2011), The impact of crime on apartment prices: Evidence from Stockholm, Sweden, *Geografiska Annaler: Series B, Human Geography,* 93 (1): 1-23

Ceccato, Vânia & Mats Wilhelmsson, (2012), Chapter 8: Acts of vandalism and fear in neighbourhoods: Do they affect housing prices? *The Urban Fabric of Crime and Fear,* (Department of Urban Planning and Evirononment, Royal Institute of Technology, Sweden)

Ceccato, Vânia & Mats Wilhelmsson, (2020), Do crime hotspots affect housing prices? Nordic *Journal of Criminology*, 21(1): 84-102

Cullen, Berry Julien & Steven D. Levitt (1996) Crime, Urban flight and the consequences for cities - *Review of economics and statistics* (1999), 81(2): 159-169.

Dagens Nyheter (2021), Här är alla skjutningar i Stockholm de senaste fem åren. https://www.dn.se/sthlm/har-ar-alla-skjutningar-i-stockholm-de-senaste-aren/

Epple, Dennis (1987) - Hedonic prices and Implicit markets: estimating demand and supply functions for differentiated products, *Journal of Political Economy*, 95(1): 59-80.

Filippova, Olga & Michael Rehm, (2014), Market conditions, Marketing time, and House prices - *Journal of Housing Research*, 23(1): 45-56.

Gerell, Manne., Sturup, Joakim., Magnusson, Mia-Maria., Nilvall, Kim., Khoshnood, Ardavan & Amir Rostami, (2021), Open drug markets, vulnerable neighbourhoods and gun violence in two Swedish cities, *Journal of Policing, Intelligence and Counter Terrorism*, 16(3):223-244.

Gibbons, Steve (2004), The costs of urban property crime , *The Economic Journal*, 114(499): 441-46.

Glower, Michael., Haurin, R. Donald., & Patric H. Hendershott (1998), Selling time and selling price: the influence of seller motivation, *Real Estate Economics* 26(4): 719-740.

Larsen, E. James., Lowrey, J. Kenneth & Joseph W. Coleman, (2003), The effect of proximity to a registered sex offender's residence on single-family house selling price, *Appraisal Journal*, 71(3): 253-265.

Linden, L. Leigh & Rockoff, E. Jonathan, (2008), Estimates of the impact of crime risk on property values from Megan's laws, *American Economic Review*, 98(3): 1103–1127.

Lynch, K. Allen and David W. Rasmussen (2001) - Measuring the impact of crime on house prices, *Applied economics*, 33(15): 1981-1989.

Mapsdirections.info, (2022), https://www.mapsdirections.info/en/measure-map-radius/

McGreal, Stanley., Adair, Alastair., Brown, Louise & James R. Webb, 2009, Pricing and Time on the market for residential properties in a Major U.K. City, *The Journal of Real Estate Research*, 31(2): 209-234.

National Council for Crime Prevention - BRÅ (2021), Rapport 2021:8 Dödligt skjutvapenvåld i Sverige och andra europeiska länder: En jämförande studie av nivåer, trender och våldsmetoder - (Gun homicide in Sweden and other european countries)

National Council for Crime Prevention - BRÅ (2021), Rapport 2021:11 - Nationella trygghetsundersökningen 2021: Om utsatthet, otrygghet och förtroende

National Council for Crime Prevention - BRÅ (2021), Rapport 2021:12 - Nationella trygghetsundersökningen 2021: Teknisk rapport

Pope, C. Jaren (2008), Fear of crime and housing prices: Household reactions to sex offender registries *Journal of Urban economics (*2008), 64(3): 601-614

Swedish Police Authority - Polismyndigheten, Nationella Operativa Avdelningen (2021), Lägesbild över utsatta områden: Regeringsuppdrag 2021

Swedish Police Authority – Polismyndigheten, (2021), Kartgränser utsatta områden i region Stockholm

Sherwin Rosen (1974) - Hedonic prices and implicit markets: Product differentiation in pure competition, *Journal of Political Economy,* 82(1): 34-55.

Stockholm municipality, (2020), Trygghetsmätningen 2020.

Sturup, Joakim., Rostami, Amir., Gerell, Manne and Anders Sandholm, (2018), Near-repeat shootings in contemporary Sweden 2011 to 2015, *Security journal*, 31, 73-92.

