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# Do School Shootings Erode Property Values?

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# Do School Shootings Erode Property Values?

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**Abstract**

This paper exploits the exogenous timing of mass shootings in schools to estimate the causal effects of school shootings on housing values and sheds light on the underlying mechanism. Using a difference-in-differences strategy, we find that house prices within a school district decline by seven percent after a school shooting, with stronger effects among houses with more bedrooms (proxy for school-age children in household). We also find evidence of a decrease in school enrollment and the number of teachers after the shooting. This suggests that decreased demand for schools within the affected school districts explains the drop in property prices.

**JEL Classification: I21, R21**

**Keywords: Mass shooting, house prices, schooling amenities, crime**

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# 1. Introduction

The United States has more mass shootings than any other country, and the number of episodes rose by more than five times in the period from 2014 to 2017.[1,2] These episodes have been shown to create trauma that devastates its victims by increasing suicides, accidental deaths, mental health conditions, and anti-depressant consumption (Cabral, Kim, Rossin-Slater, Schnell, and Schwandt, 2021; Levine and McKnight, 2020; Nader, Pynoos, Fairbanks, and Frederick, 1990; Rossin-Slater, Schnell, Schwandt, Trejo, and Uniat, 2020). Furthermore, the exposure to mass shootings also has negative effects on the economic activity of a county by decreasing employment and earnings (Brodeur and Yousaf, 2020).

About 15 percent of these mass shootings occurred in schools, directly affecting young students and their residential communities. School shootings are a type of crime that is unpredictable, exceptionally traumatic, highly unlikely to be repeated in the same location, and directly targeted towards young students.[3] These episodes have been shown to have negative effects on their victims by lowering test scores and increasing absenteeism, (Beland and Kim, 2016), but is still uncertain how they widely affect the communities around the schools in a longer term. As opposed to other types of *mass* shootings, the shootings that occur in schools directly affect schooling amenities and might indirectly impact the perception of parents about the quality of schools in the area. These adverse effects might reduce the preference for a geographical area and, thereby, reduce housing prices.

This paper examines the effects of school shootings on residential housing values and sheds light on mechanisms behind the relationship between crime and house prices. This relationship has been broadly documented as negative and strong.[4] Households might avoid areas with high

[1] See Figure A1.

[2] We define mass shootings as gun-related episodes with three or more victims (excluding perpetrators) that do not involve gangs, drugs, or organized crime.

[3] We define school shootings as *mass* shootings that occurred in an elementary, middle, or high school.

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

levels of crime because of the associated potential loss if they were to be victimized in the future. This link appears to be the logical explanation for the relationship between crime and housing prices, but it is not the only one. Crime, in fact, may have some externalities that may lead to a reduction in house prices due to some other understudied or unrecognized channels. In the case of school shootings, homeowners, and potential home-buyers, might avoid areas where a shooting took place simply because they do not want their kids to attend nor to be associated with the affected school, and not because of fear of future victimization.

We exploit the exogenous timing of the shooting to implement a differences-in-differences strategy that estimates the causal effects of school shootings on housing prices, and to test the mechanisms behind this relationship. The key empirical challenge is identifying the counterfactual scenario, i.e., how prices would have evolved in absence of the shooting. Relying on cross-sectional variation alone might lead to biased estimates because house prices might vary across geographic administrative boundaries due to both observed and unobserved characteristics. The difference-in-differences strategy addresses this potential concern by comparing prices in the affected school district with those in neighboring school districts.

Descriptive statistics at the census tract level suggest differences in levels among observable characteristics between affected and neighboring school districts prior to the school shootings. Our difference-in-differences strategy takes care of these preexisting differences in *levels*, but, to ensure that our results are not driven by differences in pre-existing *trends* between the treated and untreated areas, we use a matching approach within the difference-in-differences framework to reduce potential concerns. Given that we use repeated cross-sections of transaction data, we match at the census tract level using observable characteristics before the shooting and select the untreated group based on the nearest neighbor match. We supplement this analysis by also using a boundary discontinuity approach, within the difference-in-differences framework, to compare houses within half a mile of the school-district boundary to better control for unobserved amenities.

We focus on school shootings during the period 1998 to 2014. Our analysis employs two key sources of data: 1) the Stanford Mass Shootings of America data project; and 2) individual transaction and assessment records for the school districts where shootings took place and the adjacent school districts for the period from 1995 to 2017. These data were obtained from CoreLogic, Inc., which collects real estate information nationwide. Our analysis uses micro-level transaction data from all U.S. school districts in which a mass shooting took place on a school campus. The coverage of the data makes our results externally valid.

Our results suggest that house prices within the affected school districts fall by an average of seven percent (or $13,000 on average), and its effects persist for, at least, nine years. This effect is stronger when we restrict the analysis to properties near a school-district boundary (nine percent), and the decline remains persistent again for nine years after the shooting. Additionally, we find that, in line with a short-term increase in supply, the number of transactions in the affected school district increases in the short term.

This paper is the first systematic evaluation of the long-term effects of school shootings on property values. A recent paper Gourley (2019) estimates the effect of the Columbine shooting on housing values, but it focuses on estimating the short-term effects of a single episode. Therefore, it does not have enough power to identify additional mechanisms concerning schooling preferences, or to explode alternative estimation strategies such as the boundary discontinuity. Gourley (2019) claims that the decrease in prices is explained by social stigma (i.e. a subjective distaste unrelated to any traditional product characteristic). However, school shootings have strong externalities on the preferences for the schools associated with the shootings, inducing home-buyer and owners to avoid the area.

We test for this mechanism and find that school shootings decrease school enrolment and number of teachers, and affect exclusively the prices of family-sized properties (a proxy for a family with school-aged kids). We additionally use buffers of different radius to compare the prices of properties around the school with those farther away. We do not find any sizable effect of the shooting when restricting the estimation to properties within the school district, suggesting an overall price decrease among properties within the district. These results suggest that housing prices decrease due to an increase in the distaste of the schools within the school district, rather than by social stigma.

Our analysis additionally 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 work adds to the existing works of Linden and Rockoff (2008) and J. C. Pope (2008), who analyze how proximity to the home of a registered sex offender decreases house prices, and to 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 housing 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.[5] These papers estimate a lower willingness to pay for housing in neighborhoods in which schools are reputed to be of poor quality.[6] Our paper adds to this literature by analyzing whether a lower demand for schools after a school shooting is capitalized in house prices.

# 2. Data

We combine data from two main sources. First, we use arm's length real estate transaction data for the period 1996-2017 for the school districts that were affected by a mass shooting in schools, and for the neighboring districts that were unaffected.[7] We merge these data with assessment records using a unique property identifier for each property to ascertain the

[5] A summary of this literature is provided by Gibbons and Machin (2008), Black and Machin (2011), Nguyen-Hoang and Yinger (2011) and Machin (2011). There is a consensus estimate of around 3–4 percent house price premium for one standard deviation increase in school average test scores.

