Do School Shootings Erode Property Values?

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Abstract

We examine how residential property values are impacted by school shootings, which are crimes that have a very low probability of being repeated in the same location. We exploit the exogenous timing of eleven mass shootings that took place between 1998 and 2014, and use a difference-in-differences framework to estimate the causal effect of these school shootings on property values of homes in the affected school attendance area. We find that house prices decline by an average of around 2.4 percent in a four-year period after a mass school shooting. We also find that enrollment falls in the affected school district, suggesting that families subsequently avoid schools in areas that have experienced such events.

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1. Introduction

The United States has more mass shootings than any other developed country[1], and the number of episodes has risen by more than five times in recent years (i.e., from 2011 to 2016).[2] About seven percent of the mass shootings during the period 1998 to 2016 occurred in primary or secondary schools, directly affecting young students and their communities. More recently, the 2021 National Center for Education Statistics report showed that the number of school shootings with casualties at public and private elementary and secondary schools soared to the highest number in the last two decades.[3] These increasing numbers raise concerns about the potential impact of these horrific episodes on the affected communities.

In this paper, we analyze how mass shootings in schools affect the communities around the schools by examining their effects on residential house values and discussing the mechanisms that may explain the effects. The relationship between crime and house prices has been broadly documented as negative and strong.[4] Households might avoid areas with high levels of crime because they want to avoid future victimization and the associated losses. This link appears to be the logical explanation for the relationship between crime and house prices, but it is not the only one. Indeed, crime may have externalities that reduce house prices through some other understudied or unrecognized channels. While mass shootings in schools are highly unlikely to be repeated in the same location, they are unpredictable and exceptionally traumatic, and they directly affect young students. Thus, homeowners and potential homebuyers might avoid areas where a shooting took place not because they fear future victimization, but simply because they do not want their kids to attend the affected school or be associated with traumatic memories of the horrific incident.

[1] https://www.bloomberg.com/graphics/2022-us-gun-violence-world-comparison/, last accessed July 2022.

[2] We follow the Stanford Mass Shooting of America data project in defining mass shootings as gun-related episodes with three or more victims (excluding perpetrators) that do not involve gangs, drugs, or organized crime. The data on the number of episodes are summarized in Appendix Figure A.1.

[3] https://nces.ed.gov/pubs2022/2022092.pdf, last accessed July 2022.

[4] See, for instance, Thaler (1978), Hellman and Naroff (1979), Lynch and Rasmussen (2001), Gibbons (2004), Abadie and Dermisi (2008), J. C. Pope (2008), Linden and Rockoff (2008), Gautier, Siegmann, and Vuuren (2009), Ihlanfeldt and Mayock (2010), D. G. Pope and Pope (2012), and Ratcliffe and von Hinke (2015).

We estimate the causal effect of school shootings on house prices by exploiting the timing of the shooting within a difference-in-differences framework using property-level transaction data. To identify episodes of school shootings, we use data collated by the *Stanford Mass Shooting of America* project and define school shootings as violent episodes that occurred in an elementary, middle, or high school and had three or more victims, excluding the perpetrator. We focus on eleven mass shootings that took place on a school campus during the period 1998 to 2014. The coverage of the data makes our results externally valid, as these events are geographically spread across the country and staggered over a long period of time.

The key empirical challenge is to identify the counterfactual scenario, i.e., how prices would have evolved in the absence of the shooting. Descriptive statistics at the census tract level show differences in levels among observable characteristics between affected and neighboring school districts prior to the school shootings. We reduce these differences by restricting the analysis to properties within one mile of the school attendance boundary. We define properties located less than one mile away from the school attendance boundary and *inside* the attendance area as "treated" and properties located less than one mile away from the attendance boundary but *outside* the attendance area as "controls". The difference-in-differences estimation compares these two groups and takes care of the remaining preexisting differences in *levels* but can yield biased estimates if there are differences in *trends*. Thus, the validity of the design requires that prices would have evolved in parallel between treated and control areas in the absence of the shooting. We examine the validity of the design by estimating a stacked event study analysis for the price effects where each school mass shooting episode has its own treatment and control observations composed of three years before and ten years after the event. The event analysis rules out the existence of any pre-trends in our estimation.

Our results suggest that house prices decline by around 2.4 percent in the four-year period after the shooting episode. We perform robustness checks using alternative counterfactuals such as areas selected using propensity score matching, areas around future school shooting episodes, and areas around the school district boundaries. We obtain qualitatively similar results. We also find some suggestive evidence that prices recover over the long term, and the negative effect of shootings starts to fade away around seven years after the incident. However, we are cautious in interpreting the long-term estimates, as they are driven by fewer episodes.

We then explore mechanisms that explain the decrease in house prices in the affected areas. We first rule out that house prices decrease because of a lower demand in these areas due to a subsequent increase in crimes in these areas. To rule out this channel, we estimate a difference-in-differences model and find no statistical increase in crime rates in cities where the shooting occurred.

Next, we evaluate whether school shootings adversely affect the demand for schools in the affected areas. We find evidence that school shootings decrease school enrollment. These results are in line with recent literature documenting that school shootings cause trauma that devastates victims by lowering test scores, decreasing the enrollment of students in Grade 9, and increasing absenteeism, suicides, accidental deaths, mental health conditions, anti-depressant consumption, and turnover of teachers and teaching staff.[5] These adverse effects might reduce the preference for the school attendance area where a shooting took place. To discern whether the results are driven by homebuyers who value schooling services, we examine the market for houses that have more bedrooms, which serve as a proxy for family-size households (those that are most likely to include children who will attend local schools). The decline is higher for houses with four bedrooms, but the differences in estimates between these houses and those with fewer bedrooms are not statistically different.

Our analysis contributes to two strands of research. First, we contribute to the literature on the effect of crime on house prices by analyzing how crimes with almost zero probability of repetition affect property values. Our research builds on the work of Linden and Rockoff (2008) and J. C. Pope (2008), who analyze how proximity to the home of a registered sex

[5] See Beland and Kim (2016), Nader, Pynoos, Fairbanks, and Frederick (1990), Levine and McKnight (2021), Cabral, Kim, Rossin-Slater, Schnell, and Schwandt (2021), Bharadwaj, Bhuller, Løken, and Wentzel (2021), and Rossin-Slater, Schnell, Schwandt, Trejo, and Uniat (2020).

offender decreases house prices, and the work of Abadie and Dermisi (2008), Gautier et al. (2009), and Ratcliffe and von Hinke (2015), who analyze the effects of terrorism.

Second, we contribute to the literature on the capitalization of school quality into house prices. Existing research shows that house prices respond to local school quality as measured by test scores, value added, level of capital expenditure per pupil, school report cards, the popularity of the school, etc.[6] These papers estimate a lower willingness to pay for housing in neighborhoods in which schools are reputed to be of poor quality.[7] Our paper adds to this literature by analyzing whether the effect of a school shooting is capitalized in house prices.

Our paper complements that of Gourley (2019), who estimates the effect of the Columbine shooting on housing values. Gourley (2019) documents that property values in the Columbine Catchment Area declined by 5.7 percent as compared to other properties in Jefferson County in the first year after the Columbine shooting, with an average effect of 3.1 percent over the longer term (a period of around 11 years after the shooting). While Gourley's data allow him to perform a repeat sales analysis to address the effect of school shootings, our paper contributes by looking at multiple mass shootings in schools and using an alternative estimation strategy. Our work is also complementary to a recent paper by Brodeur and Yousaf (2020), who estimate that mass shootings (of which school shootings are a small subset) negatively affect employment, earnings, and house prices in counties affected by mass shootings relative to other counties.

The rest of the paper is organized as follows. Section 2 describes the data used, and section 3 explains the empirical methodology. We present the empirical results and robustness tests in section 4 and discuss the possible mechanisms for our results in section 5. Finally, we conclude in section 6.

[6] For a summary of this literature, see Gibbons and Machin (2008), Black and Machin (2011), Nguyen-Hoang and Yinger (2011), and Machin (2011).

[7] See Black (1999), Davidoff and Leigh (2008), Fack and Grenet (2010), Gibbons, Machin, and Silva (2013), Agarwal, Rengarajan, Sing, and Yang (2016), and Andreyeva and Patrick (2017), among others.

2. Data

We combine data from two main sources. First, we use arm's-length real estate transaction data from three years before the shooting to 10 years after (i.e., this includes observations from 1995 to 2017) for the school districts that were affected by a mass shooting in a school, and for the neighboring districts.[8] We match the transaction data to the school attendance areas and school districts by using the property's latitude and longitude coordinates. The school attendance and school-district boundary maps come from the National Center for Education Statistics (NCES).[9] We merge these data with assessment records using a unique property identifier for each property to ascertain the characteristics of the house. Both data sets come from Corelogic Inc. and contain information on the transaction price and date of sale, along with the house's geographic coordinates and characteristics like size, age, the number of bathrooms, and the presence of a garage, fireplace, etc.[10]

The CoreLogic database does not include sociodemographic information about homeowners, although it does contain very rich and descriptive information about the characteristics of the house. To present the descriptive statistics of the sociodemographic characteristics where the shooting took place, we use census information at the census tract level prior to the shootings (i.e., we use 1990 census data) merged with the affected and neighboring school districts. We identify the census tract by overlaying the transaction data with the Census Tract shapefile (2010 definition) obtained from the IPUMS National Historic Geographic Information System (Manson, 2020). The census data allow us to characterize our sample and guide our estimation strategy.

[8] We do not have access to data after 2017. Therefore, we are not able to observe prices ten years after the shooting for the episodes that occur after 2007.