Swedish Television, (2021), Skjutningar i Sverige, https://www.svt.se/datajournalistik/skjutningar-i-sverige/?rid=0

Thaler, Richard., (1978), A note on the value of crime control: evidence from the property market - *Journal of Urban Economics*, 5(1): 137-145

Troy, Austin and Morgan J. Grove, (2008), Property values, parks, and crime: A hedonic analysis in Baltimore, MD, *Landscape and Urban planning,* 87(3): 233-245.

Wentland, Scott., Waller, Bennie., & Raymond Brastow, (2014), Estimating the effect of crime risk on property values and time on market: Evidence from Meghan's law in Virginia, *Real Estate Economics*, 42(1): 223-251

Wilhelmsson, Mats., Ceccato, Vânia., & Manne Gerell, (2021), What effect does gun-related violence have on the attractiveness of a residential area? The case of Stockholm, Sweden

Wooldridge, Jeffrey M, (2018), 'Properties of OLS under Measurement Error', *Introductory Econometrics: A Modern Approach.* 7th edition

Appendix

Table 13. The list of all gun violence including date of occurrence and coordination

Spånga-Kista

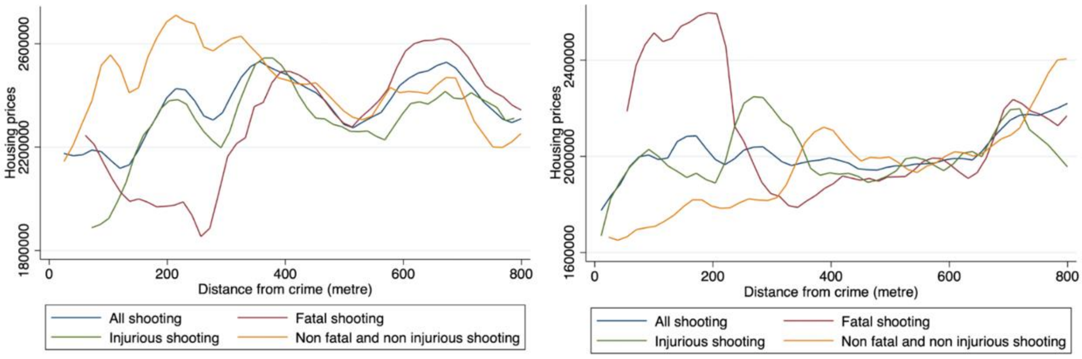
Crime date	Crime type	Latitude	Longitude	Crime date	Crime type	Latitude	Longitude
2018-01-08	Fatal	59.38965473975	17.93014313369	2019-11-23	Non-harmful	59.4083277	17.925686
2018-02-09	Non-harmful	59.38971206170	17.92658443092	2019-12-09	Injurious	59.3858041	17.9276115
2018-03-10	Injurious	59.40283012556	17.94539300637	2019-12-31	Injurious	59.3922971	17.9299722
2018-03-16	Non-harmful	59.37662983458	17.90789243916	2020-01-09	Injurious	59.4031709	17.9385915
2018-03-17	Injurious	59.39456170502	17.91109628181	2020-03-09	Non-harmful	59.3925158	17.9042589
2018-04-03	Non-harmful	59.39057138295	17.92838672797	2020-03-09	Non-harmful	59.4008898	17.9304925
2018-04-15	Injurious	59.39566795884	17.90238486625	2020-03-17	Fatal	59.3979767	17.8914371
2018-04-21	Non-harmful	59.39292206807	17.90390155483	2020-03-23	Injurious	59.3737513	17.9197242
2018-04-30	Non-harmful	59.40948978051	17.9247530213	2020-05-20	Injurious	59.3945447	17.9059026
2018-05-02	Non-harmful	59.38043124647	17.8993224728	2020-05-24	Non-harmful	59.3930323	17.9245725
2018-05-23	Injurious	59.40731068234	17.94216047069	2020-06-17	Non-harmful	59.3969415	17.8990584
2018-05-30	Non-harmful	59.39471042987	17.90416245941	2020-06-30	Non-harmful	59.4082509	17.9217495
2018-06-01	Injurious	59.38974741718	17.92303746578	2020-07-03	Non-harmful	59.3957428	17.8857941
2018-06-04	Non-harmful	59.39745104134	17.9007805064	2020-07-17	Non-harmful	59.3904555	17.9302431
2018-06-07	Injurious	59.39562143385	17.90414492003	2020-07-25	Injurious	59.4010151	17.9323476
