Local exposure to school shootings and youth antidepressant use

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While over 240,000 American students experienced a school shooting in the last two decades, little is known about the impacts of these events on the mental health of surviving youth. Using large-scale prescription data from 2006 to 2015, we examine the effects of 44 school shootings on youth antidepressant use. Our empirical strategy compares the number of antidepressant prescriptions written by providers practicing 0 to 5 miles from a school that experienced a shooting (treatment areas) to the number of prescriptions written by providers practicing 10 to 15 miles away (reference areas), both before and after the shooting. We include month-by-year and school-by-area fixed effects in all specifications, thereby controlling for overall trends in antidepressant use and all time-invariant differences across locations. We find that local exposure to fatal school shootings increases youth antidepressant use by 21.4% in the following 2 y. These effects are smaller in areas with a higher density of mental health providers who focus on behavioral, rather than pharmacological, interventions.

school shootings | gun violence | youth mental health | antidepressants

School shootings have become frequent tragedies in the United States. Since the shooting at Columbine High in April 1999, there have been over 249 shootings at primary and secondary schools, resulting in the loss of 147 lives (1). Public attention often focuses on the victims who were killed, but an important and understudied question is how survivors fare in the subsequent months and years. While over 240,000 students were on school grounds during a shooting in the past 20 y, little is known about the impacts of these events on the mental health of surviving youth. The large private and social costs of mental illness—especially during childhood—suggest that estimates of the effects of school shootings on mental health may be critical for assessing the overall welfare consequences of these events.*

Existing work on the mental health consequences of school shootings is limited to studies at several schools in the 1980s and 1990s (see refs. 5 and 6 for recent reviews and citations therein). These studies suggest that such events are associated with adverse psychological outcomes, but they rely on small samples, often lack control groups or preexposure data, and use surveys that may be subject to selective response bias (5, 6). Furthermore, it may be hard to extrapolate correlations from 30 to 40 y ago to events happening today.

In this study, we examine the impacts of local exposure to 44 school shootings from 2006 to 2015 on an important indicator of youth mental health: the use of prescription antidepressants.† Our empirical strategy compares the number of antidepressant prescriptions written by providers practicing 0 to 5 miles from a school that experienced a shooting (treatment areas) to the number of prescriptions written by providers practicing 10 to 15 miles away (reference areas), both before and after the shooting. We include month-by-year and school-by-area fixed effects in all specifications, thereby controlling for overall trends in antidepressant use and all time-invariant differences across locations. As communities may differ in their capacity to cope with shooting-related trauma, we further investigate heterogeneity in effects by the local availability of mental health care resources.

Our research design is predicated on the idea that providers practicing very close to an affected school treat patients who are more likely to have been exposed to the event but are otherwise very similar to the patients of providers practicing slightly farther away. As it is unclear ex ante how far from a school the effects of a shooting will extend, we demonstrate how our results change as we use alternative distances between schools and providers to define treatment areas.‡ We further examine specifications using two alternative reference areas consisting of the prescriptions written by providers practicing in the vicinity of observationally similar schools that did not experience any shootings.

Our data on antidepressants come from the IQVIA Xponent database and cover the period from January 2006 to March 2015.

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Significance

In the last two decades, over 240,000 American students were on school grounds when a gunman opened fire at their school. While public attention often focuses on the victims who were killed, less is known about the impacts of school shootings on surviving youth. This study represents the largest analysis to date of the effects of school shootings on an important indicator of youth mental health: the use of prescription antidepressants. We find that local exposure to fatal school shootings leads to persistent and significant increases in youth antidepressant use. These impacts are smaller in areas with a higher density of mental health providers who focus on behavioral interventions.

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*Mental illness in childhood has been shown to have negative long-term impacts on human capital formation and adult economic outcomes, exceeding the impacts of poor physical health conditions such as childhood epilepsy and low birth weight (2, 3). Recent estimates suggest that depression alone costs the US economy over $210 billion/y in lost productivity, missed days of work, and direct health care costs stemming from related physical and mental illnesses (4).

†Antidepressants are frequently used to treat mental health conditions that may be relevant for shooting survivors, including major depressive disorder and posttraumatic stress disorder (7, 8).

‡Among our sample schools included in the School Attendance Boundary Survey, the average school attendance area is 80 square miles (*SI Appendix*, section D). Students attending schools that experienced a shooting are therefore likely to live within our treatment areas of 5 miles surrounding the school ($\pi * 5^2 = 78.5$).

Table 1. Descriptive Statistics

	School shootings		
	Fatal	Nonfatal	None
School characteristics			
Enrollment (1,000s)	1.22	0.90	0.44
Private	0.13	0.07	0.22
High school	0.40	0.55	0.15
% White	0.49	0.43	0.59
% Black	0.17	0.37	0.16
% Reduced lunch	0.43	0.51	0.44
Characteristics of shootings			
Victims killed	2.80	0.00	–
Victims injured	0.47	0.76	–
Shooter age	25.60	18.46	–
Shooter male	0.80	0.97	–
Number of schools	15	29	127,363
Youth antidepressant prescriptions per 1,000			
Treatment areas			
Preshooting/postshooting	7.70/9.52	5.64/6.86	–
Reference areas			
Primary, preshooting/postshooting	8.09/9.05	8.71/10.02	–
Alt. A, overall	6.87	10.45	–
Alt. B, preshooting/postshooting	11.07/12.63	7.09/7.95	–

The first two columns report averages for schools with shootings in our analysis. The last column reports averages for all US schools. Youth antidepressant prescriptions per 1,000 refer to mean monthly antidepressant prescriptions for individuals under age 20. Treatment (primary reference) areas consist of providers practicing 0 to 5 miles (10 to 15 miles) from schools that experienced a shooting. Alternative (Alt.) reference areas A and B consist of providers practicing 0 to 5 miles from nonshooting schools with the highest predicted shooting probability (A) and with characteristics matched to individual shooting schools (B). We cannot separate preshooting and postshooting prescription rates for alternative reference areas A, as the comparison schools in those areas are not matched to specific shooting dates.

In contrast to typical claims data that cover either one or a few insurers, the IQVIA data include the near universe of prescriptions irrespective of patients' insurance coverage or type.[§] Our primary analysis focuses on antidepressants prescribed to youth, whom we define as individuals under age 20 (i.e., aged 0 to 19). The IQVIA data are collected directly from pharmacies; as such, they do not include information on the number of patients seen by each provider. As we therefore do not have the population base necessary to construct exact prescription rates, we use the natural log of antidepressant prescriptions as our main outcome. As an alternative outcome, we consider prescription rates calculated as the number of antidepressant prescriptions written to individuals under age 20 by providers practicing in a given area divided by the number of individuals under age 20 living in the same area.

We combine the prescription data with information on school shootings from the *Washington Post* school shootings database. The data contain all shootings at primary and secondary schools in the United States since 1999 that occurred during school hours and posed a threat to students. To allow for 2 y of prescription data before and after each shooting, we consider school shootings that occurred between January 2008 and April 2013. For each affected school, we identify providers practicing in the treatment and reference areas using practice addresses in the IQVIA data.