[9] We could not access the school attendance and school district boundary maps over time; thus we use the earliest map we could access for a given school. However, most of the maps we use are from after the shooting took place. For some schools, we could not find the school attendance boundary; for those schools, we use the school district boundary instead.

[10] To eliminate outliers, we drop transactions with sales prices in the top and bottom one percent of the distribution for each county. We normalize the sales prices using quarterly Federal Housing Finance Agency House Price Indices for each state to September 2017.

Second, we use data on mass shootings in America from the Stanford Mass Shootings of America data project (courtesy of the Stanford Geospatial Center and Stanford Libraries). The project, which started in 2012 in reaction to the Sandy Hook mass shooting incident in Connecticut, collects data from online media sources. It defines mass shootings as those that involve three or more victims (not necessarily fatalities), excluding the shooter. The shootings do not include those that are related to gangs, drugs, or organized crime. The data set includes the time, date, and location of the shooting, along with the number of victims and fatalities. It also indicates whether the shooting took place at a school.

We consider all mass shootings at schools from 1998 to 2014 that were included in the Stanford Mass Shooting of America data project, except for the Red Lake school shooting, as we do not have good transaction data for that episode. We present the episodes and their dates in Appendix Table A.1. For our main analysis, we exclude the three episodes that took place in 2007: Springwater Trail High School in Gresham, Oregon; Success Tech Academy in Cleveland, Ohio; and South Middle School Football Game in Saginaw, Michigan because the results might be confounded by the financial crisis that occurred around the same time, and we do not have the school attendance boundary maps for these three schools. Including the Success Tech Academy school shooting episode could be particularly problematic because we don't know the school attendance boundary and the school district boundary includes most of the city of Cleveland, which might have witnessed a larger drop in prices than the surrounding areas during the financial crisis. We also exclude the shooting that took place in West Nickel Mines Amish School in Lancaster, Pennsylvania, because the school was a small niche school. It was an Amish one-room schoolhouse in the Old Order Amish community of Nickel Mines, a village in Bart Township, Lancaster County. We discuss the results for these four episodes in Appendix B.

We analyze a time window of three years before and four years after the episode. We use this time period for our main results as we can track most of the episodes for four years after the shooting, but for informative purposes, we present event study estimates up to ten years after the shooting. The results with a longer time window show potential longer-term effects, although they should be analyzed with caution due to the change in sample composition.[11]

Additionally, we use crime data at the city level from City-Data.com, and school-level data on enrollment (measured by membership) and number of teachers (expressed in full time equivalents) from the National Center for Education Statistics (NCES).

3. Empirical Strategy

Individuals choose where to live based on many factors such as school quality, local amenities, proximity to labor markets, house characteristics, etc. This individual sorting usually hinders any potential estimation of the effect of crime on house values. Low-crime areas are expected to have higher demand, and crime is endogenously determined in certain locations. Unobserved characteristics may also play an important role in housing choice and thus may introduce potentially confounding factors into the estimation.

School shootings could be considered isolated exogenous episodes that homeowners and buyers are not able to predict. However, a recent paper by Levine and McKnight (2020) suggests that there are differences in geographic and socioeconomic patterns among different kinds of school shootings, such as indiscriminate shootings, suicides, personal attacks, and crime-related shootings. For instance, indiscriminate shootings are more likely to affect white students, schools in more rural locations, and those where incomes are higher. Nonetheless, homeowners and potential homebuyers – and even the police – lack the ability to predict the timing of a school shooting. The random timing of the shooting enables us to devise an estimation framework free of confounding factors such as individual sorting, at least in the short run.

The key empirical challenge, however, is to find a valid counterfactual distribution – i.e.,

[11] We have four years of post-shooting data for all episodes except the Marysville episode, which took place in 2014. Four events in our data set took place after 2010, and we observe transaction data until 2017. Thus, the point estimates corresponding to five to ten years include only seven episodes.

what would have happened if the shooting had not taken place. Prior literature has shown that the areas affected by shootings might be different from other areas. To see if this is the case in our data set, we present descriptive statistics for all the census tracts in affected and adjacent school districts in columns (1) and (2) of Table 1.[12] Column (1) presents averages across census tracts *inside* affected school attendance area, and column (2) presents averages across census tracts *outside* affected attendance zone but within affected and adjacent school districts. We observe differences in levels between them, which could potentially bias our estimations (Kahn-Lang and Lang, 2020). We reduce these differences in levels by focusing on transactions that are close to the attendance boundary. We present descriptive statistics for census tracts around one mile of the boundary in columns (5) and (6). The p-values of the differences between these two are presented in column (7) and suggest a much more similar sample in terms of levels. For 13 of the 19 variables shown in Table 1, there are no statistical differences (assuming a significance level of 10 percent) between census tracts that are up to one mile inside the attendance boundary and those that are up to one mile outside the boundary. In particular, we do not observe differences in the level of house prices before the shooting.

Motivated by these similarities, we use houses that are up to one mile outside the attendance boundary of a school where a shooting took place as the counterfactual (i.e., the "control" area) for houses that are up to one mile inside the school attendance boundary (i.e., the "treated" area). We implement a difference-in-differences strategy comparing these two areas before and after the school shooting.

Figure 1a describes our strategy using a map for Orange County, North Carolina, where the Orange High School shooting took place in 2006. The different colors represent the distance from the boundary. We use the properties marked in blue inside and outside the border as the treated and control areas, respectively. This strategy estimates the ex-post average price difference between the treated and control areas by taking into account the

[12] In some instances, the school attendance boundary (or the school district boundary) does not encompass the entire census tract, as the boundary crosses through the census tract. When the census tract is split by the boundary, we treat the resulting two areas as separate spatial units.

preexisting differences across locations. Since this estimation strategy includes properties that are physically closer, they are likely to be similar in observed and unobserved characteristics. Note that our estimation strategy is based on an economic intuition: homeowners in treated school attendance boundaries are likely to be affected as their children might attend a school that witnessed a shooting, whereas homeowners in control areas are eligible to enroll their children in a school that is not directly affected by the shooting.

To perform this analysis, we pool the data across the shooting episodes, stacking the 11 episodes and aligning them with the time of the shooting. The equation estimating the effects of school shootings on house prices is as follows:

$$\begin{aligned}\ln Price_{ijt} =& \alpha + \beta\, Affected\, AB * After_{ijt} + \nu\, After_{jt} + \gamma X_{ijt} + \delta_j + \mu_t \\ &+ \sum_{(k=1)}^{14} \phi_t \times 1(episode)_k + \varepsilon_{ijt}, \qquad (1)\end{aligned}$$

where ln *Price* is the log of deflated sale price of house i in census tract j in year-month t.[13] *Affected AB* takes the value of one if the transaction is within the school attendance boundary where a shooting took place, and *After* takes the value of one for the time period after the shooting.[14] The coefficient on the interaction term, β, is the coefficient of interest that captures the effect of school shootings. We include census tract fixed effects, δ_j, to control for the time-invariant characteristics of the neighborhood and year-month fixed effects, μ_t, to control for time trends. We also include observable house characteristics such as log area of the building, log area of land, and distance to shooting, as well as episode-specific time trends, $\phi_t * 1(episode)_k$, to account for time-varying trends across episodes.[15] ε_{ijt} is the error term that

[13] As previously discussed in this section, some census tracts are not entirely within an attendance boundary. If the census tract is split by the attendance boundary, then we treat each portion of the census tract within an attendance boundary (census tract - by attendance boundary) as a different spatial unit. We get similar results when we use census tract fixed effects without this modification. Whenever we refer to census tract, we do take into account whether the census tract is being split by the attendance boundary. Similarly, when we perform our analysis using properties around the school district boundary, we do consider whether the school district boundary is splitting the census tract.

[14] Since we are using a stacked research design, each shooting episode has a treatment area and its own corresponding set of control areas and is assigned a date of shooting. Thus, *After* takes a value of one for both the treated and control areas for the time period after the shooting.

[15] $1(episode)_k$ is a dummy that equals one if the observation corresponds to mass shooting episode k, which is interacted with time to control for differing time trends across the regions.

is clustered at the census tract level. As a robustness check, we further refine this specification to include additional house characteristics such as number of bathrooms, age, a binary variable for whether the house has a fireplace, and a binary variable for whether the house has a garage.

Our identification is unlikely to be substantially affected by potential biases caused by variation in the treatment timing, which affects the staggered difference-in-differences models (Callaway and Sant'Anna, 2020; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021). This is because the analyzed shooting episodes are spatially spread out, so the treated area for a school is unlikely to be a control area for another shooting, and vice versa. However, to overcome any potential issues, we further refine our methodology following Gormley and Matsa (2011). In this analysis, we allow the census tract and month-year fixed effects to vary by episode. In our main specification, we again compare the house prices within a mile inside the attendance boundary with those within a mile outside the boundary. More formally, we estimate:

$$\ln Price_{ijet} = \alpha + \beta\, Affected\, AB * After_{ijet} + \gamma X_{ijet} + \delta_{je} + \mu_{te} + \varepsilon_{ijet}. \tag{2}$$

This equation is similar to Equation (1), but it adds the episode interactions. The interaction term, $Affected\, AB * After_{ijet}$, takes a value of one if transaction i is in census tract j that lies within the school attendance boundary for episode e, and the transaction took place after the shooting. We include census tract-episode fixed effects, δ_{je}, to ensure that we estimate the effect of shooting after controlling for observable and unobservable time-invariant differences between census tracts. We also include month-year-episode fixed effects, μ_{te}, to control for any time trends, and house characteristics, X_{ijet}. Standard errors are clustered at the census tract level.