2018-07-01	Non-harmful	59.39451141238	17.90568977357	2020-08-06	Injurious	59.3973843	17.9107722
2018-07-10	Injurious	59.38621076976	17.92276740072	2020-08-13	Fatal	59.3894631	17.9304296
2018-07-17	Injurious	59.41034364208	17.93013134667	2020-08-20	Fatal	59.3846642	17.8910155
2018-07-26	Non-harmful	59.38815698753	17.9175830013	2020-08-27	Fatal	59.4097684	17.9219638
2018-08-30	Injurious	59.38706480834	17.92285866054	2020-09-24	Injurious	59.394501	17.8978706
2018-09-08	Non-harmful	59.39050629862	17.931887374096	2020-10-07	Injurious	59.4133726	17.9196564
2018-09-19	Non-harmful	59.39236046556	17.900314512032	2020-12-29	Fatal	59.3948087	17.9000251
2018-10-22	Non-harmful	59.37563620512	17.914770168455	2020-12-29	Injurious	59.4104827	17.9275062
2018-12-07	Non-harmful	59.39248429685	17.923266467967	2021-02-08	Fatal	59.3947517	17.8983562
2018-12-17	Injurious	59.39031564214	17.900204355881	2021-02-26	Non-harmful	59.388109	17.9242636
2018-12-26	Non-harmful	59.39117494242	17.902023537058	2021-04-18	Fatal	59.4050643	17.9386534
2019-01-12	Fatal	59.39628294213	17.910455157439	2021-05-02	Non-harmful	59.4021466	17.9458502
2019-01-27	Injurious	59.3953268	17.9135062	2021-05-12	Fatal	59.3968123	17.8869111
2019-01-27	Non-harmful	59.41435510695	17.925807411583	2021-05-31	Fatal	59.3957865	17.8926219
2019-02-16	Non-harmful	59.3955976	17.9027211	2021-05-31	Fatal	59.410004	17.9259312
2019-02-17	Injurious	59.3940458	17.9037523	2021-06-06	Non-harmful	59.3938962	17.9077689
2019-02-22	Non-harmful	59.3894704	17.9282053	2021-07-05	Fatal	59.3902249	17.925958
2019-03-01	Non-harmful	59.4159603	17.921204	2021-07-10	Injurious	59.3889194	17.9071343
2019-03-20	Injurious	59.396694	17.8882324	2021-07-25	Injurious	59.395176	17.9026628
2019-04-01	Injurious	59.3871909	17.9206074	2021-07-31	Non-harmful	59.3969633	17.8992086
2019-05-26	Injurious	59.3913511	17.9329072	2021-08-06	Non-harmful	59.3947704	17.9045725
2019-06-03	Non-harmful	59.4136852	17.9181329	2021-08-10	Non-harmful	59.3960286	17.904238
2019-06-10	Non-harmful	59.3943302	17.9126036	2021-08-16	Fatal	59.3942586	17.9126577
2019-06-18	Injurious	59.4134982	17.9188518	2021-08-29	Non-harmful	59.3953985	17.9013646
2019-07-05	Non-harmful	59.3887194	17.8933028	2021-08-30	Fatal	59.3977348	17.8883446
2019-08-03	Non-harmful	59.388504	17.9274749	2021-09-07	Injurious	59.3954202	17.8969864
2019-08-07	Fatal	59.3917496	17.8954736	2021-10-18	Injurious	59.3922261	17.923247
2019-09-27	Non-harmful	59.4009113	17.9309288	2021-10-28	Injurious	59.4158407	17.9131304
2019-10-07	Injurious	59.392888	17.9250988	2021-11-01	Injurious	59.4102238	17.9260781