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

[7] See Appendix A for the list of counties we use in our analysis

characteristics of the house. Both data sets come from Corelogic Inc. The data contain information on transaction, price, and date of sale, along with the geographic coordinates of the house and characteristics of the house like size, age, number of bedrooms, baths, presence of garage, fireplace etc.[8] These data do not include socio-demographic information about homeowners, although it is very rich and descriptive about house prices and amenities. In order to describe the setting, we, therefore, use census information at the census tract level prior to the shootings (i.e. we use census 1990 data) merged to the affected and non-affected school districts.

We match the sales data to the school districts by using the latitude and longitude coordinates of the property. The school-district boundary maps are obtained from the National Center for Education Statistics (NCES). We also identify the corresponding census tract by overlaying the transaction data with the Census Tract shapefile (2010 definition) obtained from the U.S. Census Bureau.

Second, we use data for mass shootings in America from the Stanford Mass Shootings of America (MSA) data project (courtesy of the Stanford Geospatial Center and Stanford Libraries). The project started in 2012 in reaction to the Sandy Hook mass shooting incident in Connecticut and collects data from online media sources. The project 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 gang-, drug- or organized crime-related. The dataset includes the time, date, and location of the shooting, along with the number of victims and the number of fatalities. It also indicates whether the shooting took place at a school or not. We consider all mass shootings at schools that happened after the year 1998.[9]

Additionally, we also use crime data at the city level from City-Data.com and the school enrollment and data on the number of teachers from the National Center for Education Statistics

[8]We drop transactions with sales prices in top and bottom 1 percent of the distribution for each county to eliminate outliers. We normalize the sale prices using quarterly Case-Shiller Home Price Indices for each state to September 2017.

[9]The date and episodes are presented in Appendix Table 1.

(NCES).

# 3. Empirical Strategy

## 3.1. Effect of Mass Shootings on House Prices

Individuals choose where to live based on many factors such as housing characteristics, school quality, local amenities, proximity to labor markets, etc. This individual sorting usually hinders any potential estimation of the effect of crime on housing values. It is expected that areas with a low crime have higher demand and is also the case that crime is endogenously determined in certain locations. Furthermore, unobserved characteristics also play an important role by including potential confounding factors into the estimation.

School shootings, however, are isolated exogenous episodes that homeowners and buyers are not able to predict. They occur in a random fashion and thus enable a potential estimation framework free of confounding factors such as individual sorting.

The key empirical challenge is finding a valid counterfactual distribution - i.e., what would have happened if the shooting had not taken place. We, therefore, use a difference-in-differences strategy that compares house prices in the school district where a shooting took place ("treated" school district) with the adjacent school districts ("untreated" school districts).[10] Figure 1a describes our strategy using a map for Fairfield County, Connecticut, where the Sandy Hook Elementary School shooting took place in 2012. This strategy estimates the ex-post average price difference between treated and untreated areas by taking into account the preexisting differences across locations. Note also that our estimation strategy is based on an economic intuition since homeowners in treated school districts are likely to be impacted

[10] We use school-district boundaries instead of school-attendance zones (as treated and untreated units) because the attendance boundaries are not available for some of the schools in our datasets. Moreover, it is more difficult to clearly identify the untreated areas (which are the adjacent areas to the treated unit) for our analysis as the attendance zones overlap. The advantage of using school district-level data is that the schools within the district are subject to the same policies and regulations, and boundaries are less subject to boundary changes across time. For more information on the advantages of school district boundaries see Dhar and Ross (2012).

as their children are likely to attend the affected school, whereas homeowners in untreated areas are eligible to enroll their children outside the affected school district. Thus, the shooting episode affects homeowners in the entire school district and not only those living geographically closer to the school.

Empirically, we append 15 shooting episodes from 1998 to 2014 and analyze a time window of three years before and nine years after the episode.[11] We are able to track the entire 15 episodes for three years after the shooting, but the number of episodes decreases when we analyze after three years due to data limitations. We are only able to observe 11 episodes between four and nine years after the shooting.

We collapse the transaction data at the census tract level for the tracts in the treated and adjacent school district. The estimating equation for the effects of school shootings on house prices is as follows:

$$ln(p_{jt}) = \alpha + \beta(T_j * 1(\text{After shooting})_t) + \gamma X_{it} + \delta_j + \mu_t + \sum_{(k=1)}^{14} \phi_t \times 1(\text{episode})_k + \varepsilon_{jt}, \quad (1)$$

where $p_{jt}$ is the median housing price in census tract $j$ in year-month $t$, $T_j$ takes the value of one if census tract is within the school district where a shooting took place, $1(\text{After shooting})_t$ takes the value of one after the shooting, $X_{jt}$ is a matrix of observable housing characteristics, collapsed at the census tract level, such as average *log* area of building, average log area of land, the average distance to shooting, average square footage, and percentage of houses with fireplace and garage. We include census tracts fixed effects ($\delta_j$) to control for the time-invariant characteristics of the neighborhood and year-month fixed effects ($\mu_t$) to control for time trends. We also include episode-specific time trends ($\phi_t * 1(\text{episode})_k$) to account for time-varying trends across episodes. $1(\text{episode})_k$ 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. $\varepsilon_{jt}$ is the error term. Standard errors are clustered at the census tract level.

[11] We present the episodes and their dates in Appendix Table 1

The difference-in-difference estimator controls for preexisting differences across treated and untreated census tracts. However, to further reduce the concern about preexisting differences, and to find a more appropriate counterfactual distribution, we use two alternative strategies. First, we use a nearest-neighborhood matching estimator that gets rid of observable differences between treated and untreated areas (Abadie and Imbens, 2002, 2006). The counterfactual distribution is selected by matching from the set of census tracts that are in the adjoining school districts of the untreated school districts. We use socioeconomic and demographic variables at the census tract level from the 1990 census as matching variables. This matching vector includes the tract's population, median home value, median rent, median household income, percentage black, percentage Hispanic, unemployment rate, share of college graduates, percentage married, poverty rate, percentage of old houses, and percentage of households that moved in the last 10 years. We also use lagged median prices of the census tract one year, two years, and three years before the event took place and restrict to the common support. We use an optimal caliper of 0.2 standard deviations of the logit of the propensity score (Austin, 2011; Wang et al., 2013). It is possible that using different calipers, or different matching techniques, might change the estimated point estimates. Thus, we use different alternative matching techniques as a robustness test and find that our estimates remain negative and statistically significant for different caliper and bandwidth levels.

Second, we use a boundary discontinuity design that compares the treated school districts with the adjacent ones but restricting the sample to observations within a half-mile from the school-district boundary. Figure 1b describes this strategy for the shooting in Orange High School, NC, in 2006. The estimation strategy is the same as Equation 1, but includes properties that are physically closer and are, thus, likely to be similar in observed and unobserved amenities. We additionally perform a matching algorithm among these border census tracts to further reduce the observable differences.

Table 1 presents the summary statistics for the unmatched and matched samples, using the full and the boundary data. We observe significant differences in levels between treated and untreated census tracts in both unmatched samples (p-values in columns (6) and (12)). However, the matching algorithm selects a sample in which all the covariates are perfectly balanced (columns (7) and (13)).[12] This approach decreases potential biases caused by the differences in levels between the treated and untreated census tracts and computes a perfectly balanced counterfactual distribution. Our main results correspond to those using the matched sample, although, for the sake of completeness, we present estimates using the unmatched sample in all our estimations.