Results

Since April 1999, the annual number of school shootings in the United States has ranged from 5 in 2002 to 17 in the first 5 mo of 2018 (*SI Appendix*, Fig. S1); 240,718 students were enrolled in these schools when they experienced a shooting. Table 1 presents mean characteristics of the 44 schools with shootings in our analysis, out of which 15 resulted in at least one victim death ("fatal" shootings). Relative to the average US school, schools that experienced a shooting had higher average enrollment, were less likely to be private, were more likely to be a high school, and had a higher average share of black students.

To examine changes in youth antidepressant use surrounding school shootings, we first plot monthly antidepressant prescription rates for individuals under age 20 in the 2 y surrounding a school shooting. We plot prescription rates separately for the treatment and reference areas and for fatal and nonfatal shootings. We further plot linear fits of the data using only preshooting observations; these lines provide counterfactuals for how prescriptions are expected to have progressed in the absence of a school shooting.

As shown in Fig. 1, antidepressant use in the treatment areas increased dramatically following a school shooting and remained at elevated levels through the end of the observation period. There is no apparent trend break in the reference areas. The increase in antidepressant use in the treatment areas is noticeably larger following fatal than nonfatal shootings. Patterns in the raw data therefore point to a persistent effect of fatal school shootings on youth antidepressant use.

We formalize this analysis using a difference-in-difference design. We regress the natural log of the monthly number of antidepressant prescriptions written for individuals under age 20 in the 2 y surrounding a school shooting on an indicator denoting the postshooting time period, an interaction between the postshooting indicator and an indicator denoting treatment areas, year-by-month fixed effects, and school-by-area fixed effects. The year-by-month fixed effects account for aggregate trends in youth antidepressant use. The school-by-area fixed effects account for all time-invariant differences across locations,

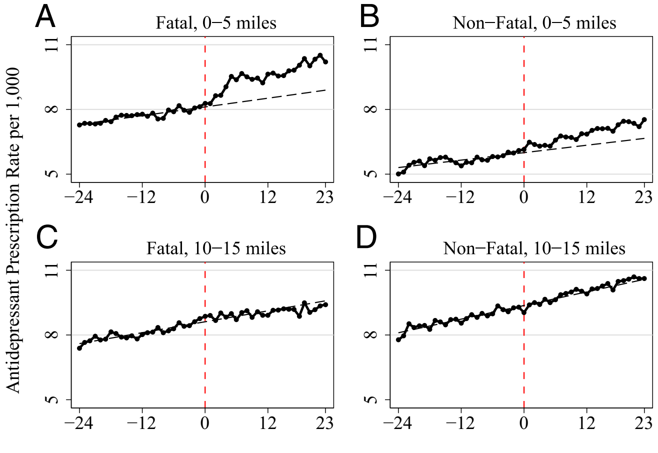

Fig. 1. Youth antidepressant use by month relative to school shootings. The solid black lines show the monthly number of antidepressant prescriptions written to individuals under age 20 by providers located 0 to 5 miles (*A* and *B*) or 10 to 15 miles (*C* and *D*) from a school that experienced a fatal (*A* and *C*) or non-fatal (*B* and *D*) shooting per 1,000 individuals under age 20 residing in these areas. The dashed gray lines are linear fits based on regressions using only preshooting observations.

[§]We only observe the number of prescriptions, not the number of pills or strength of the medication provided with each script. While the number of scripts could increase without the total quantity of antidepressants dispensed rising, more frequent but smaller prescriptions would suggest increased monitoring that in itself is indicative of worsening mental health (7, 8).

including preshooting levels of antidepressant use. We cluster standard errors at the school-by-area level.

The results are presented in Table 2. When the outcome is log antidepressant prescriptions, our coefficient of interest measures the percentage difference in the postshooting change in the number of antidepressant prescriptions written to individuals under age 20 between the treatment and reference areas. As shown in the first column, youth antidepressant use increases by 21.3% in the treatment areas in the 2 y following a fatal school shooting. When extending the postshooting observation window to 3 y, we observe a 24.5% increase. These effects of fatal school shootings are concentrated among shootings that occur in high schools (SI Appendix, Table S2). We find no statistically significant changes in youth antidepressant use following nonfatal school shootings, and results are very similar in percentage terms when we use prescription rates as an alternative outcome.

Our difference-in-difference strategy requires that prescriptions would have followed similar trends across the treatment and reference areas in the absence of a school shooting. To examine the validity of this parallel trends assumption and to explore the time path of effects, we estimate quarterly event study specifications. In these models, we replace the postshooting indicator with separate indicators for each of the eight quarters before and after the shooting, omitting the indicator for the quarter before the shooting.

Fig. 2 plots the estimated coefficients from these regressions. The insignificant coefficients before the shooting indicate that there were no differential pretrends across the treatment and reference areas. Within 6 mo of a fatal school shooting, however, antidepressant prescriptions increase by nearly 30% and remain at this elevated level for at least 2 y. As in Table 2, Fig. 2 shows that youth antidepressant use is not affected by nonfatal school shootings.

Prior work has suggested that the negative effects of exposure to violence on children's cognitive outcomes are temporary (e.g., ref. 9). To examine whether the observed increases in youth antidepressant use fade over time, *SI Appendix*, Fig. S2 presents estimates from event study specifications that use follow-up windows of 3 to 6 y. Although our sample size diminishes considerably as we extend the observation window, and thus these results may not be generalizable, we find that fatal school shootings lead to increases in youth antidepressant use that persist over these longer time horizons.

Table 2. Effects of school shootings on youth antidepressant use

	Fatal		Nonfatal	
	2 y	3 y	2 y	3 y
ln(antidepressant prescriptions)				
Treatment × post	0.213***	0.245***	0.0187	0.0603
	(0.064)	(0.079)	(0.060)	(0.053)
Antidepressant prescription rate per 1,000				
Treatment × post	1.982**	2.645**	1.674	0.348
	(0.940)	(1.241)	(1.219)	(0.701)
Relative to mean	0.206**	0.297**	0.163	0.037
Schools	15	12	29	24
Observations	1,412	1,410	2,601	2,718

The table reports output from estimation of our primary difference-in-difference specification. We regress measures of antidepressant prescriptions for individuals under age 20 at the school–area–month level on an indicator denoting months in or after a school shooting ("post"), an interaction between the post indicator and an indicator denoting treatment areas, month-by-year fixed effects, and school-by-area fixed effects. The treatment (reference) areas include providers practicing 0 to 5 miles (10 to 15 miles) from an affected school. All regressions are weighted by school enrollment, and standard errors (reported in parentheses) are clustered at the school-by-area level. $^{**}P < 0.05$; $^{***}P < 0.01$.

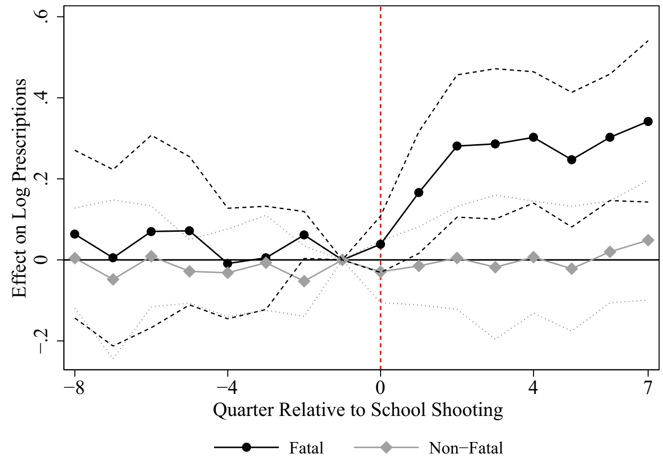

Fig. 2. Effects of school shootings on youth antidepressant use. The figure presents output from estimation of event study analogs of our primary difference-in-difference specification, in which we replace the postshooting indicator with separate indicators for each of the eight quarters before and after the shooting. The indicator for the quarter before the shooting is omitted. We plot the coefficients and 95% CIs on the interactions between quarterly event time indicators and the indicator denoting treatment areas.