The stacked difference-in-differences estimation strategy has a significant advantage over traditional two-way fixed effects. By construction, the events happen contemporaneously; thus, this strategy removes any potential concern of negative weights caused by the two-way fixed effect strategy. Moreover, since each panel is an individual treatment, the analysis is unlikely to suffer from any attenuation bias arising from heterogeneous treatment effects.

Our identification strategy relies on the parallel trends assumption, which implies that prices in the treated areas would have evolved similarly to prices in the control areas in the absence of the shooting. Thus, the difference in the post-shooting period can be interpreted as the causal effect of the school shooting. We test for the existence of pre-trends by estimating our difference-in-differences specification using event study analysis that interacts the treatment dummy with the year dummies before and after the shooting. We reject the existence of pre-trends between the treated and counterfactual distributions for our school attendance boundary specification, providing evidence for the causal interpretation of our estimates.

To further reduce the preexisting differences in levels of observable characteristics between treated and control census tracts, we implement a propensity score matching algorithm. We match at the census tract level using as covariates the variables marked with a dagger in Table 1.[16] We provide additional details on the matching algorithm in Appendix C. We present the estimated means in the matched treated and control samples in columns (8) and (9), and the corresponding p-values are in column (10). Most of the differences disappear, allowing us to compute a counterfactual distribution that balances almost perfectly in terms of observable characteristics. We use this matched sample to check the robustness of our main results.

4. Results

4.1. Main Results

Table 2 presents our main results, where we analyze the effect of school shootings on house prices by restricting the analysis to transactions that took place within one mile of the school-attendance boundary (on either side of the boundary) in the three years before and four years after the shooting. Column (1) includes census tract and month-year fixed effects along with log

[16]The variables included in the matching algorithm include average house prices three years, two years and one year before shooting and the variables selected by implementing a lasso estimation using the treatment dummy as outcome variable.

of land area, log of building area, and distance from the shooting. It suggests an average house price decline of 2.5 percent. Column (2) includes the episode-specific time trend to control for varying time trends over different regions of the country, which corresponds to the specification illustrated in Equation (1). Our point estimates are similar to those of the previous specification. To ensure that our results are not driven by observable differences in house characteristics, in Column (3) we include variables such as age, the number of bathrooms, whether the house has a garage, and whether the house has a fireplace. We do not observe a significant change in the point estimates, although the sample is smaller due to missing values of house characteristics.

In column (4), we present the difference-in-differences results estimated using Equation (2). This is our main specification. The results suggest that house prices decline by 2.4 percent in the school attendance area where a shooting took place. In column (5), we again include additional house characteristics as controls, and we observe a qualitatively similar decline.[17]

In column (6), we estimate the medium- and long-term effects of school shootings using our entire sample of 10 years after the shooting. Please note that this sample is different from the ones used in the first five columns of the table. We find that house prices decline by an average of 2.4 percent in the four years after the shooting and by 2.5 percent between four and seven years after the shooting. The point estimate declines to 1.3 percent in the longer term (from seven to ten years after the shooting), though the latter coefficient is no longer significant. We are cautious in interpreting our long-term results, as they are driven by fewer episodes.

Our results are likely to underestimate the true effects, as in some instances, the schools themselves changed after the incidents occurred. For instance, after the Sandy Hook shooting, the school was demolished, and a new one was built.[18] Similarly, four months after the

[17]The results for the four additional episodes that are not included in the main analysis are presented in Appendix B. The event analysis graph for these four episodes, Appendix Figure B.1a, indicates that the price trends in treated areas were very different from those in the control areas before the shooting. These findings suggest that some other factor might have been differentially affecting the house prices in these areas, even before the shooting took place. Thus, we use a matched sample for estimating the effects. They suggest a decline in house prices after the school shootings, but they are not statistically different from zero in most specifications.

[18]https://www.history.com/this-day-in-history/gunman-kills-students-and-adults-at-newtown-connecticut-elementary-school, last accessed July 2022.

shooting at Columbine High School, the school reopened after spending more than $1.2 million on building repairs and renovations such as changing the sound of the alarm, demolishing the library, and replacing it with an atrium and a memorial.[19] These changes could have a mitigating bias on the coefficient, as demolishing the building and having a new structure might reduce the associated trauma to some extent, and thus the true effect of such school shooting episodes might be even larger.

4.2. Event Study Analysis

We examine the validity of the design by estimating our main specification but interacting the treatment dummy with year indicators. We leverage the length of our transaction data set to estimate the dynamic effects three years before and ten years after the shooting occurred. Recall that the estimates from five to ten years after the shooting should be analyzed with caution because they are estimated using fewer shooting episodes. We present the event study estimates for school attendance boundary in Figure 2. The figure plots the point estimates from a modified version of Equation 2 where we allow the effect of the treatment to vary by year.

In the event study graph using the data around a mile from the school attendance boundary, Figure 2, we find no indication of a difference in price trends before the school shooting. However, after the event, the areas affected by the shooting witnessed a decline in prices in the first few years after the shooting.[20] There is some suggestive evidence that the estimated decline in house prices starts to fade away around seven years after the shooting, which could be because the community is able to recover from its traumatic experiences in the long term or/and the affected cohorts are out of school. The latter channel is in line with the findings of Levine and McKnight (2021), who show that test scores rebound over time. Our results are also robust to the choice of a different window for analysis. Figure A.2 in the appendix shows results for a window of nine years prior to ten years after the event.

[19] https://www.latimes.com/nation/la-na-mass-shooting-locations-20160804-snap-story.html, last accessed July 2022.

[20] The point estimates of the event study analysis are also presented in Appendix Table A.2.

4.3. Robustness: Alternative Counterfactuals

We explore alternative counterfactuals to validate the robustness of the effect of school shootings on house prices. In particular, we use (i) a propensity score matching approach, (ii) future episodes as controls, and (iii) school district boundaries to define treated and control areas.

4.3.1 Propensity Score Matching

Column (1) of Table 3 presents the estimates of our specification using the matched sample of census tracts obtained using a propensity score matching algorithm that selects kernel weights that decrease observable differences across characteristics (for more details, see Appendix C). This sample corresponds to the one described in columns (8) and (9) of Table 1. The estimations are done at the transaction level, even though the matching is performed at the census tract level. The estimates using transactions in the one-mile radius around the school attendance boundary suggest a 2.4 percent decline in house prices. The results are similar when we include additional house characteristics in column (2). The event study estimates for the matched sample are presented in the Appendix Figure A.3a.

Our matching results could be driven by the choice of a specific bandwidth.[21] Therefore, we conduct alternative estimations varying the bandwidths and present the results in Appendix Table A.3. Smaller bandwidths imply stricter matching conditions that further reduce the observable differences across treated and control census tracts, while larger bandwidths result more relaxed conditions in the matching procedure. We observe consistently negative point estimates across all of the different specifications that range between 1.9 percent and 2.3 percent. These results provide evidence that our results are robust to alternative bandwidths.

[21] We choose the optimal bandwidth for the kernel using the method described in Huber, Lechner, and Steinmayr (2015); Huber, Lechner, and Wunsch (2013).

4.3.2 Using Future Episodes as Controls

Even if there are differences in observable and unobservable characteristics between the treated and control areas, the timing of the shooting is likely to be random. Thus, we use the areas affected by future school shootings as controls and exploit the staggered nature of these events. To exploit this variation, we restrict the time period to transactions that took place prior to 2012, were a mile inside the school attendance boundary, and were affected by a school shooting. The areas where a shooting took place before 2012 are considered "treated", while those where a shooting took place in the year 2012 or after are considered "control". Thus, we compare treated areas with those that will be affected by a shooting in the future but have not yet been affected.

The coefficients for this strategy are estimated using an equation similar to Equation 1[22], and the results appear in columns (3) and (4) of Table 3. The estimates suggest a 3.0 percent decline in house prices (column 3), and the results remain robust when we include additional house characteristics (column 4). These results are similar in magnitude to the base results presented in Table 2.

4.3.3 Using School District Boundaries

Although the school attendance area is a better geographical unit of analysis for our paper, there might be some problems with using the attendance boundary. For instance, school attendance boundaries change often and are not available for all the schools in our sample, in which case we use school district boundaries. Therefore, to be consistent, we use the school district boundary as a robustness strategy. We follow the same strategy as explained above but compare transactions on either side of the boundary. Figure 1b describes this alternative strategy.[23]

[22]For this analysis, we exclude the After Shooting variable in Equation 1 as each treated sample does not have its own set of controls.

[23]The advantage of using school district boundaries is that the schools within the district are subject to the same policies and regulations, and boundaries are less subject to changes over time. For more information on the advantages of using school district boundaries, see Dhar and Ross (2012).

We estimate Equation (2) using transactions that took place within a mile (on either side) of the school district boundary. The results using this sample are presented in columns (5) to (7) of Table 3. The difference-in-differences estimate suggests a 4.1 percent decrease in house prices. This estimation does not satisfy the parallel trends assumption (the event study estimates are presented in Appendix Figure A.3b), so we use propensity score matching to eliminate the differences in the pre-trends. The estimate using the matched sample suggests a 2.0 percent decline in house prices, as shown in column (6). Next, we include additional house characteristics in column (7), and the magnitude of the point estimate declines, remaining negative but not statistically different from zero.

4.4. Additional Robustness Tests

Two other potential concerns could affect our estimations. First, our results could be driven by a few episodes, which would decrease the external validity of our study. To rule this out, we estimate our model dropping each episode one at a time, and we present the results in Table 4. We find that all the point estimates remain negative and significant, which allows us to conclude that the observed negative effects are not driven by a particular episode.