### 3.2. Effect of Mass Shootings on Number of Sales

In addition to the effect of school shootings on sales prices, we also estimate the change in the number of transactions taking place after the shooting. The empirical strategy we use is very similar to the one we use for analyzing the effect on prices, but we instead use a balanced panel at the census tract month year level to include months with no sales. We estimate the following difference-in-differences specification in the balanced panel:

$$\ln(\text{Sales})_{jt} = \alpha + \beta(T_j * 1(\text{After shooting})_t) + \delta_j + \mu_t + \sum_{(k=1)}^{14} \phi_t \times 1(\text{episode})_k + \varepsilon_{jt},$$

where $\ln(\text{Sales})_{jt}$ is the number of sales in census tract j in year t. All the other variables take the same values as in Equation 1.

## 4. Results

### 4.1. Main Results

Panel A of Table 2 presents the results for the full sample which includes all the census tracts in the affected and adjacent school districts. Column (1) includes the census tract and year*month fixed effects. It suggests an average decline of 11.6 percent in affected school

[12]To reduce multicollinearity issues, we include in the matching algorithm only the variables marked with a † in Table 1.

districts as compared to house prices in the neighboring school districts over a nine-year period after the shooting took place. Since we are pooling different events together, we next include in column (2) the episode-specific time trend to control for varying time trends over different regions of the country. We find point estimates that are similar to the previous specification. In column (3), we include average property characteristics. This specification is the same as the one illustrated in Equation 1, and suggests that housing prices decline by 11.7 percent in the school district where shooting took place.

A potential concern for our analysis would be if our estimates are driven by a large number of transactions in a few census tracts. To rule out this possibility, we reweigh our data by the inverse of the number of observations per episode before the shooting (column 4). Such an estimation strategy gives equal weight to all the episodes. We estimate a 17.8 percent decline in house prices when using the weighted regression. This estimator reinforces our results and suggests that our findings of a decline in house prices after shootings are not driven by any specific episode where a higher number of transactions have taken place.[13]

Next, we re-estimate Equation (1) and restrict the analysis to properties within half a mile of the school-district boundary, which may be more comparable in observed and unobserved characteristics. Columns (5) to (8) in Table 2 summarize the results. We observe a decrease between 14 and 19 percent over a nine-year period after the shooting. This result is again robust to alternative specifications and weighing strategy and suggests that the estimated effect also exists when exploiting exogenous boundary discontinuities among properties located very close geographically.

We present our main results using the matched sample in Panel B of Table 2. The point estimates decrease in magnitude compared to the unmatched sample, but they remain robust and significant. Our preferred estimation strategy which includes the average property characteristics and the episode-specific time trends suggests a 6.9 percent decline when using

[13] In addition, we estimate in Appendix Table 3 the same weighted regression but excluding each episode separately. The results hold and are still robust showing that one episode is not explaining the entire result.

the full sample (column 3) and an 8.7 percent decline when using observations that are half a mile within the school district boundary.[14] These results suggest that the estimated effects are not driven by observable differences between the affected and unaffected tracts and that the point estimates are robust to alternative methods.

The results in Table 2 are estimated using the aggregated data at the census tract level. As a robustness test, we provide estimates of Equation (1) at the transaction level. The results are summarized in Appendix Table 2, and are qualitatively similar to our base results.

## 4.2. Event Study Estimates

Our identification strategy relies on the parallel trend assumption, which implies that the price in the treated census tracts would have evolved similar to the price in the untreated census tracts in the absence of shooting. Thus, the difference in the post-shooting period is the causal effect of shooting. Even though the shootings occur in a random fashion, there is a possibility that the estimated decrease in home values is a result of differential trends between affected and unaffected areas before the shooting. To test for this, we estimate an event study version of our differences-in-differences model by interacting the treatment dummy with year dummies before and after the shooting.

We leverage the length of our transaction data set to estimate the dynamic effects three years before and nine years after the shooting occurred. However, the estimates from three to nine years after the shooting should be analyzed with caution because they are estimated using fewer episodes of shooting .[15] We present in Figure 2 the event study estimates of Equation 1. These results test the existence of potential pre-trends and estimate the dynamic effects after the shooting.

For the unmatched sample, we observe a small but significant difference in price trends

[14]We additionally provide estimations with alternative calipers and bandwidths in Appendix Table 4, including matching with a kernel.

[15]Recall that four events in our data set took place after 2010, and we observe transaction data until 2017. Thus, the point estimates corresponding to three to nine years include exclusively 11 episodes.

before the shooting, both for the full sample, in panel 2a, and for the boundary sample in panel 2b. This could probably be due to the differences in levels among the affected and unaffected census tracts, as shown in Table 1.

In fact, such differences disappear when estimating the event study using the matched sample. We plot the point estimates using a red and solid line in Figure 2. We observe that the pre-event estimates are no longer statistically significant and thus comply with the parallel trends assumption.

The post-event estimates provide evidence of the persistence of the effects for many years after the shooting took place. The event analysis graph indicates that prices decrease 11.4 percent during the first year after the shooting and then decline by 6.3 percent and 4.6 percent in the second and third years, respectively. The results imply that the effect of shooting declines but persists up to nine years in the aftermath of the shooting, even though the point estimates corresponding to years four to nine include a smaller number of episodes.

## 4.3. Effect on Number of Transactions

The decline in prices may be explained by shifts in housing supply or demand after the shooting. To understand what drives the decrease we estimate the effect of shootings on the number of sales using Equation (2). The estimates for the event study analysis are presented in Figure 2c. The graph reveals a transitory increase in the number of transactions The residents of an affected school district could have been eager to relocate and therefore put their house on the market. These houses might have been bought by households moving from other areas of the country who were not affected directly by this school shooting, and are therefore less traumatized by the particular shooting episode. However, unlike the decline in prices, we do not see a persistent increase in the number of transactions.[16]

[16] Appendix Table 5 presents the differences-in-differences results under alternative specifications, suggesting an average increase in the number of transactions.

# 5. Mechanisms

The relationship between crime and property values may be explained by many 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 a school district experiences a shooting again is very low and is no different from the probability that any other school district experiences its first school shooting.

It may be the case that school shootings are associated with a simultaneous increase in other types of crime. Thus, we test for this by estimating a difference-in-differences model at the city level using yearly crime rates from 2002 to 2016 as the dependent variable. We use cities where the school shooting took place as treated units and neighboring cities as controls.[17] As shown in Appendix Table 6, our estimates provide evidence of declines in crime, implying that crime (i.e. a higher probability of repetition) is not the reason for the decline in house prices.

Gourley (2019) additionally suggests that social stigma drives the housing prices down after a school shooting. However, an increase in the distaste for the schools (i.e. a decrease in the demand for schools) in the affected school district could also drive such results. Parents of affected students might want to move out, whereas home-buyers might avoid the area because they do not want their kids either to attend or be associated with these schools.