Hässelby-Vällinby

Crime date	Crime type	Latitude	Longitude	Crime date	Crime type	Latitude	Longitude
2018-06-13	Fatal	59.3654100	17.8452358	2020-09-17	Non-harmful	59.3662348	17.8439277
2018-11-16	Injurious	59.3665094	17.8788377	2020-09-18	Non-harmful	59.3581265	17.8366767
2019-06-30	Fatal	59.3495826	17.8805997	2020-09-18	Non-harmful	59.3641023	17.8356397
2019-07-04	Injurious	59.3932196	17.8527333	2020-11-03	Fatal	59.3588361	17.895702
2019-08-28	Fatal	59.3590058	17.8819333	2021-05-14	Non-harmful	59.3651983	17.8339279
2020-03-01	Injurious	59.3706893	17.8394757	2021-08-06	Injurious	59.3617359	17.8324179
2020-04-14	Fatal	59.3721195	17.8551796	2021-09-28	Non-harmful	59.3653885	17.8336348
2020-05-29	Injurious	59.3608095	17.8598952	2021-10-03	Non-harmful	59.3626676	17.8392395
2020-07-15	Fatal	59.3722375	17.848618	2021-10-09	Injurious	59.3708615	17.8396206
2020-07-29	Injurious	59.362602	17.8392154	2021-11-12	Fatal	59.3845414	17.8482808
2020-09-08	Injurious	59.3644543	17.8693859				

Note: Dates, type of gun violence, latitude and longitude of each of the shootings gathered for the study.

Figure 6. Apartment sales the first 100 days after the occurrence of gun violence



Note: Local polynomial regression of sale price on distance from shooting
(The left graph refers to sales in Hässelby-Vällingby. The right graph refers to sales in Spånga-Kista)

Figure 6 shows the price gradient of distance to different types of shootings coordinates during the first 100 days after shooting (without bandwidth). Prices for homes closest to fatal shootings are higher, sink with distance until around 300 and then rise again in Hässelby-Vällingby. Prices close to fatal shootings in Spånga-Kista are higher compared to other types of shootings and rise even more till approximately 200 metres away, then sink with distance until nearly 400 metres before flattening out.

Table 14. Spatio-temporally connected shootings in Hässelby-Vällingby

date	latitude	longitude	crime	Crime date	Latitude	Longitude	crime type	Distance	Difference
9/18/2020	59.364102	17.83564	Non-harmful	9/17/2020	59.366235	17.843928	Non-harmful	527.7896	1
10/3/2021	59.362668	17.83924	Non-harmful	9/28/2021	59.365389	17.833635	Non-harmful	439.8439	5
9/17/2020	59.366235	17.843928	Non-harmful	7/29/2020	59.362602	17.839215	Injurious	485.3773	50
9/18/2020	59.358126	17.836677	Non-harmful	7/29/2020	59.362602	17.839215	Injurious	519.0624	51
9/18/2020	59.364102	17.83564	Non-harmful	7/29/2020	59.362602	17.839215	Injurious	263.2176	51
9/28/2021	59.365389	17.833635	Non-harmful	8/6/2021	59.361736	17.832418	Injurious	412.7476	53
10/3/2021	59.362668	17.83924	Non-harmful	8/6/2021	59.361736	17.832418	Injurious	401.5925	58
8/6/2021	59.361736	17.832418	Injurious	5/14/2021	59.365198	17.833928	Non-harmful	395.1593	84
7/15/2020	59.372237	17.848618	Fatal	4/14/2020	59.372119	17.85518	Fatal	373.2837	92

Note: The right-hand side of the table lists initial shootings and is sorted by shooting category. The left-hand side lists follow up shootings.