Second, our results could be sensitive to our choice of using properties that are within a one-mile radius of the school attendance boundary. We check the sensitivity of our results by varying the distance from the school attendance boundary from 0.2 miles to 1.4 miles. We re-estimate Equation (2) using the different distances from the boundary and present the results in Appendix Table A.4. The point estimates continue to remain negative regardless of our choice of radius, and they are statistically significant when the sample size increases in the 0.8- to 1.4-mile range.

These robustness checks provide further evidence of the validity of our findings. We observe a decrease in house prices after a school shooting, and this effect is not driven by the choice of our estimation sample.

5. Mechanisms

The relationship between crime and property values may occur through different potential channels. Perhaps the most prominent channel is that housing demand in high-crime areas is low because individuals do not want to personally experience crime. However, the probability that an area that witnessed a mass school shooting will experience another mass shooting is very low; in fact, it is no different from the probability that any other school will experience a first mass school shooting.

It could be that school shootings are associated with a simultaneous increase in other types of crime, which leads to a decline in house prices. We test for this by estimating the effect of school shootings on crime within a difference-in-differences model using yearly crime rates at the city level from 2002 to 2017. We use cities where the school shooting took place as treated units and the nearest cities as controls.[24] The results are presented in Table 5. We analyze the effects of mass shootings on murders, rapes, robberies, assaults, burglaries, thefts, auto thefts, arson, and a combined crime index provided by City-Data.com. The first eight columns use the number of crimes per 100,000 as the dependent variable, and the last column is a crime index that weighs serious and violent crimes more heavily.

For this analysis, we again pool the data across episodes, stacking the episodes and aligning them with the time of shooting.[25] The estimating equation is as follows:

$$Crime\ Rate_{ct} = \alpha + \beta\, Affected\ City * After_{ct} + \nu\, After_t + \delta_c + \mu_t + \varepsilon_{it},$$

where *Crime Rate* is the number of crimes per 100,000 people in city c in year t. *Affected City*

[24]This analysis does not cover all of the eleven episodes, but only episodes were we had data for both before and after the school shooting. It includes only the cities where data were available. The other drawback of this data is that in some instances, the school lies outside the city borders depicted in the maps shown on the City-Data.com website. The nearest cities are chosen based on the list provided by City-Data.com.

[25]To be consistent with the time frame used in our main price analysis, we restrict our data to three years before the shooting took place and four years after the shooting, where year one in the post-shooting period refers to the year in which the shooting took place. We use a similar time frame for the analysis of school enrollment and number of teachers.

takes the value of one if city c is where a shooting took place, and *After* takes the value of one for the time period after the shooting.[26] The coefficient on the interaction term, β, measures whether crime increased in cities where shootings took place. We include city fixed effects, δ_c, to control for the time-invariant characteristics of the city and year fixed effects, μ_t, to control for time trends. The results using this estimating equation are presented in Panel A of Table 5. The estimates suggest that there has not been a statistical increase in crime.

We refine our strategy to allow the time fixed effects to vary by episode (denoted by e) by including city-episode, δ_{ce}, and year-episode, μ_{te}, fixed effects as we would expect the crime trends to vary across the different regions. This strategy is also similar to the one we used for price analysis. The modified estimation equation is as follows:

$$Crime\ Rate_{cet} = \alpha + \beta Affected\ City * After_{cet} + \delta_{ce} + \mu_{te} + \varepsilon_{cet}.$$

The results of this analysis are presented in Panel B of of Table 5. We do not find evidence of a statistical increase in crime across various categories, which suggests that crime (i.e., a higher probability of repetition) is unlikely to be the reason for the decline in house prices.

Alternatively, it could be that social stigma or associated trauma drives house prices down after a school shooting, as parents of affected students might want to move out. The shooting incidents we focus on in this paper had three or more victims and likely received media attention. This might discourage potential homebuyers who might avoid the area because they do not want their children to attend or be associated with these schools.

In fact, studies have shown that shootings can have severe consequences on mental health. Lowe and Galea (2017) do a meta-analysis of 49 peer-reviewed articles on the mental consequences of mass shootings and conclude that these incidents are associated with various adverse psychological outcomes in survivors and the affected communities. In addition, Daly et al.

[26]Since we are using a stacked research design, each shooting episode has a treatment city and its own corresponding set of control cities and is assigned a date of shooting. Thus, as in the price analysis, *After* takes a value of one for both the treated and control areas for the time period after the shooting.

(2008) show that mass casualty incidents can trigger post-traumatic stress disorder symptoms and increase suicides at the one-year anniversary of the traumatic event.[27]

Moreover, recent literature has documented that school shootings have long-lasting effects among students in affected schools, which may create strong incentives for parents to move out. Cabral et al. (2021) show that students exposed to shootings have a higher rate of absenteeism, are more likely to repeat a grade, are less likely to graduate high school and college, and have decreased employment and earnings at 24-26 years of age. Beland and Kim (2016) find that fatal shootings in high schools result in lower school enrollment in grade 9 and lower test scores, and Levine and McKnight (2021) find that exposure to school shootings leads to lower test scores, an increase in chronic absenteeism (an absence rate of greater than 10 percent), and increases in suicides and accidental deaths. Moreover, Rossin-Slater et al. (2020) find that antidepressant consumption increases in the areas surrounding the affected schools.

We complement these results by testing whether the mass school shootings analyzed herein result in a decrease in the number of students (measured by membership) and teachers (full-time equivalents) in the affected school districts. A decrease in demand for the schools in the affected districts would imply that students and teachers leave after the shooting, or that prospective students and teachers are reluctant to join the school in the affected area. Recent work suggests that the effects of school shootings might spill over to the neighboring schools that were not directly affected by the school shooting but were within the same school district. Levine and McKnight (2021) find that after the Sandy Hook shooting, test scores also fell at other elementary schools in Newton, suggesting a broader impact within the school district.

To analyze the effect on enrollment and the number of teachers, we compare schools in the school district where the shooting took place with schools in the adjacent district in a difference-in-differences setting.[28] We use Poisson regression to estimate the model. The

[27] Two students at the Stoneman Douglas High School committed suicide around the one-year anniversary date of the shooting that took place at the school.

[28] We drop the schools for which we do not have even a single year of data for the pre-shooting period. We also drop schools for which the average enrollment in the pre-shooting period is below 1 percent and above 99 percent

controls included are similar to the one used for the crime analysis, except that school is the unit of observation for this analysis and thus we include school and year fixed effects. The results are summarized in Table 6.

We first compare all schools in an affected school district (i.e., those where the shooting took place and those in the same school district that did not experience any shooting) with the schools in neighboring districts, and we present the results in columns (1) and (2). We observe an overall decrease of 1.7 percent in school enrollment, but we cannot claim that the number of teachers decreased, although the point estimate is equivalent to a roughly one percent drop as the latter estimate is statistically insignificant.

Next, we modify the treated set to include schools within the affected school attendance boundary, which includes schools that were located within the boundary but were not the site of the shooting .[29] The results, presented in columns (3) and (4), suggest that the enrollment of the schools within the directly affected attendance boundary declines. Our main specification suggests an average enrollment decline of 2.8 percent in the schools located within the affected attendance boundary.

This result is similar to the findings of other researchers like Beland and Kim (2016) and Levine and McKnight (2021), who also find a decline in enrollment. Our imprecise result on the effect on the number of teachers is complementary to the work of Cabral et al. (2021). Although Cabral et al. (2021) do not find a change in the number of full-time equivalent teachers, they do find a reduction in the probability of retention for these groups. This can be indicative of high turnover at the school after the shooting. They do not find a significant change in teachers and teaching staff, but they do find an increase in the number of full-time equivalent leadership staff following a school shooting. The increase in leadership staff is driven primarily by an increase in assistant principals, who are typically responsible for dealing with safety and disciplinary

of the average enrollment of the schools in that episode.

[29]The sample of schools used for this analysis is very similar to that of the previous analysis. It excludes only the schools for which we do not have the geographic coordinates, as we could not be sure whether the school was inside or outside the attendance boundary of the affected school.

issues. This is again consistent with the notion that schools take steps to improve safety after a shooting on their grounds.

Another proxy for school quality could be the school's expenditure on various services. Levine and McKnight (2021) find that all school shootings result in higher spending on school services (counseling and security as well as other services). Shootings with fatalities lead to an increase in all forms of school spending, including instructional spending and a large increase in support services. Anecdotal evidence also suggests that expenditure on safety measures increases after a school shooting. According to a media article, shortly after the Sandy Hook shooting, the state put in place an infrastructure grant program and a school emergency plan requirement for schools and school districts.[30] According to NCES, between 1999-2000 and 2015-16, there was a substantial increase in the percentage of schools with security measures such as controlled access to buildings during school hours, the requirement that faculty and staff wear badges or picture IDs, and the use of security cameras to monitor schools.[31] These measures are likely to have some counteracting effect, as some parents might feel safer sending their children to school after the enhanced security measures. Our results indicate that despite this additional spending, which may counteract some of the effects of shootings, there is a decline in prices. This suggests that the true effect of the mass shootings could even be larger than what our results suggest.

If schooling demand is a key driver of the house price drop, then our results could be driven by families with school-age children. They are the type of home-buyers who care the most about schooling amenities. Unfortunately, we cannot directly observe which families have children, but we can use the number of bedrooms in a house as a proxy for family size, since families with children are likely to have houses with more bedrooms. We present the results in Table 7, where we observe that the effect is smaller in magnitude and imprecise for one-bedroom and two-bedroom properties, whereas it is negative and significant for properties with

[30]https://www.theday.com/article/20220529/NWS01/220529513, last accessed July 2022.