In fact, 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. 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. (2008) show that mass casualty incidents can trigger post-traumatic stress disorder symptoms, and increase suicides, at the one-year anniversary of their traumatic exposure.[18]

[17]This analysis does not cover all of the cities but only cities where data was available.

[18]Two students at the Stoneman Douglas High School committed suicide around the one-year anniversary date

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 significantly decrease school enrollment and test scores, whereas Levine and McKnight (2020) find that exposure to school shootings led to increase in chronic absenteeism, and subsequent 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 if the school shootings analyzed herein decrease the number of teachers and students in the affected schools. A decrease in demand for the schools in the affected districts will imply that students and teachers leave after the shooting. We again follow the strategy of comparing the affected school districts with the adjacent ones in a difference-in-difference setting and present the results in Table 3. We observe a decrease in the number of students and teachers, which is in line with Beland and Kim (2016). The results show an overall decrease of 16 percent in school enrolment, and 8 percent in the number of teachers, among all schools within an affected school district. We also look separately at the schools where the shooting took place, and we observe a reduction of 18 percent in the number of students and 13 percent in the number of teachers. These effects also spilled over to the neighboring schools which were not directly affected by the school shooting but were within the same school district. For them, we observe a decrease of 7.5 percent in enrolment and 5.2 percent in the number of teachers, although the latter is not precisely estimated. These results provide evidence about the distaste for schools acting as a mechanism and explaining the relationship between school shootings and housing prices.

We additionally test if our results are driven by families with school-age children, who are the type of home-buyers who care the most about schooling amenities. Unfortunately, we cannot directly observe which families have children, but we use the number of bedrooms in

of the shooting that took place at the school.

a house as a proxy for family size as families are likely to have houses with more bedrooms. We find that the effect is non-existent among one-bedroom properties and concentrated among properties with two or more bedrooms (Appendix Table 7). This supports the fact that families with children might drive the price decrease.

Finally, we analyze if the price decrease is concentrated around properties close to the affected school or if, instead, there is a generalized price decrease around the entire school district. If prices decrease throughout the school district it implies that the preference for the school district declined. In contrast, if stigmas about the affected areas led to a decline in prices, then properties closer to the shooting will have a larger decrease in prices compared to the ones farther away. To test for this, we follow Linden and Rockoff (2008) and J. C. Pope (2008) and construct buffers around the location of the shooting. We define as treated those properties within 0.3, 0.5, and one mile around the school. The properties outside these radii but inside the buffer are considered as untreated. We present the results in Appendix Table 8. We only find significant negative effects when using a large five-mile buffer and including properties outside the affected school district (columns (4) to (6)). All the significant negative effects disappear once we condition on properties lying within the affected school district, suggesting that the entire school district decreased its prices. This supports our priors that distaste of the schools within the school district, and not stigmas exclusively, are the mechanism connecting school shootings and property values.

# 6. Conclusion

In this paper, we estimate the effect of school shootings on property values in the United States. We use a differences-in-differences method to compare the treated and adjacent school districts and minimize the observable differences between census tracts using a matching estimator and a border discontinuity design.

We find that, on average, the home values in affected school districts decrease by seven percent to 11 percent after the shooting. The effect is stronger when we look at homes closer to the school district boundary (about nine percent to 12 percent).[19] These effect seems to be very persistent, even several years after the shooting. We additionally find that the number of transactions increases in the short term, which implies a short-run increase in supply.

We also explore the mechanisms behind this price adjustment and find evidence that it is explained by an increase in the distaste for the schools within the affected school district, or, in other words, a decrease of the demand for those schools. A big body of literature has suggested that schools are a highly valued amenity among households, and our results validate these findings by suggesting that potential home buyers avoid school districts in which shooting has taken place because of a distaste for the schools within it.

The magnitudes we estimate for the entire school district (around seven percent over a nine-year period) are slightly larger compared 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) estimate a three percent increase in prices for an increase of one standard deviation in average value added. Our estimates are smaller compared to the disamenity found by Linden and Rockoff (2008), when examining the effect on house prices in close proximity to the residence of a registered sex offender (a decline of 11.6 percent). Our results are also similar to the effects on house prices that stem from the discovery of a cancer cluster of child leukemia (a decline in values of 14 percent) (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 have a strong preference to reside in areas with schools they highly value. We provide evidence that crime does not affect property values only by the fear of being victimized, but also by other alternative channels. Incidents such as school shootings, that affect the schooling amenities, might also lead to a decline in house prices.

[19] The average price of houses in affected school districts prior to the shooting was around $183,000, which implies an average decrease between $13,000 and $20,000.

Future research is needed to understand how to deal with locations affected by crime shocks, particularly with a school-related crime.

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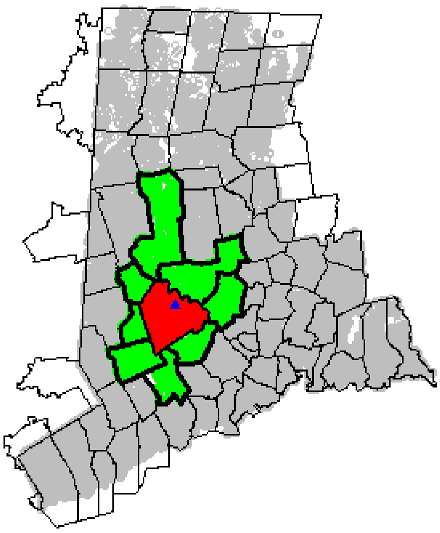
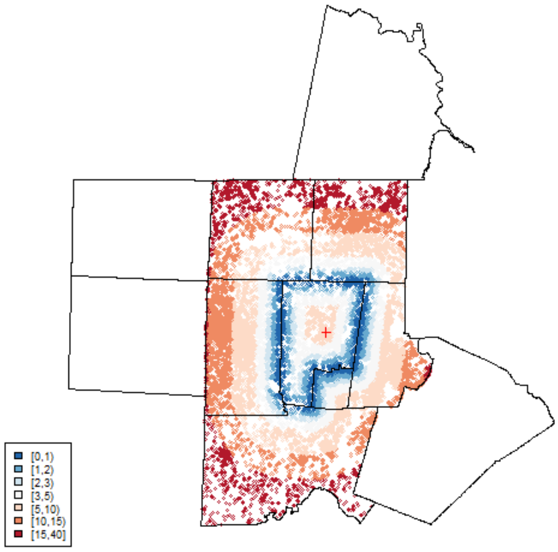
**Figure 1**
Description of Empirical Strategy