Local areas may vary in their capacity to cope with trauma in the aftermath of a school shooting. To investigate whether the impacts of school shootings differ by the availability of local mental health care resources, we examine how our estimates vary across locations with differing densities of providers who treat mental health conditions among children. As different types of providers differ in their focus on pharmacological versus behavioral treatment, we consider both the local density of prescribing providers (physicians in family medicine, pediatrics, and psychiatry) and nonprescribing providers (psychologists and social workers).[¶]

SI Appendix, Table S3 presents estimates from an augmented version of our difference-in-difference specification that includes triple interactions between the post indicator, the treatment indicator, and terciles of county-level measures of provider densities. We see that there are no significant differences in the effects of fatal school shootings across counties with differing densities of prescribing providers. In contrast, areas with a higher density of nonprescribing practitioners experience significantly smaller increases in youth antidepressant use following a fatal school shooting. This pattern holds even when controlling for the density of prescribing practitioners within each county. This is consistent with the possibility that areas with more psychologists and social workers may rely on nonpharmacological treatment, such as cognitive behavioral therapy, to treat shooting-induced trauma. However, we note that this analysis relies on residual variation in prescriber and nonprescriber densities across only 12 counties represented in our sample of fatal school shootings. The results may therefore be of limited generalizability to other areas.

Discussion

Understanding the mental health consequences of school shootings is critical both for informing cost–benefit analyses of policies aimed at reducing gun violence and for designing programs to help mitigate the consequences of shootings when they do

[¶]Although a few states have recently expanded their scope of practice legislation to allow certain psychologists to prescribe some medications, prescribing has traditionally been limited to physicians. Furthermore, while physicians can in principle provide psychotherapy, the vast majority of physicians—even those who specialize in psychiatry—no longer provide behavioral therapy.

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occur. Accordingly, leading scholars across multiple disciplines have recently issued calls for more evidence on the impacts of shootings on survivors (6, 10, 11). Using large-scale prescription data, we document that local exposure to fatal school shootings leads to significant and persistent increases in antidepressant use among American youth.

A large literature suggests that exposure to violence—such as neighborhood crime, domestic violence, and school bullying—has adverse effects on children's mental health (e.g., refs. 12–17). However, nonrandom selection into violent settings makes it difficult to ascertain causal relationships from previous work (18). We overcome this challenge by leveraging the quasi-random timing of school shootings as a natural experiment. Our work therefore complements existing studies that use natural experiments to examine the impacts of violence on outcomes other than mental health, such as children's short-term educational and behavioral outcomes (9, 19–30).

Moreover, it is possible that school shootings have different effects on youth mental health than other types of violence. A recent study of police killings of African Americans found a deterioration in the self-reported mental health of black adults due to communal bereavement and increased fear and vigilance (31). Analogously, youth exposed to school shootings may suffer differentially because of the direct connection they feel to victims and the loss of a sense of security at their schools. Notably, we find no differences in effects across areas with differing crime rates (*SI Appendix*, Table S4), suggesting that fatal school shootings even affect the mental health of students who are already exposed to violence outside of school.

The impacts of school shootings on youth antidepressant use that we find are remarkably persistent. In contrast to the literature on childhood resilience, which suggests that exposure to a single violent event may not have lasting effects (32, 33), we find that exposure to a single, fatal school shooting leads to worse mental health among local youth for years. These lasting impacts are consistent with the development of chronic mental health conditions for which clinical practice guidelines recommend long or indefinite treatment (7, 8). Other factors, such as heightened insecurity in the local community (34, 35), could further contribute to persistent mental health effects.[#]

Our primary specification compares the number of antidepressant prescriptions written by providers practicing 0 to 5 miles from an affected school to those written by providers practicing 10 to 15 miles away, both before and after the shooting. Two assumptions must hold for this research design to be valid. First, antidepressant prescriptions in these areas must be on parallel trends in the absence of a school shooting. Since providers practicing very close to an affected school are likely to treat patients who are similar—both in terms of sociodemographics and access to mental health care resources—to the patients of providers practicing slightly farther away, we expect this assumption to hold. Consistent with this conjecture, Table 1 shows that mean antidepressant prescription rates before fatal school shootings were similar in our treatment and reference areas (7.70 and 8.09 per 1,000 youth, respectively), while Fig. 1 shows that antidepressant prescription rates were on similar trends across these areas in the 2 y before the event.

Second, our research design requires that providers practicing very close to an affected school are more likely to treat patients who were impacted by the event than providers practicing slightly farther away. If individuals who see providers practicing 10 to 15 miles away from the school are also impacted by the shooting, then our estimates will understate the true effects of school shootings. That said, we found no observable changes in antidepressant prescriptions written by providers practicing 10 to 15 miles from affected schools (Fig. 1), suggesting that such spillovers are not present in our setting.

Nevertheless, to ensure that our results are not driven by our choice of treatment or reference areas, we conduct two additional sets of analyses. First, we estimate specifications that use as alternative reference areas the number of antidepressant prescriptions written by providers practicing 0 to 5 miles from 1) nonshooting schools with the highest predicted probability of experiencing a shooting based on school characteristics ("alternative reference areas A") and 2) nonshooting schools matched individually to each school that experienced a shooting based on observable characteristics ("alternative reference areas B").[‖] As shown in *SI Appendix*, section C, analyses using these alternative reference areas yield estimates that are slightly smaller, but statistically indistinguishable, from our baseline model. At the same time, Table 1 shows that our primary reference areas are most similar to the treatment areas in terms of antidepressant prescription rates prior to fatal school shootings, supporting our choice of baseline model.

Second, we assess the robustness of our results to defining treatment areas using distances of 0 to 1 miles, 0 to 2 miles, …, 0 to 9 miles between providers and schools. As shown in *SI Appendix*, Fig. S5, the effects become more precise as we include providers practicing in larger areas. The estimated effect peaks when defining the treatment area as 0 to 4 miles from a school, but the point estimate is very similar and not statistically different for our primary definition of 0 to 5 miles. As expected, the effects decline as we add providers practicing farther away.

By examining the effects of 44 shootings at primary and secondary schools across the United States, this study provides the largest analysis to date of the impacts of school shootings on youth mental health. While fortunately only 15 of these shootings resulted in any victim deaths, this limited number of events poses statistical challenges when estimating effects separately for fatal and nonfatal shootings. We conduct two additional analyses to address these concerns. First, to verify that no single event is driving our results, we examine the sensitivity of our estimates to dropping each school that experienced a shooting. As shown in *SI Appendix*, Fig. S6, our estimates are very stable regardless of which school is excluded. Second, our primary specifications cluster standard errors at the school-by-area level. As inference with a small number of clusters may be biased, we also present results using a wild cluster bootstrap in *SI Appendix*, Table S5 (38). The results remain statistically significant at conventional levels.