[31]https://www.educationnext.org/wp-content/uploads/2022/01/ednext_XIX_2_warnick_kapa.pdf, last accessed July 2022.

four or more bedrooms. However, we cannot conclude that there is a differential effect by the number of bedrooms, as the differences between the coefficients for one to two bedrooms, three bedrooms, and four or more bedrooms are statistically insignificant.

Putting together these results, we find suggestive evidence that shootings decrease households' demand for schools within the affected areas. Current residents may decide to relocate because they don't want their children to attend schools where such traumatic episodes occurred; for the same reason, potential homebuyers might want to avoid the area.

6. Conclusion

In this paper, we use property transaction data to estimate the effect of school shootings on property values in the United States. We exploit the exogenous timing of the shootings to implement a differences-in-differences strategy and restrict the analysis to properties that are close to the school attendance boundary. We find that house prices decrease by an average of around 2.4 percent in the four-year period after a school shooting. We perform an event study analysis that allows us to rule out the existence of pre-trends and reveals that the effect starts to fade away around seven years after the shooting took place. Our results are robust to the use of alternative samples and matching procedures. Our estimates are similar to those of a contemporaneous study, Brodeur and Yousaf (2020), that uses all mass shootings and finds a 1.6 percent decline in house prices in the affected county. Our results are smaller in magnitude than the recent finding of Gourley (2019), who analyzes the effect of the Columbine shooting and finds a 5.7 percent decline in house prices in the immediate aftermath of the shooting.

We also find evidence of a decline in school enrollment following a mass shooting in school. This result contributes to recent literature documenting that exposure to school shootings might lead to adverse outcomes such as decreased test scores, higher absenteeism rates, lower graduation rates, lower employment rates, etc. Thus, school shootings are likely to discourage residents from enrolling their kids in the area's schools and might dissuade potential homebuyers from moving to the area probably, due to the trauma associated with the school shooting.

The magnitudes we estimate are similar to previous estimates of the effects of schooling outcomes on property values. For instance, Black (1999) estimates a 2.5 percent increase in housing values for a five percent increase in test school scores, whereas Gibbons et al. (2013) estimates a three percent increase in prices for an increase of one standard deviation in average value added. Our estimates are smaller than the disamenity found by Linden and Rockoff (2008), who examine the effect on house prices of close proximity to the residence of a registered sex offender (a decline of 11.6 percent). Our results are also smaller in magnitude than the effects on house prices that stem from the discovery of a cancer cluster of child leukemia (a 14 percent decline in value) (Davis, 2004), and the temporary, one-year effect of getting a school quality rating of "A" rather than "B" (20 percent) found by Figlio and Lucas (2004).

Overall, our results suggest that households prefer to reside in areas with schools they highly value. We provide evidence that crime affects property values not only because people fear being victimized, but also via other channels. Incidents such as school shootings also lead to a decline in house prices due to the trauma associated with the incident. Future research is needed to understand how to deal with locations affected by crime shocks, particularly school-related crime shocks.

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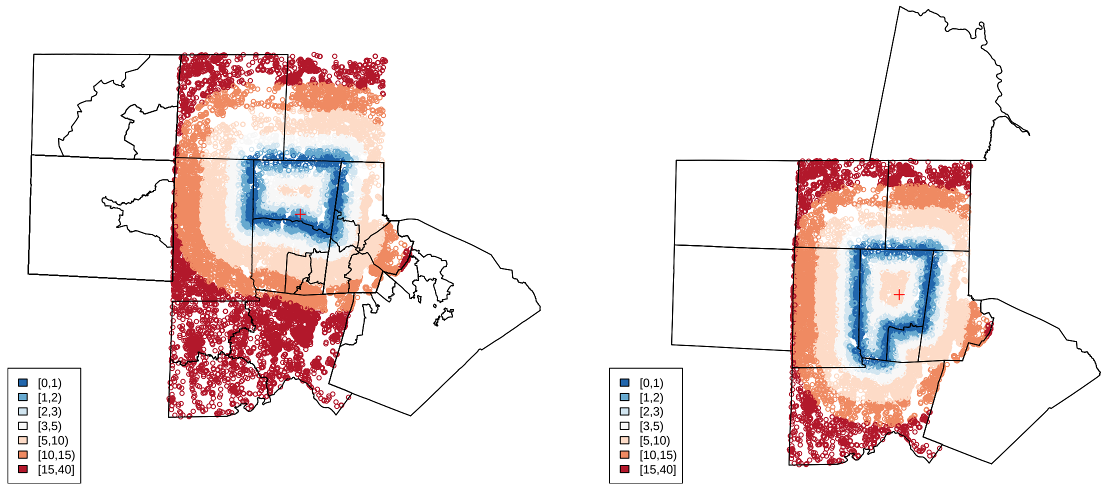
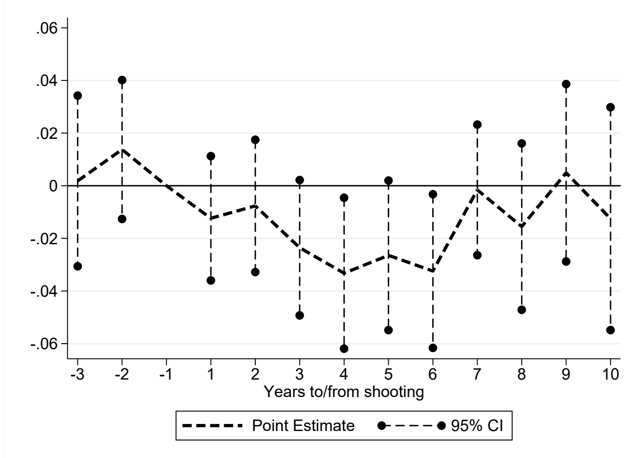
Figure 1
Description of Empirical Strategy

(a) School Attendance **(b)** School District



Note: Figure 1a plots the school attendance boundary for Orange High School in North Carolina and the neighboring attendance boundaries. Figure 1b plots the school district boundary in which Orange High School is located and the adjoining school district boundaries. The red cross indicates the location of Orange High School, where a shooting took place in 2006. The different colors represent distances from the school attendance or school district boundary in which Orange High School is located.

Figure 2
Event Study Analysis Using Attendance Boundaries



Notes: The figure plots event study estimates of the effect of school shootings on log prices using the specification of column 3 in Table 2. The estimation includes month-year-episode and census tract-episode fixed effects. We also control for log of land area, log of building area, and the distance to the shooting. Standard errors are clustered at the census tract level.

Table 1

Summary Statistics at the Census Tract Level

	All Census Tracts				Around One Mile of Attendance Boundary					
	Affected and Adjacent SDs			P-value	Unmatched			Matched		
	Inside AB	Outside AB	Rest	(1)-(2)	Treated	Control	P-Value	Treated	Control	P-value
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ln(Population)	8.04	7.93	7.96	0.22	8.03	7.87	0.26	8.11	7.81	0.02
ln(Median Home Value)†	11.44	11.58	11.29	0.01	11.44	11.47	0.54	11.47	11.47	0.97
ln(Median Rent)	6.04	6.13	5.86	0.04	6.04	6.04	0.99	6.03	6.02	0.84
Share College Graduates	0.10	0.17	0.13	0.00	0.10	0.13	0.02	0.12	0.12	0.55
Perc. of Old Houses†	0.19	0.26	0.40	0.01	0.19	0.23	0.09	0.21	0.22	0.69
Percentage Married	0.47	0.44	0.43	0.00	0.47	0.47	0.65	0.47	0.47	0.77
Unemployment Rate†	0.05	0.05	0.07	0.44	0.05	0.05	0.61	0.05	0.05	0.91
Perc. Moved in $<$ 10 Years	0.24	0.27	0.24	0.00	0.23	0.24	0.18	0.23	0.24	0.05
Labor Force Part.†	0.51	0.54	0.50	0.00	0.52	0.53	0.06	0.53	0.52	0.73
Percentage Hispanic†	0.03	0.07	0.07	0.00	0.03	0.03	0.90	0.03	0.03	0.98
Percentage Black†	0.02	0.10	0.11	0.00	0.02	0.05	0.09	0.04	0.04	0.92
Self-Employment Share	0.08	0.08	0.08	0.62	0.08	0.08	0.85	0.08	0.08	0.37
Manufacturing Share†	0.21	0.15	0.17	0.00	0.21	0.19	0.05	0.20	0.20	0.80
ln(Price)$_{t-3}{}^{\dagger}$	7.89	7.99	.	0.18	7.89	7.86	0.62	7.86	7.84	0.76
ln(Price)$_{t-2}{}^{\dagger}$	7.90	7.99	.	0.23	7.91	7.84	0.37	7.89	7.86	0.71
ln(Price)$_{t-1}{}^{\dagger}$	7.89	8.01	.	0.09	7.89	7.89	0.96	7.89	7.86	0.70
ln(Sales)$_{t-3}$	2.48	2.99	.	0.00	2.46	2.80	0.09	2.78	2.86	0.67
ln(Sales)$_{t-2}$	2.74	3.06	.	0.04	2.72	2.97	0.18	2.87	3.14	0.11
ln(Sales)$_{t-1}$	2.77	3.08	.	0.04	2.72	3.00	0.15	3.00	3.15	0.39
Number of Observations	84	2,227	70,268		80	177		65	149	

Note: The table presents mean differences of the variables among affected and adjacent census tracts-by-school attendance zones using the 1990 census. SD stands for school districts, whereas AB stands for attendance boundary. The displayed number of observations corresponds to that in the census data, which implies that the number of observations used in the rows with prices and sales (coming from CoreLogic) can be smaller because of missing values. t-1, t-2, and t-3 refers to one year, two years and three years before the shooting. Column (1) presents averages across all census tracts-by-attendance zones in affected attendance zones. Column (2) presents averages across census tracts-by-attendance zones that are within affected and adjacent school districts but are not within the directly affected attendance zone. Column (3) corresponds to the average across all the census tracts in the US in 1990, excluding the treated and adjacent census tracts-by-attendance zones. Column (4) presents the p-values of the differences in means between columns (1) and (2). Columns (5) and (6) present averages among census tracts-by-attendance zones (we create separate spatial units if the attendance boundary splits the census tract) located one mile around the school attendance boundary. Column (7) presents the p-value of the difference between columns (5) and (6). Columns (8) and (9) present averages reweighted by kernel weights computed using a propensity score matching algorithm. The variables included in the matching algorithm are covariates selected using a lasso estimation and log of average prices in t-1, t-2, and t-3, and are displayed with a †. Details of the matching procedure are discussed in Appendix C. Column (10) presents the p-values of the differences in means between columns (8) and (9).