Table 15 Spatio-temporally close shootings in Spånga-Kista

date	Latitude	Longitude	type	Crime date	Latitude	Longitude	crime Type	Distance	Difference
6/17/2020	59.396941	17.899058	Non-harmful	3/17/2020	59.397977	17.891437	Fatal	448.0728	92
2/8/2021	59.394752	17.898356	Fatal	12/29/2020	59.394809	17.900025	Fatal	95.03263	41
10/7/2020	59.413373	17.919656	Injurious	8/27/2020	59.409768	17.921964	Fatal	422.3559	41
5/31/2021	59.395786	17.892622	Fatal	5/12/2021	59.396812	17.886911	Fatal	343.987	19
1/27/2019	59.395327	17.913506	Injurious	1/12/2019	59.396283	17.910455	Fatal	203.4537	15
2/17/2019	59.394046	17.903752	Injurious	1/12/2019	59.396283	17.910455	Fatal	455.1259	36
7/25/2021	59.395176	17.902663	Injurious	5/31/2021	59.395786	17.892622	Fatal	574.5119	55
8/30/2021	59.397735	17.888345	Fatal	5/31/2021	59.395786	17.892622	Fatal	325.8216	91
7/31/2021	59.396963	17.899209	Non-harmful	5/31/2021	59.395786	17.892622	Fatal	396.5118	61
8/29/2021	59.395398	17.901365	Non-harmful	5/31/2021	59.395786	17.892622	Fatal	498.5906	90
5/2/2021	59.402147	17.94585	Non-harmful	4/18/2021	59.405064	17.938653	Fatal	522.261	14
9/7/2021	59.39542	17.896986	Injurious	5/31/2021	59.395786	17.892622	Fatal	251.3027	99
2/16/2019	59.395598	17.902721	Non-harmful	1/12/2019	59.396283	17.910455	Fatal	445.9869	35
9/7/2021	59.39542	17.896986	Injurious	8/30/2021	59.397735	17.888345	Fatal	554.5587	8
2/9/2018	59.389712	17.926584	Non-harmful	1/8/2018	59.389655	17.930143	Fatal	202.3225	32
4/3/2018	59.390571	17.928387	Non-harmful	1/8/2018	59.389655	17.930143	Fatal	142.7894	85
7/26/2018	59.388157	17.917583	Non-harmful	7/10/2018	59.386211	17.922767	Injurious	365.8007	16
5/30/2018	59.39471	17.904162	Non-harmful	4/15/2018	59.395668	17.902385	Injurious	146.8963	45
4/21/2018	59.392922	17.903902	Non-harmful	3/17/2018	59.394562	17.911096	Injurious	447.741	35
12/29/2020	59.394809	17.900025	Fatal	9/24/2020	59.394501	17.897871	Injurious	127.1194	96
2/17/2019	59.394046	17.903752	Injurious	12/17/2018	59.390316	17.900204	Injurious	461.8655	62
8/16/2021	59.394259	17.912658	Fatal	7/25/2021	59.395176	17.902663	Injurious	576.9947	22
7/26/2018	59.388157	17.917583	Non-harmful	6/1/2018	59.389747	17.923037	Injurious	357.0199	55
5/30/2018	59.39471	17.904162	Non-harmful	3/17/2018	59.394562	17.911096	Injurious	394.3021	74
5/23/2018	59.407311	17.94216	Injurious	3/10/2018	59.40283	17.945393	Injurious	531.8409	74
6/17/2020	59.396941	17.899058	Non-harmful	5/20/2020	59.394545	17.905903	Injurious	471.6959	28
7/31/2021	59.396963	17.899209	Non-harmful	7/25/2021	59.395176	17.902663	Injurious	279.5656	6
4/21/2018	59.392922	17.903902	Non-harmful	4/15/2018	59.395668	17.902385	Injurious	317.8036	6
2/17/2019	59.394046	17.903752	Injurious	1/27/2019	59.395327	17.913506	Injurious	572.2584	21
6/4/2018	59.397451	17.900781	Non-harmful	4/15/2018	59.395668	17.902385	Injurious	218.5531	50
6/7/2018	59.395621	17.904145	Injurious	4/15/2018	59.395668	17.902385	Injurious	100.1309	53
3/9/2020	59.40089	17.930492	Non-harmful	1/9/2020	59.403171	17.938592	Injurious	525.5729	60
9/8/2018	59.390506	17.931887	Non-harmful	6/1/2018	59.389747	17.923037	Injurious	509.9421	99
8/6/2021	59.39477	17.904573	Non-harmful	7/25/2021	59.395176	17.902663	Injurious	117.5334	12
7/1/2018	59.394511	17.90569	Non-harmful	4/15/2018	59.395668	17.902385	Injurious	227.7228	77
12/31/2019	59.392297	17.929972	Injurious	10/7/2019	59.392888	17.925099	Injurious	284.6219	85
8/30/2018	59.387065	17.922859	Injurious	7/10/2018	59.386211	17.922767	Injurious	95.28268	51
9/7/2021	59.39542	17.896986	Injurious	7/25/2021	59.395176	17.902663	Injurious	323.6509	44
7/1/2018	59.394511	17.90569	Non-harmful	6/7/2018	59.395621	17.904145	Injurious	151.642	24
12/29/2020	59.410483	17.927506	Injurious	10/7/2020	59.413373	17.919656	Injurious	549.8693	83
7/10/2018	59.386211	17.922767	Injurious	6/1/2018	59.389747	17.923037	Injurious	394.2878	39
6/7/2018	59.395621	17.904145	Injurious	3/17/2018	59.394562	17.911096	Injurious	412.2121	82
8/30/2018	59.387065	17.922859	Injurious	6/1/2018	59.389747	17.923037	Injurious	299.0202	90
8/3/2019	59.388504	17.927475	Non-harmful	5/26/2019	59.391351	17.932907	Injurious	442.5891	69
8/6/2020	59.397384	17.910772	Injurious	5/20/2020	59.394545	17.905903	Injurious	420.2508	78
8/10/2021	59.396029	17.904238	Non-harmful	7/25/2021	59.395176	17.902663	Injurious	130.5017	16
8/29/2021	59.395398	17.901365	Non-harmful	7/25/2021	59.395176	17.902663	Injurious	77.81101	35
4/15/2018	59.395668	17.902385	Injurious	3/17/2018	59.394562	17.911096	Injurious	510.0553	29
12/26/2018	59.391175	17.902024	Non-harmful	12/17/2018	59.390316	17.900204	Injurious	140.8876	9
7/1/2018	59.394511	17.90569	Non-harmful	4/21/2018	59.392922	17.903902	Non-harmful	204.137	71
8/30/2018	59.387065	17.922859	Injurious	7/26/2018	59.388157	17.917583	Non-harmful	323.5536	35