**(a)** Main Empirical Strategy

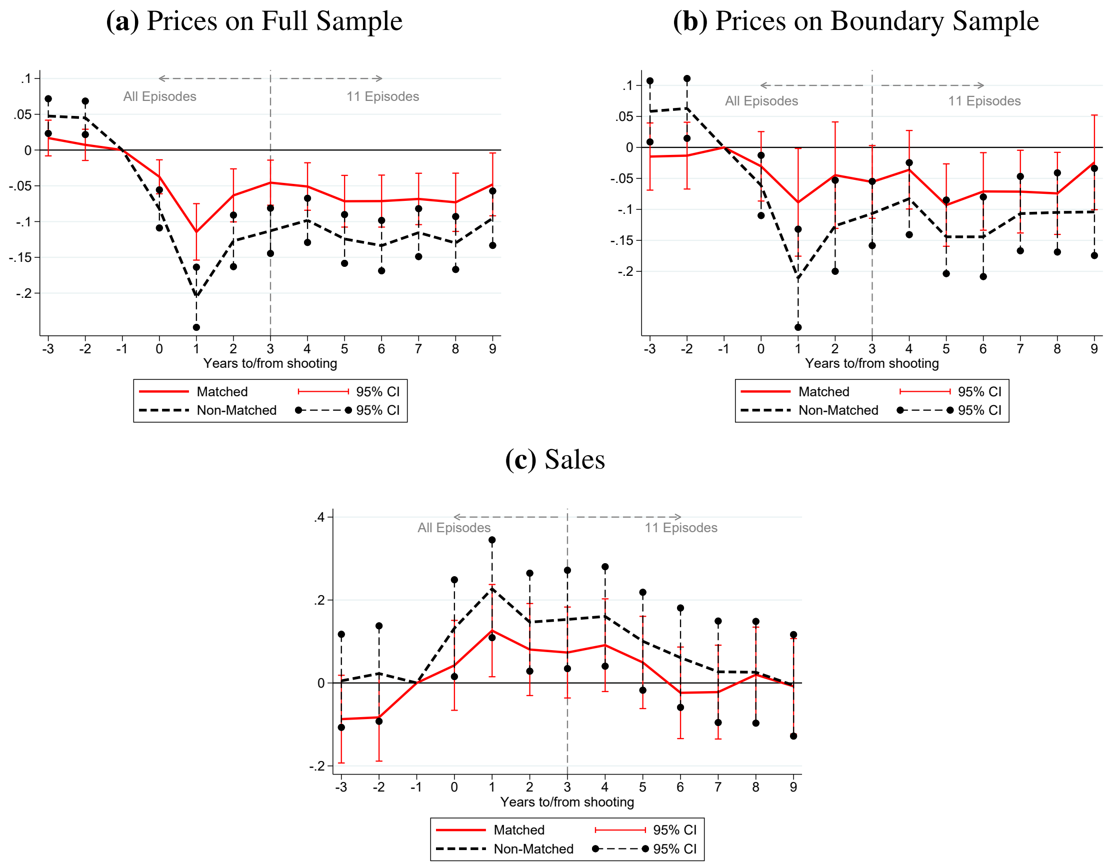


**(b)** Border Discontinuity




*Note:* The map in Figure 1a plots Sandy Hook school district in Newton, CT. The blue triangle indicates the location of the 2012 shooting. The red area indicates the affected school district whereas the green area the adjacent school districts. The map in Figure 1b plots Orange County, NC County. The red cross indicates the location of Orange high school, where a shooting took place in 2006. The different colors show the distance to the school district boundary.

**Figure 2**
Estimates of the Effect of School Shootings on Housing Prices and Sales




*Note:* These figures plot event study estimates. Panel (a) and (b) use the specification of column 3 and column 7 in Table 2 respectively and Panel (c) use the specification of column 3 in Table 5. The estimations include year-by-month and census tract fixed effects, episode-specific trends, and average property characteristics. Average property characteristics include the share of properties with a fireplace, share of properties with a garage, the share of condos, the average land square footage, the average log of building and land area, and the average distance to the shooting. Standard errors clustered at the census tract level.

**Table 1**
Summary Statistics at the Census Tract Level

| | Full Sample | | | | | | | Boundary Sample | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Unmatched | | | Matched | | P-values | | Unmatched | | Matched | | P-values | |
| Covariate | Treated (1) | Controls (2) | Rest (3) | Treated (4) | Controls (5) | (1)-(2) (6) | (4)-(5) (7) | Treated (8) | Controls (9) | Treated (10) | Controls (11) | (8)-(9) (12) | (10)-(11) (13) |
| Population† | 3,233.26 | 3,364.46 | 3,423.00 | 3,312.59 | 3,272.50 | 0.07 | 0.68 | 3,496.85 | 3,481.81 | 3,586.46 | 3,488.42 | 0.91 | 0.52 |
| Median Home Value† | 93,083.56 | 112,388.18 | 101,298.33 | 103,260.56 | 105,692.43 | 0.00 | 0.49 | 83,195.95 | 97,301.40 | 94,743.30 | 94,786.74 | 0.00 | 0.99 |
| Median Rent† | 415.46 | 462.40 | 391.96 | 449.07 | 459.03 | 0.00 | 0.31 | 382.66 | 414.17 | 416.29 | 409.42 | 0.01 | 0.64 |
| Income Per Capita | 14,019.70 | 16,083.35 | 14,448.21 | 15,110.84 | 15,575.65 | 0.00 | 0.17 | 13,276.52 | 15,307.79 | 14,738.62 | 14,741.87 | 0.00 | 0.99 |
| Median HH Income† | 32,019.23 | 35,692.19 | 32,076.80 | 34,916.65 | 36,233.69 | 0.00 | 0.11 | 30,544.73 | 35,000.42 | 34,628.23 | 34,057.45 | 0.00 | 0.65 |
| Percentage White | 0.80 | 0.83 | 0.83 | 0.85 | 0.86 | 0.05 | 0.70 | 0.77 | 0.84 | 0.84 | 0.84 | 0.01 | 0.90 |
| Percentage Black† | 0.15 | 0.11 | 0.11 | 0.10 | 0.09 | 0.00 | 0.63 | 0.19 | 0.12 | 0.12 | 0.11 | 0.01 | 0.95 |
| Percentage Hispanic† | 0.05 | 0.07 | 0.07 | 0.05 | 0.04 | 0.00 | 0.12 | 0.04 | 0.04 | 0.04 | 0.04 | 0.55 | 1.00 |
| Labor Force Part. | 0.50 | 0.53 | 0.50 | 0.52 | 0.53 | 0.00 | 0.73 | 0.49 | 0.52 | 0.51 | 0.52 | 0.00 | 0.33 |
| Employment Rate | 0.92 | 0.95 | 0.93 | 0.94 | 0.94 | 0.00 | 0.69 | 0.91 | 0.94 | 0.94 | 0.94 | 0.00 | 0.62 |
| Unemployment Rate† | 0.08 | 0.05 | 0.07 | 0.06 | 0.06 | 0.00 | 0.69 | 0.09 | 0.06 | 0.06 | 0.06 | 0.00 | 0.62 |
| Female Labor Force | 0.59 | 0.61 | 0.57 | 0.61 | 0.61 | 0.00 | 0.84 | 0.57 | 0.60 | 0.59 | 0.60 | 0.00 | 0.38 |
| Self-Employment Share | 0.07 | 0.07 | 0.08 | 0.07 | 0.07 | 0.00 | 0.59 | 0.06 | 0.07 | 0.07 | 0.07 | 0.01 | 0.48 |
| Share College Graduates† | 0.12 | 0.17 | 0.13 | 0.13 | 0.15 | 0.00 | 0.05 | 0.10 | 0.14 | 0.13 | 0.13 | 0.00 | 0.81 |
| Percentage Married† | 0.41 | 0.43 | 0.43 | 0.43 | 0.44 | 0.00 | 0.20 | 0.40 | 0.44 | 0.44 | 0.44 | 0.00 | 0.79 |
| Poverty Rate† | 0.14 | 0.10 | 0.13 | 0.10 | 0.10 | 0.00 | 0.54 | 0.14 | 0.09 | 0.09 | 0.10 | 0.00 | 0.48 |
| White Poverty Rate | 0.07 | 0.06 | 0.08 | 0.06 | 0.06 | 0.00 | 0.82 | 0.07 | 0.06 | 0.06 | 0.06 | 0.02 | 0.55 |
| Perc. of old houses† | 0.39 | 0.32 | 0.40 | 0.33 | 0.32 | 0.00 | 0.72 | 0.47 | 0.42 | 0.39 | 0.40 | 0.08 | 0.66 |
| Perc. moved in $<$ 10 yrs† | 0.26 | 0.26 | 0.24 | 0.26 | 0.26 | 0.07 | 0.92 | 0.24 | 0.23 | 0.23 | 0.24 | 0.36 | 0.65 |
| $\ln(\text{price})_{t-3}$† | 12.22 | 12.41 | . | 12.36 | 12.37 | 0.00 | 0.72 | 12.10 | 12.24 | 12.24 | 12.22 | 0.02 | 0.80 |
| $\ln(\text{price})_{t-2}$† | 12.22 | 12.42 | . | 12.35 | 12.36 | 0.00 | 0.73 | 12.12 | 12.25 | 12.26 | 12.25 | 0.03 | 0.95 |
| $\ln(\text{price})_{t-1}$† | 12.19 | 12.44 | . | 12.35 | 12.35 | 0.00 | 0.92 | 12.08 | 12.25 | 12.24 | 12.24 | 0.01 | 0.93 |
| Number of Observations | 655 | 1,788 | 70,390 | 544 | 544 | | | 280 | 286 | 181 | 181 | | |