Table 2

Effect of School Shootings on House Prices

	(1)	(2)	(3)	(4)	(5)	(6)
1(Affected AB)*1(After)	-0.025**	-0.025***	-0.023***	-0.024**	-0.021***	
	(0.011)	(0.010)	(0.008)	(0.009)	(0.007)	
1(Affected AB)*1(First 4 Years)						-0.024**
						(0.009)
1(Affected AB)*1(4 to 7 Years)						-0.025**
						(0.011)
1(Affected AB)*1(7 to 10 Years)						-0.013
						(0.016)
Observations	36,920	36,920	34,808	36,920	34,808	63,502
Census Tract FE	Yes	Yes	Yes			
Month-Year FE	Yes	Yes	Yes			
Basic Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Episode Specific Trend		Yes	Yes			
House Characteristics			Yes		Yes	
Month-Year-Episode FE				Yes	Yes	Yes
Census Tract-Episode FE				Yes	Yes	Yes

Note: The dependent variable is the log of house price. Affected AB takes a value of one if the property is within the affected attendance boundary. Columns (1) to (5) include observations between three years before and four years after the shooting. Column (6) uses observations between three years before and ten years after the shooting and splits the effect between the first four years, four to seven years, and seven to ten years after the episode. Basic characteristics include the log of land area, the log of building area, and the distance to the shooting. House characteristics include a dummy variable for whether the house has a fireplace, a dummy variable for whether the house has a garage, the age of the property, and the number of bathrooms. Standard errors are clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3
Alternative Counterfactuals

	Matched Sample		Future Episodes		School District Boundary		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1(Affected AB)*1(After)	-0.024***	-0.021***	-0.030**	-0.023*			
	(0.009)	(0.007)	(0.013)	(0.012)			
1(Affected SD)*1(After)					-0.041***	-0.020*	-0.014
					(0.010)	(0.012)	(0.009)
Observations	35,355	33,419	32,837	31,029	64,762	60,036	57,304
Basic Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year-Episode FE	Yes	Yes			Yes	Yes	Yes
Census Tract-Episode FE	Yes	Yes			Yes	Yes	Yes
House Characteristics		Yes		Yes			Yes
Census Tract FE			Yes	Yes			
Month-Year FE			Yes	Yes			
Episode Specific Trend			Yes	Yes			
Matched Sample						Yes	Yes

Note: The dependent variable is the log of house price. Affected AB takes a value of one if the property is within the affected attendance boundary, and affected SD takes a value of one if the property is in the affected school district. Columns (1) and (2) show the estimates reweighting by kernel weights obtained from matching. Details of matching procedure are discussed in Appendix C. Columns (3) and (4) include only observations within a mile of the affected school attendance area and transactions that took place between 1995 to 2011, such that school attendance areas that were affected in the future (i.e., areas where the shooting took place after 2011) are used as controls for the episodes that took place before 2012. Columns (5) – (7) use the school district boundary instead of the school attendance boundary. Basic characteristics include the log of land area, the log of building area, and the distance to the shooting. House characteristics include a dummy variable for whether the house has a fireplace, a dummy variable for whether the house has a garage, the age of the property, and the number of bathrooms. Standard errors are clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4

Robustness of the Effect of School Shootings on House Prices to Dropping One Episode at a Time

	Chardon (1)	Columbine (2)	Conyers (3)	Fort Gibson (4)	Hillsborough (5)	Jonesboro (6)	Marysville (7)	Newton (8)	Santee (9)	Sparks (10)	Springfield (11)
1(Affected AB)*1(After)	-0.023**	-0.029***	-0.025**	-0.022**	-0.028***	-0.024**	-0.026**	-0.024**	-0.019*	-0.024***	-0.019*
	(0.009)	(0.010)	(0.010)	(0.009)	(0.010)	(0.009)	(0.010)	(0.010)	(0.011)	(0.009)	(0.010)
Observations	36,291	27,761	32,528	36,452	34,086	36,702	32,918	35,861	30,498	36,290	29,813
Month-Year-Episode FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census Tract-Episode FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Basic Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The dependent variable is the log of house price. AB stands for attendance boundary. Basic characteristics include the log of land area, the log of building area, and the distance to the shooting. Standard errors are clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5
Effect of School Shootings on Crime Rates

	Crime Rates								Index
	Murders	Rapes	Robberies	Assaults	Burglaries	Thefts	Auto Thefts	Arson	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A) Year and City Fixed Effects									
1(Affected City)*1(After)	3.755	10.569	4.057	-33.354	-112.485	178.465	17.967	-0.982	11.729
	(3.097)	(6.391)	(4.911)	(36.987)	(91.265)	(159.372)	(15.354)	(3.657)	(8.161)
Observations	246	243	243	243	243	243	243	241	243
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
B) Fixed Effects by Episode									
1(Affected City)*1(After)	3.791	9.923	5.015	-30.897	-110.451	159.957	11.853	-1.086	10.996
	(2.883)	(6.972)	(4.805)	(41.589)	(96.532)	(158.317)	(14.159)	(3.508)	(7.251)
Observations	246	243	243	243	243	243	243	241	243
Year-Episode FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-Episode FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The dependent variables is the number of crimes per 100,000, except in column (9), which uses a crime index. Standard errors are clustered at the city level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6
Effect of School Shootings on Enrollment and Number of Teachers

	(1)	(2)	(3)	(4)
A) Enrollment				
1(Affected SD)*1(After)	-0.019**	-0.017*		
	(0.009)	(0.010)		
1(Affected AB)*1(After)			-0.035**	-0.028*
			(0.017)	(0.016)
Observations	11,576	11,576	11,452	11,452
B) Number of Teachers				
1(Affected SD)*1(After)	-0.013	-0.011		
	(0.011)	(0.012)		
1(Affected AB)*1(After)			-0.022	-0.022
			(0.017)	(0.016)
Observations	11,314	11,314	11,206	11,206
Year FE	Yes	Yes	Yes	Yes
School FE	Yes		Yes	
Year-Episode FE		Yes		Yes
School-Episode FE		Yes		Yes

Note: The model is estimated using Poisson regressions. The dependent variable in Panel A is the total count of students at a school, and the dependent variable in Panel B is the count of teachers. Affected SD takes a value of 1 if the school is within the school district where the shooting took place. Affected AB takes a value of 1 if the school is within the attendance boundary where the shooting took place. Standard errors are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Table 7
Effect of School Shooting on House Prices by Number of Bedrooms

	Number of Bedrooms		
	One and Two	Three	Four or More
	(1)	(2)	(3)
1(Affected AB)*1(After)	-0.009	-0.016	-0.031*
	(0.030)	(0.010)	(0.016)
Observations	4,329	21,022	11,052
Basic Characteristics	Yes	Yes	Yes
Month-Year-Episode FE	Yes	Yes	Yes
Census Tract-Episode FE	Yes	Yes	Yes
P-value with respect to one and two BR	-	0.846	0.566

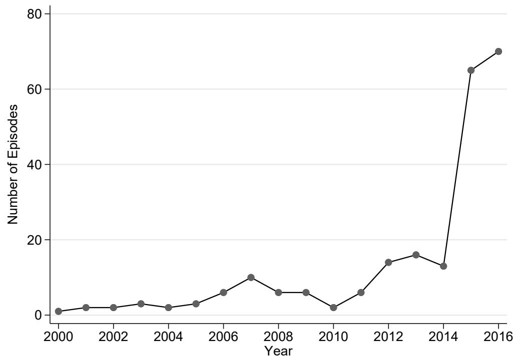
Note: Each column estimates the main model using a subsample of houses with a different number of bedroom. The dependent variable is the log of house price. AB stands for attendance boundary. Basic characteristics include the log of land area, the log of building area, and the distance to the shooting. Standard errors are clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1.

Online Appendix to
Do School Shootings Erode Property Values?

Juan S. Muñoz-Morales   Ruchi Singh

A. Appendix Figures and Tables

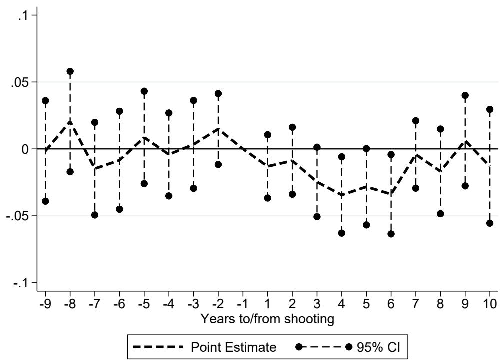
Appendix Figure A.1
Number of Mass Shootings 2000-2016



Notes: This figure uses data from the Stanford Mass Shootings of America (MSA) project (courtesy of the Stanford Geospatial Center and Stanford Libraries).