Increased antidepressant use following a school shooting could be driven either by increased incidence of mental illness or by increased treatment of existing pathology. Although we cannot definitely identify the relative contributions of these mechanisms, three findings suggest that new pathology is likely a contributing factor. First, to confirm that our results are not driven by changes in local prescribing behavior or interactions with the health care system, we examine the effects of school shootings on the other class of prescriptions available in our data extract: opioids.[**] As shown in *SI Appendix*, Fig. S7, we find no effects of school shootings on opioid prescriptions written for youth. Second,

[#]The null effects of nonfatal school shootings are consistent with work underscoring the complex and nonlinear ways in which exposure to trauma influences well-being (e.g., refs. 36 and 37). It is also possible that nonfatal shootings affect other outcomes that are not observed in our data.

[‖]See *SI Appendix*, section C for details on the construction of these alternative reference areas.

[**]Opioid prescriptions for youth are nearly as common as antidepressants: According to our data, there were 10.3 million antidepressant prescriptions for individuals under age 20 across the entire United States in 2010 compared to 9.1 million opioid prescriptions. There is therefore scope for opioid use to respond if individuals were to have greater interaction with the health care system.

to ensure that our results are not driven by changes in treatment for underlying mental health problems, we examine the effects of school shootings on antidepressant use among adults.[††] As shown in *SI Appendix*, Fig. S8, we find no evidence that antidepressant prescriptions for individuals aged 20 and older increase following a local school shooting. Third, as already discussed, we find no differences in effects across areas with differing densities of prescribing providers. If school shootings only lead to treatment of previously untreated pathology, then the effects should be larger in areas with greater undertreatment using medication in the preperiod—that is, areas with fewer prescribers.

One concern with our analysis of heterogeneous effects across areas with different densities of mental health providers is that provider densities are not randomly assigned. It is therefore possible that our findings are driven by other differences across locations that are correlated with the availability of mental health care resources. In particular, if school shootings were less severe (e.g., had fewer fatalities) or individuals were less likely to be insured in places with a higher density of nonprescribing providers, then we could observe smaller effects on antidepressant use in those areas simply due to differences in the severity of the event or access to health care resources. If anything, however, our data indicate that school shootings in counties with a higher density of nonprescribing providers result in slightly more victim fatalities ($r = 0.13$), and controlling for local insurance rates does not affect our estimates (*SI Appendix*, Table S6). While additional factors, such as income, may also correlate with cross-sectional variation in mental health resources, we note that confounding factors must be correlated with the density of nonprescribing providers conditional on the density of prescribing providers.

While sizable, the increases in antidepressant use that we document are unlikely to capture the full mental health consequences of school shootings. If local exposure to school shootings increases the use of nonpharmacological treatment, the use of pharmacological treatment with medications other than antidepressants, or the prevalence of untreated mental illness, then our estimates will underestimate the full effects of these events. Furthermore, to the extent that school shootings impact students' ability to feel secure at school, exposure to these events could lead to worse behavioral, educational, and economic trajectories. As communities continue to grapple with the aftermath of school shootings, more research is needed to assess the full costs of these events for surviving youth.

Materials and Methods

IQVIA Xponent Data. Data on antidepressant prescriptions from January 2006 to March 2015 come from the IQVIA Xponent database. IQVIA obtains these data directly from over 90% of all retail pharmacies and imputes prescriptions for missing pharmacies to match industry totals. For each prescriber, the data contain the number of antidepressant prescriptions written in each month to individuals in binned age groups. Age is provided in the following bins: 0 to 2, 3 to 9, 10 to 19, 20 to 39, 40 to 59, 60 to 64, 65 to 74, 75 to 84, and 85+. The data further contain prescriber practice addresses in 2014 from the American Medical Association.

Washington Post Data. The *Washington Post* school shootings database contains information on acts of gunfire at primary and secondary schools since 1999. The database excludes shootings at after-hours events, accidental discharges that caused no injuries to bystanders, and suicides that posed no threat to other students. The database is updated as facts emerge about individual cases; the version of the database used in this paper is from June 20, 2018. For each shooting, the data include the date, the school's name and address, the number of victims who were killed or injured, the gender and age of the shooter(s), and whether or not the shooter(s) died. The data further contain characteristics and basic student sociodemographics for the affected schools.

Supplementary Data. To construct population measures, we aggregate block group-level population counts from the 2010 census across all block groups within 0 to 5 miles and 10 to 15 miles of each school. The census reports population counts for individuals aged 0 to 17 and individuals aged 18 and above. For each area, we estimate the population under age 20 by assuming that the population of individuals aged 18 and 19 is 2/18 of the population aged 0 to 17.

To select schools that are observationally similar to schools that experienced a shooting as alternative reference groups, we use information on school and district characteristics for all primary and secondary schools in the United States for the 2009–2010 school year from the Stanford Education Data Archive (see *SI Appendix*, section C for additional details).

To examine heterogeneity in effects by mental health care resources, crime rates, and health insurance coverage rates, we use data from the Centers for Disease Control and Prevention (CDC), the Uniform Crime Reporting Program (UCR), and the American Community Survey (ACS), respectively. For each county, the CDC data provide the number of pediatricians, family physicians, psychiatrists, psychologists, and licensed social workers in 2015 per 1,000 children aged 0 to 17; the UCR data provide the number of murders, rapes, robberies, and aggravated assaults in 2006 per 1,000 individuals; and the ACS data provide the percent of individuals under age 65 who had health insurance coverage in 2008.

Sample Selection. We consider the 48 school shootings that occurred between January 2008 and April 2013. We only keep data for the first shooting that occurred at a given school since 1999 (46 school shootings over our sample window) and for schools with at least one antidepressant prescription written to an individual under age 20 by a provider in the treatment area in each of the relevant 48 mo (44 school shootings). The school shootings included in our main analysis are listed in *SI Appendix*, Table S1.

Statistical Analysis. We estimate the following equation separately for fatal and nonfatal school shootings in our primary analysis:

$$ln(RX_{sta}) = \beta_0 + \beta_1 Post_t + \beta_2 Post_t \times \text{Treat}_a + \sigma_t + \delta_s \times \text{Treat}_a + \epsilon_{sta}, \quad [1]$$

where $ln(RX_{sta})$ denotes the natural log of the number of antidepressant prescriptions written to individuals under age 20 in area a of school s in month t, $Post_t$ is an indicator that equals one for months in or after a school shooting and zero otherwise, Treat_a is an indicator that equals one for treatment areas (0 to 5 miles from a school) and zero for reference areas (10 to 15 miles from a school), σ_t is a vector of month-by-year fixed effects, $\delta_s \times \text{Treat}_a$ is a vector of school-by-area fixed effects, and ϵ_{sta} is an error term. We cluster standard errors at the school-by-area level and weight the regressions by school enrollment. When estimating quarterly event study analogs, we define the shooting quarter as the 3-mo period starting with the month of the shooting. Specifications using alternative reference areas are outlined in *SI Appendix*, section C.

Data Availability. The IQVIA Xponent database is proprietary; interested researchers should contact IQVIA to inquire about purchasing the data. All other datasets used in this study are publicly available online. See *SI Appendix*, section E for additional details and download links.

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[††] Of course, the mental health of adults could also be directly affected by school shootings.