*Note:* The table presents mean differences of variables in treated and untreated census tracts using the 1990 census. Variables with a † are included in the matching algorithm. Columns (1)-(3) present raw averages. Columns (4) and (5) display the averages among the matched sample computed using a one-to-one matching algorithm that uses calipers of width equal to 0.2 of the standard deviation of the logit of the propensity score. Columns (6) and (7) present the p-values of the differences in means. Columns (8) and (9) present raw averages among census tracts located 0.5 miles around the school district border. Columns (10) and (11) present the averages using the matched sample among census tracts near the border using a one-to-one matching algorithm that uses calipers of width equal to 0.2 of the standard deviation of the logit of the propensity score. Columns (12) and (13) present the p-values of the differences in means among the boundary sample.

**Table 2**
Effect of School Shooting on Housing Prices

| | *Full Sample* | | | | *Boundary Sample* | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *A) Full Sample* | | | | | | | | |
| 1(Affected SD)*1(After Shoot.) | -0.116*** | -0.101*** | -0.117*** | -0.178*** | -0.189*** | -0.138*** | -0.150*** | -0.188*** |
| | (0.015) | (0.012) | (0.012) | (0.022) | (0.026) | (0.024) | (0.026) | (0.038) |
| Observations | 285,558 | 285,558 | 242,857 | 242,857 | 41,387 | 41,387 | 37,090 | 37,090 |
| *B) Matched Sample* | | | | | | | | |
| 1(Affected SD)*1(After Shoot.) | -0.053*** | -0.052*** | -0.069*** | -0.108*** | -0.145*** | -0.074** | -0.087*** | -0.123** |
| | (0.014) | (0.012) | (0.013) | (0.021) | (0.030) | (0.029) | (0.032) | (0.051) |
| Observations | 127,058 | 127,058 | 110,239 | 110,239 | 24,915 | 24,915 | 22,468 | 22,468 |
| Year*Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Census Tract FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Episode Specific Trend | | Yes | Yes | Yes | | Yes | Yes | Yes |
| Av. Property Characteristics | | | Yes | Yes | | | Yes | Yes |
| Weighted Regression | | | | Yes | | | | Yes |

*Note:* Dependent variable corresponds to the log of the median housing value per census tract and month. Columns (1) to (4) present the estimations using the full sample. Columns (5) to (8) present the estimations using the sample of census tracts within 0.5 miles around the school district boundary. Panel A presents estimates for the full sample, whereas panel B for the matched sample. The matching sample is computed using a one-to-one matching algorithm that uses calipers of width equal to 0.2 of the standard deviation of the logit of the propensity score. Average property characteristics include the share of properties with a fireplace, share of properties with a garage, the share of condos, the average land square footage, average log of building and land area, average age of property, number of bedrooms, and the average distance to the shooting. Column 6 displays estimates weighing by the inverse of the total number of sales per episode in the pre-period. Standard errors clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1

**Table 3**
Effect on School Enrollment and Number of Teachers

| | ln Enrollment | | ln Teachers | |
|---|---|---|---|---|
| | District FE (1) | School FE (2) | District FE (3) | School FE (4) |
| 1(Affected SD)*1(After Shooting) | -0.161*** | | -0.083* | |
| | (0.042) | | (0.044) | |
| 1(Affected School)*1(After Shooting) | | -0.176** | | -0.126* |
| | | (0.085) | | (0.069) |
| 1(Surrounding School in SD)*1(After Shooting) | | -0.075* | | -0.052 |
| | | (0.042) | | (0.035) |
| Observations | 26,115 | 26,033 | 25,255 | 25,178 |
| Year FE | Yes | Yes | Yes | Yes |
| Episode FE | Yes | Yes | Yes | Yes |
| School District FE | Yes | | Yes | |
| School FE | | Yes | | Yes |

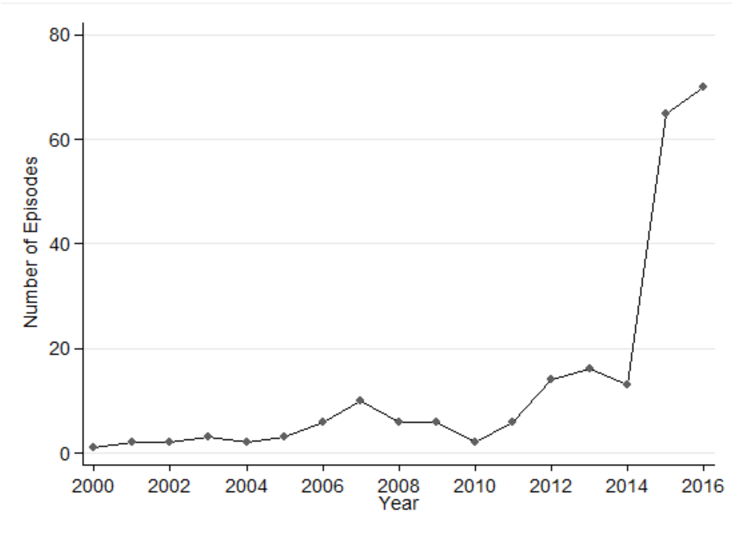
*Note:* Standard errors clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1