Appendix Figure A.2

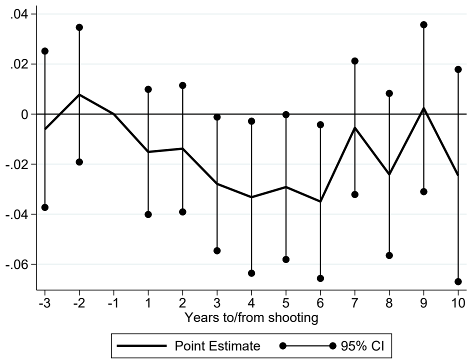
Event Study Estimates: Effects of School Shootings on House Prices



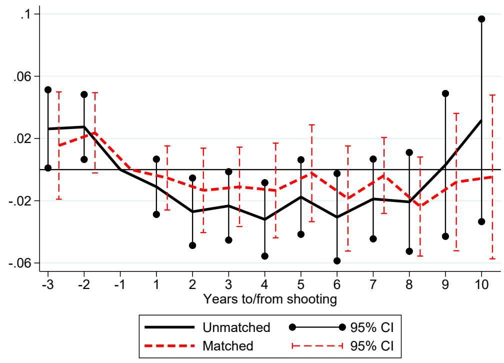
Notes: The figure plots event study estimates of the effect of school shootings on log prices using the specification of column 3 in Table 2 but including a longer time frame from nine years before to ten years after the shooting. The estimation includes year-month-episode and census tract-episode fixed effects. We also control for log of land area, log of building area, and the distance to the shooting. Standard errors are clustered at the census tract level.

Appendix Figure A.3
Event Study Estimates Using the Matching Procedure

(a) School Attendance Boundary: Matched Sample



(b) School District Boundary



Notes: The figures plot event study estimates of the effect of school shootings on log prices using the specification in column 3 in Table 2. The estimation includes year-month-episode and census tract-episode fixed effects. We also control for log of land area, log of building area, and the distance to the shooting. Point estimates in Appendix Figure A.3a correspond to an estimation performed using a matched sample. Details about the matching procedure are provided in Appendix C. Point estimates in Appendix Figure A.3b correspond to treatment areas defined with respect to the school district boundary. The black line denotes the estimates using the unmatched sample, while the red dotted line denotes estimates using the matched sample. Standard errors are clustered at the census tract level.

Appendix Table A.1
School Shooting Episodes

	Year	School	City	State	Victims	Fatalities
1	1998	Westside School	Jonesboro	Arkansas	15	5
2	1998	Thurston High School	Springfield	Oregon	29	4
3	1999	Columbine High School	Littleton	Colorado	37	13
4	1999	Heritage High School	Conyers	Georgia	6	0
5	1999	Fort Gibson Middle School	Fort Gibson	Oklahoma	4	0
6	2001	Santana High School	Santee	California	15	2
7	2006	Orange High School	Hillsborough	North Carolina	3	1
8	2006	West Nickel Mines Amish School*	Lancaster	Pennsylvania	10	5
9	2007	Springwater Trail High School†	Gresham	Oregon	10	0
10	2007	SuccessTech Academy†	Cleveland	Ohio	4	1
11	2007	South Middle School Football Game†	Saginaw	Michigan	4	0
12	2012	Chardon High School	Chardon	Ohio	6	3
13	2012	Sandy Hook Elementary School	Newtown	Connecticut	29	27
14	2013	Sparks Middle School	Sparks	Nevada	3	1
15	2014	Marysville-Pilchuck High School	Marysville	Washington	5	4

Note: Episodes marked with a † are excluded from the main analysis as the school attendance boundaries are not available for these episodes and they were likely confounded by the great recession. The episode in West Nickel Mines Amish School, marked with a *, is also dropped because it corresponds to a one-room schoolhouse and we do not know the attendance boundary. We present the results for these four excluded episodes in Appendix B.

Appendix Table A.2

Event Study Estimates: Effects of School Shooting on House Prices

	(1)	(2)	(3)	(4)
1(Affected AB)*1(-3 year lag)	-0.012	0.008	0.002	-0.004
	(0.015)	(0.016)	(0.016)	(0.012)
1(Affected AB)*1(-2 year lag)	-0.006	0.007	0.014	-0.001
	(0.011)	(0.010)	(0.013)	(0.011)
1(Affected AB)*1(1 year lead)	0.007	0.000	-0.012	-0.015
	(0.012)	(0.012)	(0.012)	(0.009)
1(Affected AB)*1(2 year lead)	0.024**	0.015	-0.008	-0.015
	(0.012)	(0.011)	(0.013)	(0.010)
1(Affected AB)*1(3 year lead)	-0.011	-0.008	-0.024*	-0.028**
	(0.015)	(0.015)	(0.013)	(0.011)
1(Affected AB)*1(4 year lead)	-0.005	-0.012	-0.033**	-0.032**
	(0.016)	(0.015)	(0.015)	(0.012)
1(Affected AB)*1(5 year lead)	-0.045***	-0.048***	-0.026*	-0.030**
	(0.015)	(0.013)	(0.014)	(0.014)
1(Affected AB)*1(6 year lead)	-0.039**	-0.047***	-0.032**	-0.027**
	(0.016)	(0.016)	(0.015)	(0.012)
1(Affected AB)*1(7 year lead)	-0.006	-0.012	-0.002	-0.013
	(0.016)	(0.012)	(0.013)	(0.010)
1(Affected AB)*1(8 year lead)	0.003	-0.004	-0.016	-0.014
	(0.019)	(0.016)	(0.016)	(0.015)
1(Affected AB)*1(9 year lead)	0.023	0.018	0.005	-0.007
	(0.022)	(0.017)	(0.017)	(0.015)
1(Affected AB)*1(10 year lead)	0.001	-0.004	-0.013	-0.023
	(0.034)	(0.026)	(0.022)	(0.021)
Observations	63,502	63,502	63,502	60,576
Census Tract FE	Yes	Yes		
Month-Year FE	Yes	Yes		
Basic Characteristics	Yes	Yes	Yes	Yes
Episode Specific Trend		Yes		
Month-Year-Episode FE			Yes	Yes
Census Tract-Episode FE			Yes	Yes
House Characteristics				Yes

Note: The dependent variable is the log of house price. AB stands for attendance boundary. Basic characteristics include the log of land area, the log of building area, and the distance to the shooting. House characteristics include a dummy variable for whether the house has a fireplace, a dummy variable for whether the house has a garage, the age of the property, and the number of bathrooms. Standard errors are clustered at the census tract level.*** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A.3

Robustness: Varying the Bandwidth of the Matching Procedure

	BW=0.005 (1)	BW=0.01 (2)	BW=0.05 (3)	BW=0.1 (4)	BW=0.3 (5)	BW=0.5 (6)	BW=0.7 (7)	BW=0.9 (8)
1(Affected AB)*1(After)	-0.025**	-0.024***	-0.020**	-0.019**	-0.020**	-0.022**	-0.023**	-0.023**
	(0.011)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)
Observations	26,967	33,566	35,769	36,104	36,104	36,104	36,104	36,104
Basic Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year-Episode FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census Tract-Episode FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched Sample	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The dependent variable is the log of house price. AB stands for attendance boundary. The table displays the results varying the bandwidth in a kernel matching algorithm. More details about the matching algorithm are provided in Appendix C. Basic characteristics include the log of land area, the log of building area, and the distance to the shooting. Standard errors are clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A.4

Robustness: Varying the Distance Around the Boundary

	0.2 Miles (1)	0.4 Miles (2)	0.6 Miles (3)	0.8 Miles (4)	1 Mile (5)	1.2 Miles (6)	1.4 Miles (7)
1(Affected AB)*1(After)	-0.033	-0.020	-0.015	-0.017*	-0.024**	-0.018**	-0.016*
	(0.022)	(0.014)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)
Observations	6,099	14,533	22,499	30,321	36,920	42,905	48,673
Month-Year-Episode FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census Tract-Episode FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Basic Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents estimates by varying the radius around the boundary. The dependent variable is the log of the deflated housing price. Basic characteristics include the log of land area, the log of building area, and the distance to the shooting. Standard errors are clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1.