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Supporting Information

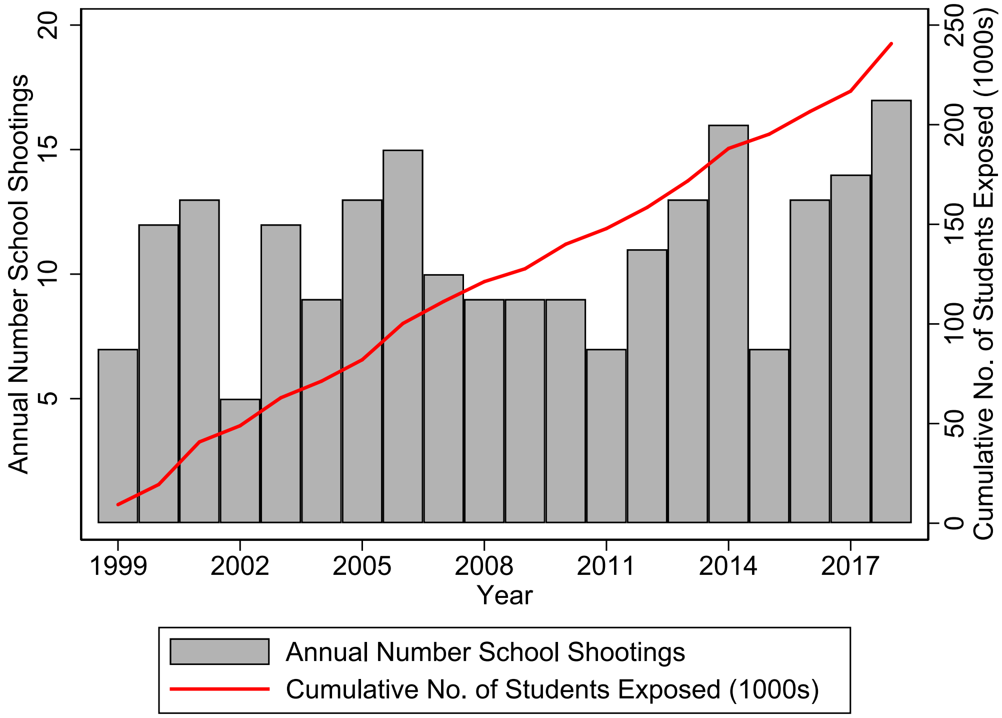
Local Exposure to School Shootings and Youth Antidepressant Use

Rossin-Slater, Schnell, Schwandt, Trejo, and Uniat (2020)

www.pnas.org/cgi/doi/10.1073/pnas.2000804117

A Supplementary Figures

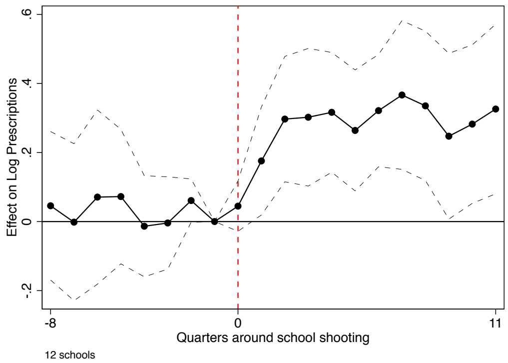
Figure S1: Shootings at U.S. Primary and Secondary Schools: April 1999–May 2018



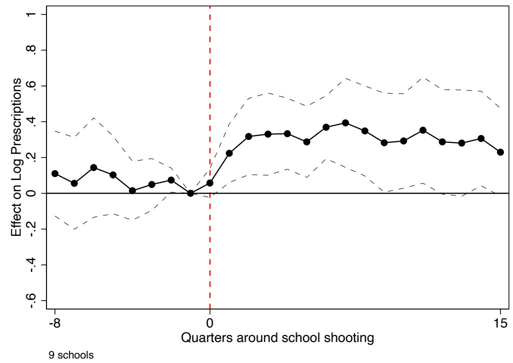
Notes: The bars depict the number of school shootings at primary and secondary schools in the United States in each calendar year over the period April 1999–May 2018; the line depicts the cumulative number of students who were enrolled in schools that experienced shootings over the same time period. Our main analysis uses data on school shootings between January 2008 and April 2013. Source: *Washington Post* database on school shootings, downloaded on June 20, 2018.

Figure S2: Effects of Fatal School Shootings on Youth Antidepressant Use: 3–6 Year Follow-Up Windows