# A. Appendix

The counties used in our analysis include Craighead, Greene, Lawrence, Jackson, Poinsett in Arkansas; Lane in Oregon; Jefferson, Park, Clear Creek, Gilpin, Boulder, Adams, Denver, Arapahoe in Colorado; Rockdale, Dekalb, Gwinnett; Walton; Newton and Henry in Georgia; San Diego, Lake, Modoc, Lassen, Plumas, Sierra, Nevada, Placer, El Dorado in California; Beltrami, Marshall, Clearwater, Pennigton, Polk in Minnesota; Orange, Alamance, Durham, Chatham, Caswell, Person in North Carolina; Lancaster, Chester in Pennsylvania; Multnomah, Clackamas in Oregon; Cuyahoga in Ohio; Saginaw, Bay in Michigan, Geauga, Lake in Ohio, New haven, Fairfield, Litchfield in Connecticut; Washoe, Harney, Carson City, Churchill, Douglas, Humboldt, Lyon, Pershing, Storey in Nevada and Snohomish in Washington.

**Appendix Figure A1**

Number of Mass Shootings 2000-2016



**Appendix Table 1**
School Shooting Episodes

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

# Appendix Table 2

Effect of School Shooting on Housing Prices at the Property Level

| | *Full Sample* | | | | *Boundary Sample* | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *A) Full Sample* | | | | | | | | |
| 1(Affected SD)*1(After Shoot.) | -0.028** | -0.039*** | -0.057*** | -0.107*** | -0.147*** | -0.100*** | -0.118*** | -0.149*** |
| | (0.012) | (0.009) | (0.009) | (0.017) | (0.026) | (0.024) | (0.029) | (0.042) |
| Observations | 1,509,896 | 1,509,896 | 1,243,818 | 1,243,818 | 105,241 | 105,241 | 84,807 | 84,807 |
| *B) Matched Sample* | | | | | | | | |
| 1(Affected SD)*1(After Shoot.) | 0.014 | 0.002 | -0.020** | -0.062*** | -0.107*** | -0.040 | -0.052 | -0.056 |
| | (0.011) | (0.010) | (0.010) | (0.018) | (0.030) | (0.028) | (0.035) | (0.051) |
| Observations | 675,813 | 675,813 | 575,632 | 575,632 | 66,475 | 66,475 | 53,755 | 53,755 |
| Year*Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Census Tract FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Episode Specific Trend | | Yes | Yes | Yes | | Yes | Yes | Yes |
| Property Characteristics | | | Yes | Yes | | | Yes | Yes |
| Weighted Regression | | | | Yes | | | | Yes |

*Note:* Dependent variable corresponds to the log price of properties at the individual transaction level. Columns (1) to (4) present the estimations using the full sample. Columns (5) to (8) present the estimations using properties within 0.5 miles around the school district boundary. Panel A presents estimates for the full sample, whereas panel B for the matched sample. The matching sample is computed using a one-to-one matching algorithm that uses calipers of width equal to 0.2 of the standard deviation of the logit of the propensity score. Property characteristics include the share of properties with a fireplace, share of properties with a garage, the share of condos, the average land square footage, average log of building and land area, average age of property, number of bedrooms, and the average distance to the shooting. Columns 4 and 8 display estimates weighing by the inverse of the total number of sales per episode in the pre-period. Standard errors clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1

## **Appendix Table 3**
## Robustness of the Effect Dropping Episodes

| | Chardon | Cleveland | Columbine | Conyers | Fort Gibson | Gresham | Hillsborough | Lancaster | Marysville | Newton | Saginaw | San Diego | Sparks | Springfield |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
| 1(Affected SD)* | -0.115*** | -0.054** | -0.120*** | -0.111*** | -0.116*** | -0.114*** | -0.118*** | -0.108*** | -0.111*** | -0.117*** | -0.110*** | -0.049*** | -0.114*** | -0.135*** |
| 1(After shoot.) | (0.022) | (0.025) | (0.023) | (0.022) | (0.022) | (0.022) | (0.022) | (0.021) | (0.022) | (0.023) | (0.022) | (0.012) | (0.022) | (0.023) |
| Observations | 108,771 | 94,242 | 81,391 | 94,378 | 109,465 | 105,849 | 105,636 | 110,239 | 107,847 | 105,582 | 108,958 | 105,807 | 98,829 | 100,381 |
| Census Tract FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year*Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Episode Specific Trend | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Weighted Regression | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Matched Sample | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Av. Property Characteristcs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Note:* This table presents estimates dropping one episode at a time. The dependent variable corresponds to the log of the median housing value per census tract and month. All estimates use the matched sample computed using a one-to-one matching algorithm that uses calipers of width equal to 0.2 of the standard deviation of the logit of the propensity score. Average property characteristics include the share of properties with a fireplace, share of properties with a garage, the share of condos, the average land square footage, average log of building and land area, and the average distance to the shooting. Observations are weighed by the inverse of the total number of sales per episode in the pre-period. Standard errors clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1

**Appendix Table 4**

Robustness of the Matching Method

| | BW 0.0005<br>(1) | BW 0.001<br>(2) | BW 0.005<br>(3) | BW 0.01<br>(4) | BW 0.05<br>(5) | BW 0.1<br>(6) | BW 0.3<br>(7) | BW 0.5<br>(8) | BW 0.7<br>(9) | BW 0.9<br>(10) |
|---|---|---|---|---|---|---|---|---|---|---|
| *A) One-to-One Matching* | | | | | | | | | | |
| 1(Affected SD)*1(After Shoot.) | -0.046*** | -0.057*** | -0.069*** | -0.070*** | -0.059*** | -0.060*** | -0.085*** | -0.101*** | -0.101*** | -0.101*** |
| | (0.014) | (0.013) | (0.013) | (0.013) | (0.013) | (0.013) | (0.013) | (0.013) | (0.013) | (0.013) |
| Observations | 90,919 | 99,517 | 109,314 | 110,397 | 110,470 | 110,562 | 116,233 | 121,435 | 121,621 | 121,621 |
| *B) Kernel Matching* | | | | | | | | | | |
| 1(Affected SD)*1(After Shoot.) | -0.054*** | -0.064*** | -0.078*** | -0.088*** | -0.092*** | -0.093*** | -0.101*** | -0.105*** | -0.108*** | -0.110*** |
| | (0.013) | (0.012) | (0.012) | (0.013) | (0.013) | (0.013) | (0.012) | (0.011) | (0.011) | (0.011) |
| Observations | 158,628 | 193,344 | 228,554 | 235,837 | 238,403 | 238,713 | 238,713 | 238,713 | 238,713 | 238,713 |
| Census Tract FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year*Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Av. Property Characteristics | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Episode Specific Trend | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Matched Sample | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Note:* Dependent variable corresponds to the log of the median housing value per census tract and month by properties with a different number of bedrooms. Panel A presents the results varying the caliper using a one-ton-one matching algorithm. Panel B displays the results varying the bandwidth in a kernel matching algorithm. Average property characteristics include the share of properties with a fireplace, share of properties with a garage, the share of condos, the average land square footage, average log of building and land area, and the average distance to the shooting. Standard errors clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1