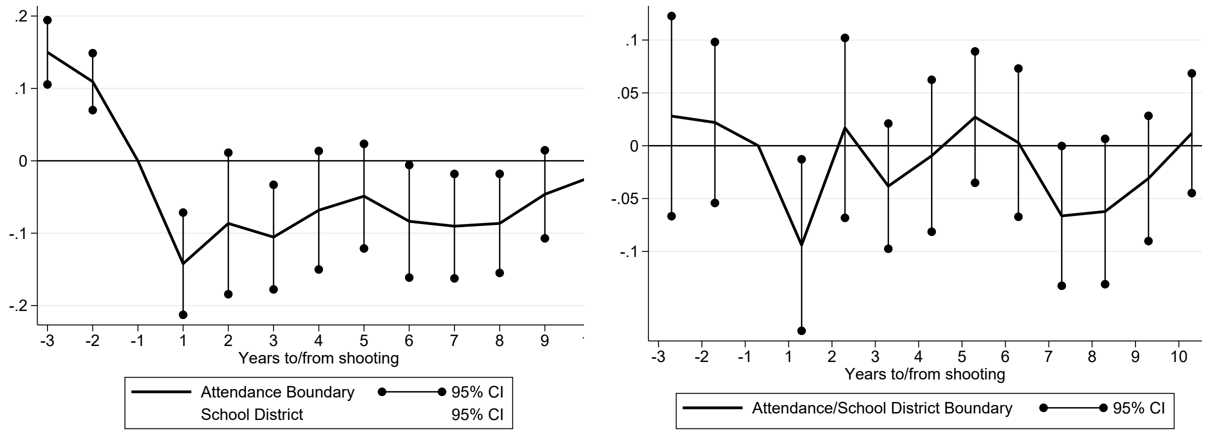
B. Effect of School Shootings on House Prices Using the Four Excluded Episodes

In this appendix, we present the estimates of the effects of shootings on house prices using the four episodes that were excluded from our main analysis (i.e., the Lancaster shooting that took place in 2006, and shootings in Gresham, Cleveland and Saginaw that took place in 2007). We do not know the school attendance boundary for any of these four episodes, so we use the school district boundary for this analysis.[1] We begin by presenting event study estimates using properties one mile around the school district boundary in Appendix Figure B.1. We observe evidence of the existence of pre-trends among the unmatched sample in Appendix Figure B.1a. Thus, to correct for the pre-trends issue, we re-estimate the model using a matched sample (reweighted by the weights computed in a matching algorithm) which satisfies the pre-trend assumption as shown in Appendix Figure B.1b. The event analysis graph indicates a decline in house prices in the post-shooting period.

Appendix Figure B.1

Effect of School Shootings on House Prices Using Four Episodes

(a) Unmatched **(b)** Matched



Notes: The figure plots event study estimates of the effect of school shootings on log prices using the specification of column 3 in Table 2. The estimations are performed using data for four episodes: Lancaster, Gresham, Cleveland, and Saginaw. We have information up to 10 years after the shooting for all of them. The estimation includes month-year-episode and census tract-episode fixed effects. Appendix Figure B.1b displays the point estimates performed in a sample matched using a kernel and a bandwidth equal to 0.1. Standard errors are clustered at the episode-by-census tract level.

We present the effect of school shootings using the excluded four episodes in Table B.1. As in our main analysis, we use a time period of three years before and four years after the school shooting. We present estimates using the unmatched sample in Panel A and estimates using the matched sample in Panel B. The results suggest a strong negative effect on house prices when using the unmatched sample of around 20 percent. These coefficients might not be valid causal estimates as the assumption of common trends is not satisfied and the untreated areas might not be a good control for the treated areas. The matched estimates in Panel B suggest a price decline in the range of 4.4 percent to 6.4 percent. Though the point estimates are significant

[1] In a few instances, the school has closed, so we use the boundary of the school district in which the school was geographically located.

(at 10 percent level) only in column 3, they remain negative across the various specifications in Panel B.

Appendix Table B.1

Effect of School Shootings on House Prices using Additional Episodes

	(1)	(2)	(3)	(4)	(5)
A) Unmatched Estimates					
1(Affected SD)*1(After)	-0.216***	-0.207***	-0.207***	-0.202***	-0.202***
	(0.047)	(0.043)	(0.043)	(0.042)	(0.043)
Observations	34,996	34,996	34,491	34,996	34,491
B) Matched Estimates					
1(Affected SD)*1(After)	-0.044	-0.063	-0.052	-0.064*	-0.054
	(0.057)	(0.039)	(0.044)	(0.037)	(0.041)
Observations	34,931	34,931	34,428	34,931	34,428
Census Tract FE	Yes	Yes	Yes		
Month-Year FE	Yes	Yes	Yes		
Basic Characteristics	Yes	Yes	Yes	Yes	Yes
Episode Specific Trend		Yes	Yes		
House Characteristics			Yes		Yes
Month-Year-Episode FE				Yes	Yes
Census Tract-Episode FE				Yes	Yes

Note: This table estimates the effect using four episodes: Lancaster, Gresham, Cleveland, and Saginaw. The dependent variable is log of house price. SD stands for school district boundary. Basic characteristics include the log of land area, the log of building area, and the distance to the shooting. House characteristics include a dummy variable for whether the house has a fireplace, a dummy variable for whether the house has a garage, the age of the property, and the number of bathrooms. The estimations in panel B use observations reweighted by kernel weights that reduce the pre-existing observable differences between observations in the treated and control areas. The bandwidth is selected using the method described in Huber et al. (2015, 2013). Details about the matching algorithm are presented in Appendix C. Standard errors are clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1.

C. Description of Matching Procedure

We use a propensity score matching procedure to reduce observable differences between treated and untreated areas. The estimation is performed separately for the school attendance analysis and the school district analysis. We collapse the price data at the census tract-school attendance (census tract-school district) level and restrict the sample to transactions observed before the shooting. We begin with all transactions in the treated and untreated areas, i.e., those that are within affected and surrounding school districts. We then merge census data at the census tract level to create a cross-section and use a least absolute shrinkage and selection operator (lasso) estimation to select the variables for the propensity score matching model. Our lasso estimation proceeds as follows:

1. We split the sample between training and estimation data sets.
2. Estimate a Lasso regression using the training sample.[2]
3. Perform the estimation using three different choices of the λ parameter:
   (a) Cross-validation;
   (b) Minimum Bayesian Information Criterion (BIC);
   (c) Adaptive lasso.
4. Compute the out-of-sample mean square error (MSE) for each criterion using the estimation data set.
5. Select the criterion with the smallest MSE.
6. In the case of the census tract-school attendance area, we selected the minimum BIC as the best criterion.[3]
7. In the case of the census tract-school district, we selected the adaptive lass as the best criterion.[4]

We then estimate a propensity score using the covariates selected in the lasso estimation and the log of average prices in $t-1$, $t-2$, $t-3$ coming from the CoreLogic data. The counterfactual distribution is then selected by estimating a kernel matching in the support of the propensity score (Abadie and Imbens, 2002, 2006).[5] We select an optimal bandwidth following Huber et al. (2013) and Huber et al. (2015). The results of the estimation of the propensity score for all the affected (i.e., inside an affected attendance area) and adjacent (i.e., within the affected and adjacent school district but outside the affected attendance area) census tracts are presented

[2]The algorithm is fed with log of population; log of median home value; log of median rent; share of college graduates; percentage of old houses; percentage married; unemployment rate; percentage who moved less than 10 years ago; labor force participation; percentage Hispanic; percentage African-American; self-employment rate; manufacturing rate; log of sales in $t-1$, $t-2$, $t-3$.

[3]The selected covariates correspond to: percentage of old houses; unemployment rate; labor force participation; percentage Hispanic; percentage African-American; manufacturing share; and log median home value.

[4]The selected covariates correspond to: log of total population; share of college graduates; log of median renting value; percentage of movers in less than 10 years; labor force participation; log of median home value in census; percentage Hispanic; percentage African-American; unemployment rate; manufacturing share; log of sales in $t-3$; and log of sales in $t-1$.

[5]We match only those census tracts that have information on house prices in the pre-shooting period. Those with no information are not included in the matching.

Appendix Table C.1
Estimation of Propensity Score

	All Census Tracts		One Mile Around Attendance Boundary	
	Full Sample	Matched Sample	Full Sample	Matched Sample
	(1)	(2)	(3)	(4)
ln(Median Home Value)	-0.296	-0.046	-0.087	-0.123
	(0.234)	(0.501)	(0.374)	(0.423)
Perc. of Old Houses	-1.321***	-0.034	-1.968**	-0.223
	(0.417)	(0.621)	(0.770)	(0.847)
Unemployment Rate	4.029	-5.285	-0.166	2.041
	(3.172)	(5.941)	(5.642)	(6.474)
Labor Force Part.	-3.071***	-1.400	-3.360	0.792
	(1.025)	(1.211)	(2.511)	(2.718)
Percentage Hispanic	-3.755**	-4.583***	1.412	-1.164
	(1.527)	(1.487)	(4.137)	(4.568)
Percentage Black	-2.830***	-1.950***	-1.405	0.161
	(0.939)	(0.612)	(1.216)	(1.386)
Manufacturing Share	4.305***	1.443	3.915**	-0.400
	(0.874)	(1.283)	(1.645)	(1.837)
$\ln(\text{Price})_{t-3}$	0.009	1.023*	-0.018	0.011
	(0.320)	(0.561)	(0.541)	(0.615)
$\ln(\text{Price})_{t-2}$	0.359	0.136	0.694	-0.046
	(0.307)	(0.485)	(0.596)	(0.674)
$\ln(\text{Price})_{t-1}$	-0.211	-1.524***	-0.535	0.198
	(0.362)	(0.482)	(0.611)	(0.658)
Observations	2,037	2,023	222	214

Note: Dependent variable is a dummy variable that takes the value of one if the census tract is affected by a school shooting and 0 otherwise. Probit estimations are performed at the census tract level. Covariates are selected using a lasso algorithm. More details are provided in Appendix C. Column (1) includes all the sample, whereas Column (2) reweights by the weights obtained from the kernel matching algorithm. Column (3) uses the sample of census tracts one mile around the attendance boundary, and Column (4) is again reweighted using the weights obtained from the kernel matching algorithm. *** p<0.01, ** p<0.05, * p<0.1.

in in column (1) of Table C.1. We then reweight by the kernel weights in Column (2), showing a good balance across the matching covariates. We do the same for the census tracts-by-school attendance area one mile around the attendance boundary and present the results in columns (3) and (4).

The matching procedure yields a set of weights that perfectly balance any observable difference between the treated and untreated observations. Such weights are used in the estimations performed in columns (8) to (10) of Table 1, and in any of the other estimations that use matched samples in the paper.