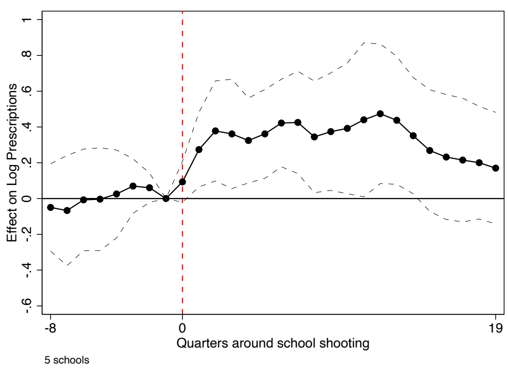
(a) 3-Year Follow-Up



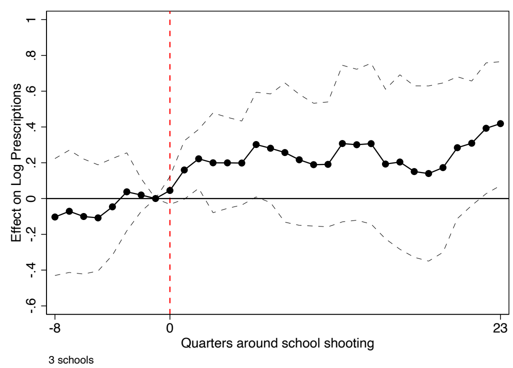
(b) 4-Year Follow-Up



(c) 5-Year Follow-Up



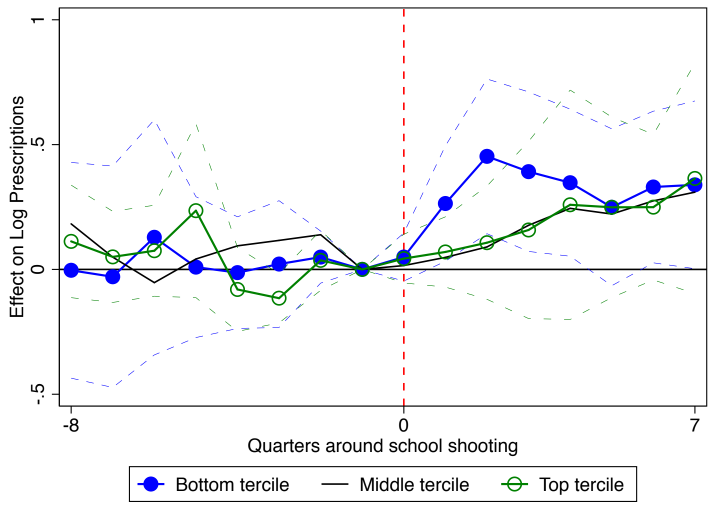
(d) 6-Year Follow-Up



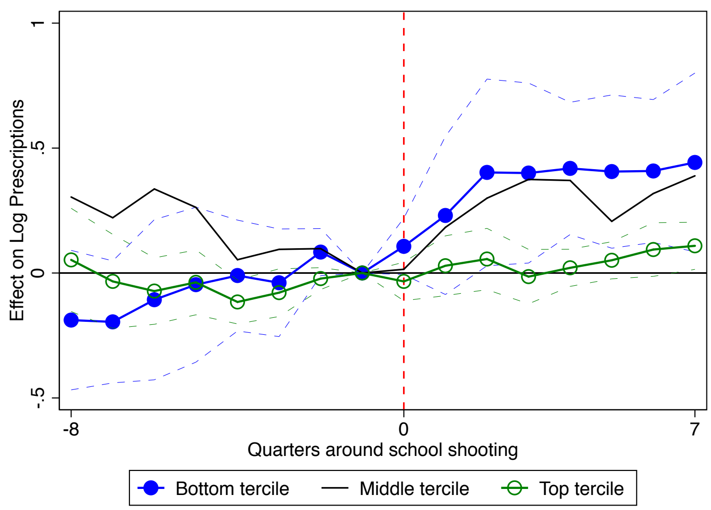
Notes: The above figures present output from estimation of our event study specification using alternative follow-up windows. The number of schools used in the estimation is denoted under each graph. We regress log antidepressant prescriptions for individuals under age 20 at the school–area–month level on quarterly event time indicators, quarterly event time indicators interacted with an indicator denoting treatment areas, month-by-year fixed effects, and school-by-area fixed effects. We plot the coefficients and 95% confidence intervals on the interactions between quarterly event time indicators and the indicator denoting treatment areas; these coefficients represent the percentage difference in the number of antidepressant prescriptions written to individuals under age 20 between the treatment and reference areas in each of the quarters surrounding a school shooting relative to the quarter before the shooting. The treatment (reference) areas include providers practicing 0–5 (10–15) miles from an affected school. All regressions only consider fatal school shootings and are weighted by school enrollment. Standard errors are clustered at the school-by-area level.

Figure S3: Effects of Fatal School Shootings on Youth Antidepressant Use by Density of Mental Health Care Providers

(a) Density of Prescribers

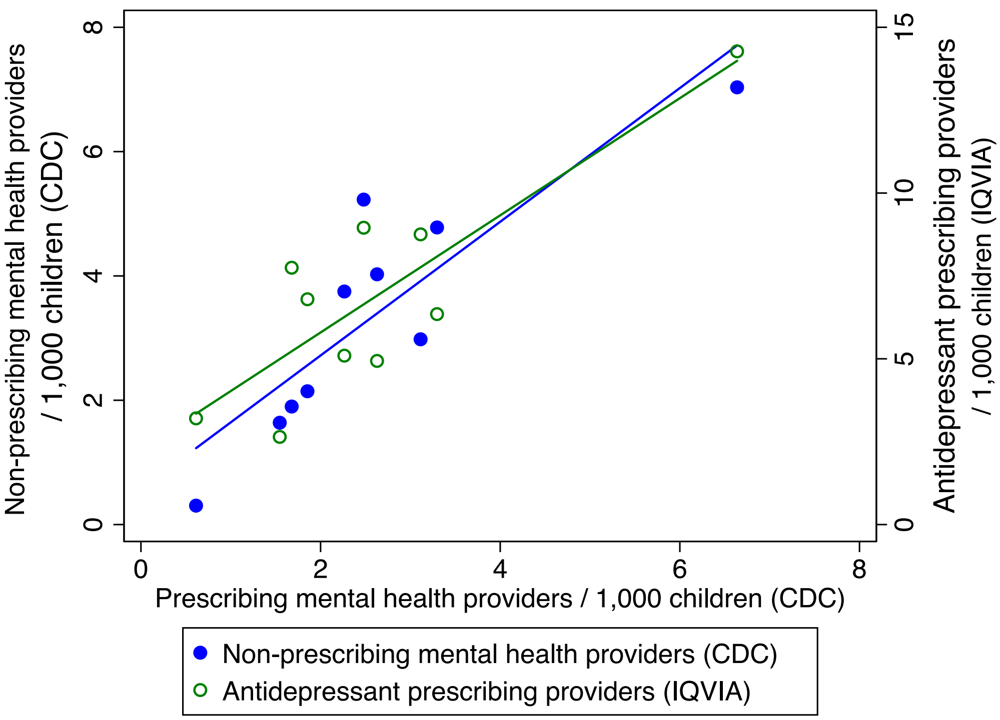


(b) Density of Non-Prescribers



Notes: The above figures present output from estimation of augmented versions of our event study specification. We regress log antidepressant prescriptions for individuals under age 20 at the school–area–month level on quarterly event time indicators; quarterly event time indicators interacted with an indicator denoting treatment areas; interactions between quarterly event time indicators, the treatment indicator, and terciles of county-level measures of the density of child mental health providers; month-by-year fixed effects; and school-by-area fixed effects. We include the density measures separately for "Prescribers" (physicians in family medicine, pediatrics, and psychiatry per 1,000 children aged 0–17; subfigure (a)) and "Non-Prescribers" (psychologists and social workers per 1,000 children aged 0–17; subfigure (b)). We plot the coefficients and 95% confidence intervals on the interactions between quarterly event time indicators, the indicator denoting treatment areas, and terciles of county-level measures of child mental health provider densities; these coefficients represent the percentage difference in the number of antidepressant prescriptions written to individuals under age 20 between the treatment and reference areas in each of the quarters surrounding a school shooting relative to the quarter before the shooting. The treatment (reference) areas include providers practicing 0–5 (10–15) miles from an affected school. All regressions only consider fatal school shootings and are weighted by school enrollment. Standard errors are clustered at the school-by-area level.

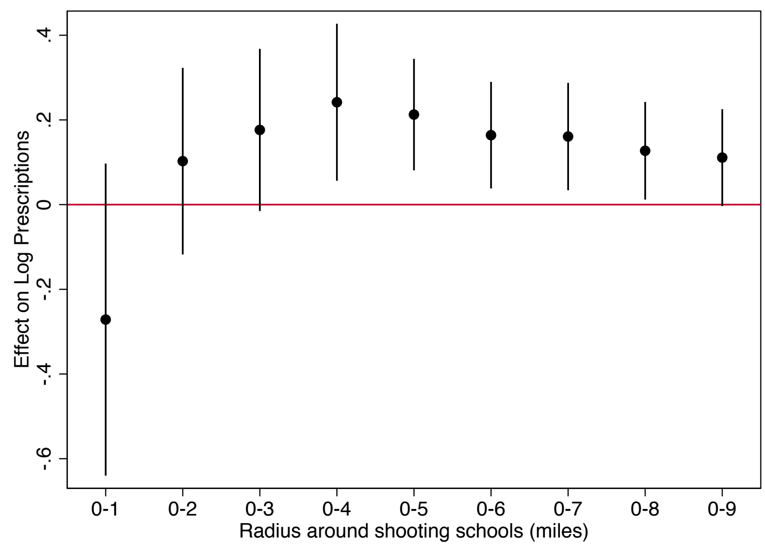
Figure S4: Correlation Between Provider Density Measures