**Appendix Table 5**

Effect of School Shooting on Housing Sales

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *A) Full Sample* | | | | |
| 1(Affected SD)*1(After Shoot.) | 0.125*** | 0.065*** | 0.091*** | 0.048** |
| | (0.023) | (0.015) | (0.015) | (0.023) |
| Observations | 369,904 | 369,904 | 369,904 | 369,904 |
| *B) Matched Sample* | | | | |
| 1(Affected SD)*1(After Shoot.) | 0.140*** | 0.074*** | 0.114*** | 0.054** |
| | (0.027) | (0.017) | (0.017) | (0.023) |
| Observations | 153,884 | 153,884 | 153,884 | 153,884 |
| Year*Month FE | Yes | Yes | Yes | Yes |
| Census Tract FE | | Yes | Yes | Yes |
| Episode Specific Trend | | | Yes | Yes |
| Weighted Regression | | | | Yes |

*Note:* Dependent variable corresponds to the log of the number of sales per census tract and month. Estimations done using a balanced panel that replaces the dependent variable with a zero if no transactions took place in the given month. Panel A presents estimates for the full sample, whereas panel B for the matched sample. The matching sample is computed using a one-to-one matching algorithm that uses calipers of width equal to 0.2 of the standard deviation of the logit of the propensity score. Column 6 displays estimates weighing by the inverse of the total number of sales per episode in the pre-period Standard errors clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1

**Appendix Table 6**

Effect on Crime

| | Crime Rates | | | | | | | | PCA Index |
|---|---|---|---|---|---|---|---|---|---|
| | Murders | Rapes | Robberies | Assaults | Burglaries | Thefts | Auto Thefts | Arson | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| 1(Affected City) | -0.532 | -5.155 | -9.161 | -104.704*** | -85.046 | 261.614** | -133.462*** | -18.292* | -24.523 |
| $*$1(After Shoot.) | (1.300) | (4.099) | (15.465) | (37.139) | (85.028) | (117.979) | (27.772) | (10.247) | (17.907) |
| Observations | 1,514 | 1,490 | 1,491 | 1,476 | 1,474 | 1,474 | 1,475 | 1,450 | 1,474 |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| City FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Note:* Standard errors clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1

**Appendix Table 7**

Effect of School Shootings by Number of Bedrooms

| | 1 BR | 2 BR | 3 BR | 4 BR | <=2 BR | >= 3 BR |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (6) | (7) |
| *A) Full Sample* | | | | | | |
| 1(Affected SD)*1(After Shoot.) | -0.051* | -0.105*** | -0.114*** | -0.082*** | -0.103*** | -0.124*** |
| | (0.029) | (0.013) | (0.011) | (0.011) | (0.013) | (0.012) |
| Observations | 9,517 | 98,081 | 208,415 | 137,792 | 101,125 | 231,840 |
| *B) Matched Sample* | | | | | | |
| 1(Affected SD)*1(After Shoot.) | -0.061 | -0.103*** | -0.087*** | -0.058*** | -0.101*** | -0.092*** |
| | (0.053) | (0.016) | (0.012) | (0.013) | (0.016) | (0.013) |
| Observations | 3,430 | 43,059 | 95,258 | 63,782 | 44,464 | 105,810 |
| Census Tract FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year*Month FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Episode Specific Trend | Yes | Yes | Yes | Yes | Yes | Yes |
| Av. Property Characteristics | Yes | Yes | Yes | Yes | Yes | Yes |

*Note:* Dependent variable corresponds to the log of the median housing value per census tract and month by properties with a different number of bedrooms. Panel A presents estimates for the full sample, whereas panel B for the matched sample. The matching sample is computed using a one-to-one matching algorithm that uses calipers of width equal to 0.2 of the standard deviation of the logit of the propensity score. Average property characteristics include the share of properties with a fireplace, share of properties with a garage, the share of condos, the average land square footage, average log of building and land area, average age of property, and the average distance to the shooting. Standard errors clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1

**Appendix Table 8**

Effect of School Shooting on Housing Prices using Buffers Around Shooting Location

| | *Unconditional Buffers* | | | | | | *Conditioning to Within Affected School District* | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Buffer = | 1 Mi. | 2 Mi. | | 5 Mi. | | | 1 Mi. | 2 Mi. | | 5 Mi. | | |
| *X* = | 0.3 | 0.3 | 0.5 | 0.3 | 0.5 | 1 | 0.3 | 0.3 | 0.5 | 0.3 | 0.5 | 1 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| *A) Full Sample* | | | | | | | | | | | | |
| 1(Within X Mi.)*1(After Shoot.) | -0.020 | -0.069 | -0.084 | -0.122* | -0.138** | -0.117** | 0.018 | -0.007 | -0.017 | 0.012 | -0.012 | -0.011 |
| | (0.067) | (0.064) | (0.071) | (0.065) | (0.070) | (0.050) | (0.065) | (0.058) | (0.059) | (0.052) | (0.045) | (0.033) |
| Observations | 13,575 | 38,770 | 38,770 | 151,677 | 151,677 | 151,677 | 13,009 | 35,505 | 35,505 | 114,130 | 114,130 | 114,130 |
| *B) Matched Sample* | | | | | | | | | | | | |
| 1(Within X Mi.)*1(After Shoot.) | -0.032 | -0.044 | -0.026 | -0.114 | -0.108 | -0.088** | -0.032 | -0.042 | -0.024 | -0.034 | -0.028 | -0.007 |
| | (0.070) | (0.065) | (0.062) | (0.070) | (0.067) | (0.042) | (0.070) | (0.065) | (0.062) | (0.052) | (0.042) | (0.020) |
| Observations | 12,259 | 33,211 | 33,211 | 119,282 | 119,282 | 119,282 | 12,259 | 32,484 | 32,484 | 105,801 | 105,801 | 105,801 |
| Census Tract FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year*Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Episode Specific Trend | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Av. Property Characteristics | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Weighted Regression | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Note:* Dependent variable corresponds to the log price of properties at the individual transaction level. Buffer corresponds to the area around the shooting location. X corresponds to the area used to split between treated and untreated units. Columns (1) to (7) present do not condition on being within an affected school district. Columns (8) to (13) present the estimations restricting to properties within the affected school district. Panel A presents estimates for the full sample, whereas panel B for the matched sample. The matching sample is computed using a one-to-one matching algorithm that uses calipers of width equal to 0.2 of the standard deviation of the logit of the propensity score. Average property characteristics include the share of properties with a fireplace, share of properties with a garage, the share of condos, the average land square footage, average log of building and land area, average age of property, number of bedrooms, and the average distance to the shooting. All columns display estimates weighing by the inverse of the total number of sales per episode in the pre-period. Standard errors clustered at the census tract level. *** p<0.01, ** p<0.05, * p<0.1