9/24/2020	59.394501	17.897871	Injurious	6/17/2020	59.396941	17.899058	Non-harmful	280.1266	99
2/16/2019	59.395598	17.902721	Non-harmful	12/26/2018	59.391175	17.902024	Non-harmful	494.2841	52
5/20/2020	59.394545	17.905903	Injurious	3/9/2020	59.392516	17.904259	Non-harmful	244.558	72
8/13/2020	59.389463	17.93043	Fatal	5/24/2020	59.393032	17.924573	Non-harmful	518.5189	81
8/16/2021	59.394259	17.912658	Fatal	8/6/2021	59.39477	17.904573	Non-harmful	462.8972	10
7/10/2021	59.388919	17.907134	Injurious	6/6/2021	59.393896	17.907769	Non-harmful	555.5963	34
7/17/2018	59.410344	17.930131	Injurious	4/30/2018	59.40949	17.924753	Non-harmful	319.9079	78
7/25/2021	59.395176	17.902663	Injurious	6/6/2021	59.393896	17.907769	Non-harmful	323.2504	49
6/4/2018	59.397451	17.900781	Non-harmful	5/30/2018	59.39471	17.904162	Non-harmful	360.7386	5
6/7/2018	59.395621	17.904145	Injurious	5/30/2018	59.39471	17.904162	Non-harmful	101.4925	8
7/1/2018	59.394511	17.90569	Non-harmful	5/30/2018	59.39471	17.904162	Non-harmful	89.56385	32
8/7/2019	59.39175	17.895474	Fatal	7/5/2019	59.388719	17.893303	Non-harmful	359.4014	33
8/29/2021	59.395398	17.901365	Non-harmful	6/6/2021	59.393896	17.907769	Non-harmful	400.5111	84
9/19/2018	59.39236	17.900315	Non-harmful	7/1/2018	59.394511	17.90569	Non-harmful	388.1941	80
6/7/2018	59.395621	17.904145	Injurious	6/4/2018	59.397451	17.900781	Non-harmful	279.426	3
2/17/2019	59.394046	17.903752	Injurious	12/26/2018	59.391175	17.902024	Non-harmful	334.5637	53
12/26/2018	59.391175	17.902024	Non-harmful	9/19/2018	59.39236	17.900315	Non-harmful	163.9281	98
7/1/2018	59.394511	17.90569	Non-harmful	6/4/2018	59.397451	17.900781	Non-harmful	430.1594	27
5/26/2019	59.391351	17.932907	Injurious	2/22/2019	59.38947	17.928205	Non-harmful	339.5287	93
8/10/2021	59.396029	17.904238	Non-harmful	6/6/2021	59.393896	17.907769	Non-harmful	310.9281	65
9/7/2021	59.39542	17.896986	Injurious	8/29/2021	59.395398	17.901365	Non-harmful	248.759	9
9/7/2021	59.39542	17.896986	Injurious	8/6/2021	59.39477	17.904573	Non-harmful	437.0447	32
7/31/2021	59.396963	17.899209	Non-harmful	6/6/2021	59.393896	17.907769	Non-harmful	594.3777	55
6/4/2018	59.397451	17.900781	Non-harmful	4/21/2018	59.392922	17.903902	Non-harmful	534.7902	44
8/29/2021	59.395398	17.901365	Non-harmful	8/6/2021	59.39477	17.904573	Non-harmful	195.2285	23
6/1/2018	59.389747	17.923037	Injurious	4/3/2018	59.390571	17.928387	Non-harmful	317.5223	59
6/18/2019	59.413498	17.918852	Injurious	6/3/2019	59.413685	17.918133	Non-harmful	45.83067	15
9/7/2021	59.39542	17.896986	Injurious	7/31/2021	59.396963	17.899209	Non-harmful	213.285	38