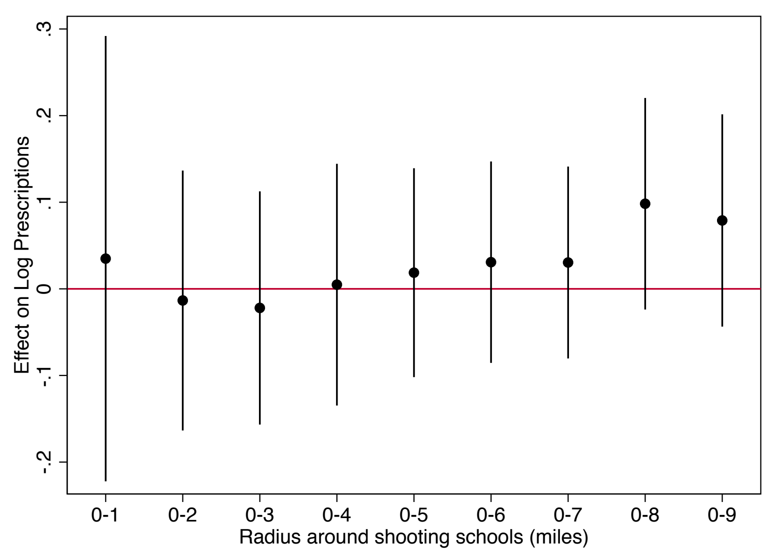
Notes: The above figure depicts the correlation between different measures of local provider density. The x-axis measures the county-level number of child mental health care providers who can prescribe medication (physicians in family medicine, pediatrics, and psychiatry) per 1,000 children aged 0–17 as reported by the CDC. The left y-axis measures the county-level number of child mental health care providers who traditionally cannot prescribe medication (psychologists and social workers) per 1,000 children aged 0–17 as reported by the CDC. The right $y-$axis measures the number of providers in the IQVIA data who prescribed at least one antidepressant to an individual under age 20 in 2010 in each school's treatment area per 1,000 individuals under age 20 living in these areas. The points represent deciles of schools in our main analysis grouped according to densities of prescribers from the CDC; the lines are linear fits of these points.

Figure S5: Sensitivity of Estimates to Varying the Radius Defining Treatment Areas

(a) Fatal Shootings



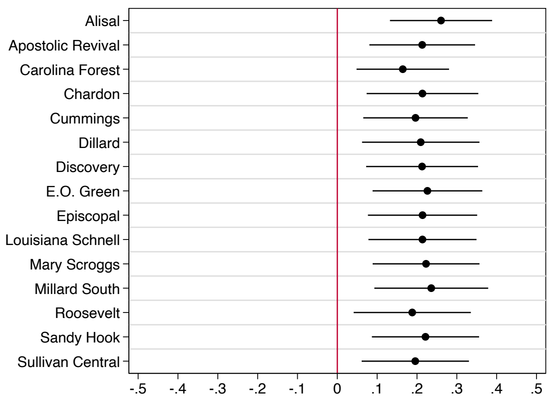
(b) Non-Fatal Shootings



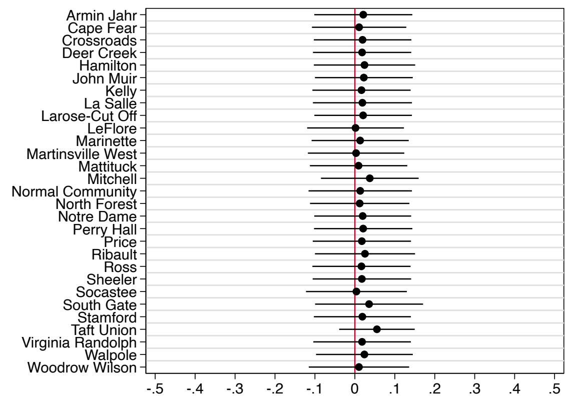
Notes: Each subfigure presents output from estimation of nine separate versions of our difference-in-difference specification in which we vary the definition of treatment areas to include providers located between 0–1 to 0–9 miles of an affected school. We hold the reference areas fixed at providers practicing 10–15 miles from an affected school in all specifications. We run these nine sets of regressions separately for fatal (subfigure (a)) and non-fatal (subfigure (b)) school shootings. We plot the coefficients and 95% confidence intervals on the interaction between the post indicator and the indicator denoting treatment areas; these coefficients represent the percentage difference in the post-shooting change in the number of antidepressant prescriptions written to individuals under age 20 between the treatment and reference areas.

Figure S6: Sensitivity of Estimates to Excluding Each School

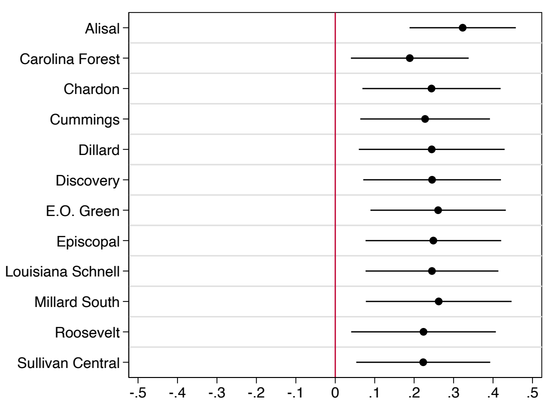
(a) Fatal Shootings, 2-Year Follow-Up



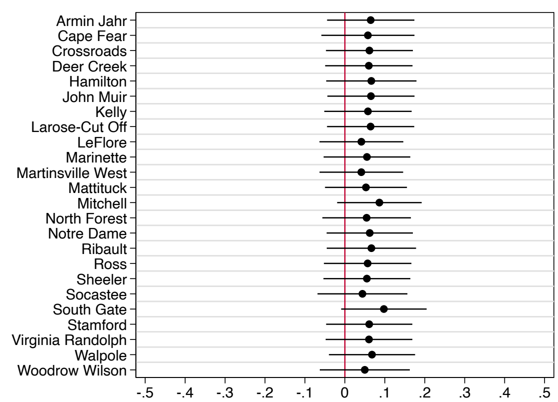
(b) Non-Fatal Shootings, 2-Year Follow-Up



(c) Fatal Shootings, 3-Year Follow-Up



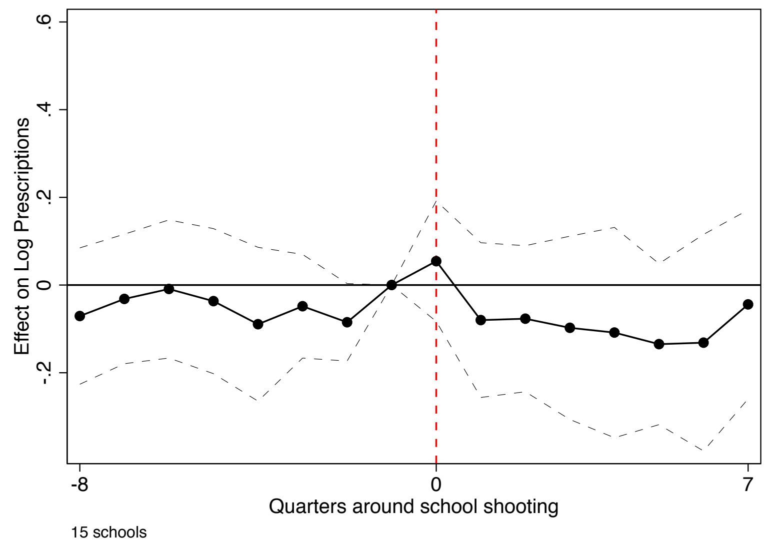
(d) Non-Fatal Shootings, 3-Year Follow-Up