8/6/2021	59.39477	17.904573	Non-harmful	6/6/2021	59.393896	17.907769	Non-harmful	206.0733	61
8/27/2020	59.409768	17.921964	Fatal	6/30/2020	59.408251	17.92175	Non-harmful	169.4903	58
8/6/2021	59.39477	17.904573	Non-harmful	7/31/2021	59.396963	17.899209	Non-harmful	390.5754	6
8/16/2021	59.394259	17.912658	Fatal	6/6/2021	59.393896	17.907769	Non-harmful	280.6865	71
4/1/2019	59.387191	17.920607	Injurious	2/22/2019	59.38947	17.928205	Non-harmful	500.9055	38
7/10/2018	59.386211	17.922767	Injurious	4/3/2018	59.390571	17.928387	Non-harmful	581.337	98
9/7/2021	59.39542	17.896986	Injurious	8/10/2021	59.396029	17.904238	Non-harmful	417.5333	28
4/3/2018	59.390571	17.928387	Non-harmful	2/9/2018	59.389712	17.926584	Non-harmful	140.1882	53
10/7/2020	59.413373	17.919656	Injurious	6/30/2020	59.408251	17.92175	Non-harmful	582.8187	99
8/29/2021	59.395398	17.901365	Non-harmful	7/31/2021	59.396963	17.899209	Non-harmful	213.0539	29
8/16/2021	59.394259	17.912658	Fatal	8/10/2021	59.396029	17.904238	Non-harmful	517.4139	6
7/17/2020	59.390456	17.930243	Non-harmful	5/24/2020	59.393032	17.924573	Non-harmful	431.5354	54
2/17/2019	59.394046	17.903752	Injurious	2/16/2019	59.395598	17.902721	Non-harmful	182.5319	1
5/30/2018	59.39471	17.904162	Non-harmful	4/21/2018	59.392922	17.903902	Non-harmful	199.7777	39
8/13/2020	59.389463	17.93043	Fatal	7/17/2020	59.390456	17.930243	Non-harmful	111.062	27
10/7/2019	59.392888	17.925099	Injurious	8/3/2019	59.388504	17.927475	Non-harmful	506.7053	65
6/3/2019	59.413685	17.918133	Non-harmful	3/1/2019	59.41596	17.921204	Non-harmful	307.6485	94
6/7/2018	59.395621	17.904145	Injurious	4/21/2018	59.392922	17.903902	Non-harmful	301.0323	47
3/1/2019	59.41596	17.921204	Non-harmful	1/27/2019	59.414355	17.925807	Non-harmful	316.7055	33
6/17/2020	59.396941	17.899058	Non-harmful	3/9/2020	59.392516	17.904259	Non-harmful	574.7903	100
2/22/2019	59.38947	17.928205	Non-harmful	12/7/2018	59.392484	17.923266	Non-harmful	437.5926	77
12/7/2018	59.392484	17.923266	Non-harmful	9/8/2018	59.390506	17.931887	Non-harmful	537.1324	90
8/29/2021	59.395398	17.901365	Non-harmful	8/10/2021	59.396029	17.904238	Non-harmful	177.7021	19
8/10/2021	59.396029	17.904238	Non-harmful	8/6/2021	59.39477	17.904573	Non-harmful	141.4485	4
8/10/2021	59.396029	17.904238	Non-harmful	7/31/2021	59.396963	17.899209	Non-harmful	304.1177	10
12/17/2018	59.390316	17.900204	Injurious	9/19/2018	59.39236	17.900315	Non-harmful	227.8831	89

Note: The right-hand side of the table lists initial shootings and is sorted by shooting category. The left-hand side lists follow up shootings.