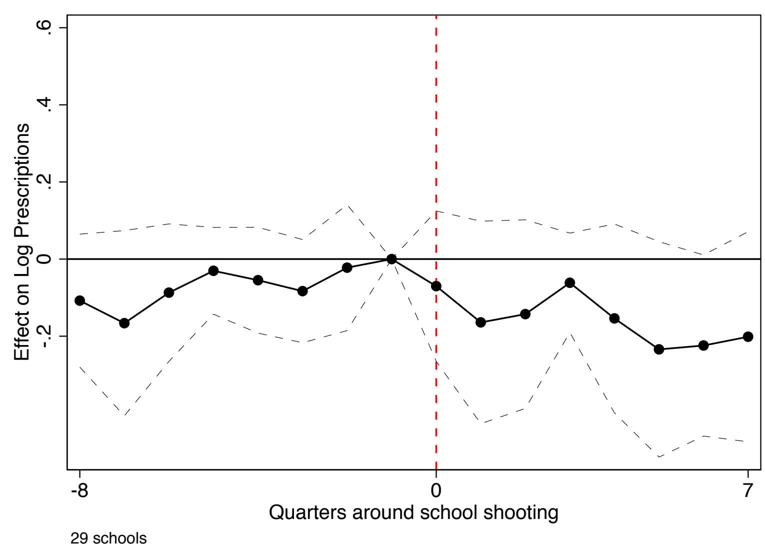
Notes: Each subfigure presents output from estimation of separate versions of our difference-in-difference specification excluding one school at a time. The excluded school is denoting on the y-axis. We run these sets of regressions separately for fatal (subfigures (a) and (c)) and non-fatal (subfigures (b) and (d)) school shootings, using two-year (subfigures (a) and (b)) or three-year (subfigures (c) and (d)) follow-up windows. We plot the coefficients and 95% confidence intervals on the interaction between the post indicator and the indicator denoting treatment areas; these coefficients represent the percentage difference in the post-shooting change in the number of antidepressant prescriptions written to individuals under age 20 between the treatment and reference areas.

Figure S7: Effects of School Shootings on Youth Opioid Use (Placebo)

(a) Fatal Shootings



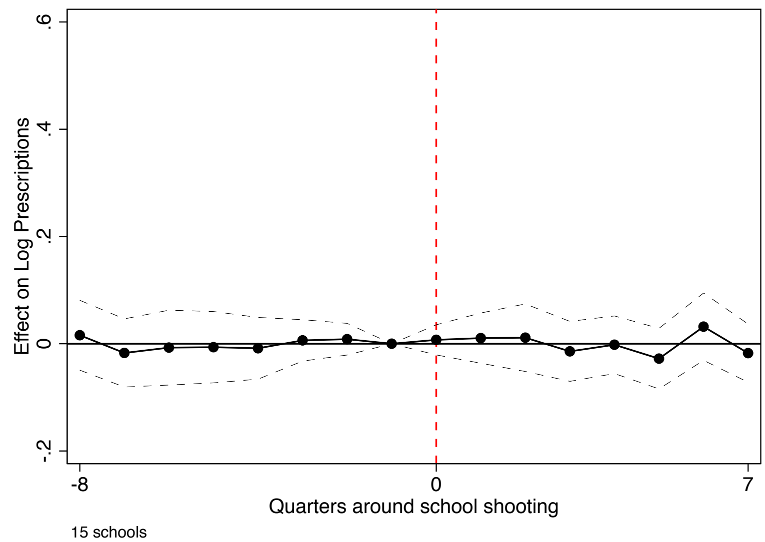
(b) Non-Fatal Shootings



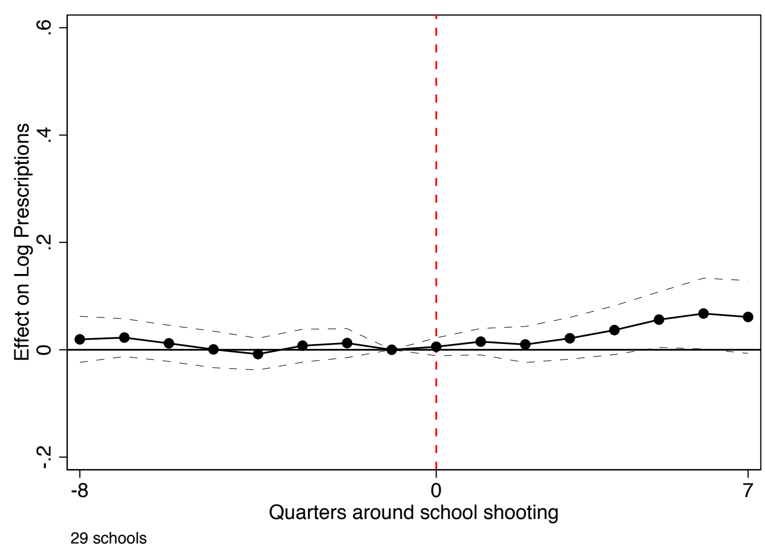
Notes: The above figures present output from estimation of our event study specification using opioid prescriptions as the outcome. We regress log opioid prescriptions for individuals under age 20 at the school–area–month level on quarterly event time indicators, quarterly event time indicators interacted with an indicator denoting treatment areas, month-by-year fixed effects, and school-by-area fixed effects. We run separate regressions for fatal (subfigure (a)) and non-fatal (subfigure (b)) school shootings. We plot the coefficients and 95% confidence intervals on the interactions between quarterly event time indicators and the indicator denoting treatment areas; these coefficients represent the percentage difference in the number of opioid prescriptions written to individuals under age 20 between the treatment and reference areas in each of the quarters surrounding a school shooting relative to the quarter before the shooting. The treatment (reference) areas include providers practicing 0–5 (10–15) miles from an affected school. All regressions are weighted by school enrollment, and standard errors are clustered at the school-by-area level.

Figure S8: Effects of School Shootings on Adult Antidepressant Use

(a) Fatal Shootings



(b) Non-Fatal Shootings



Notes: The above figures present output from estimation of our event study specification using adult antidepressant use as the outcome. We regress log antidepressant prescriptions for individuals aged 20 and older at the school–area–month level on quarterly event time indicators, quarterly event time indicators interacted with an indicator denoting treatment areas, month-by-year fixed effects, and school-by-area fixed effects. We run separate regressions for fatal (subfigure (a)) and non-fatal (subfigure (b)) school shootings. We plot the coefficients and 95% confidence intervals on the interactions between quarterly event time indicators and the indicator denoting treatment areas; these coefficients represent the percentage difference in the number of antidepressant prescriptions written to individuals aged 20 and older between the treatment and reference areas in each of the quarters surrounding a school shooting relative to the quarter before the shooting. The treatment (reference) areas include providers practicing 0–5 (10–15) miles from an affected school. All regressions are weighted by school enrollment, and standard errors are clustered at the school-by-area level.

B Supplementary Tables

Table S1: School Shootings in Main Analysis Sample

School	Date	#Killed	#Injured	Shooter Died?
Alisal High School	Oct 2010	1	0	No
Apostolic Revival Center and Christian School	Jan 2013	1	0	No
Armin Jahr Elementary School	Feb 2012	0	1	No
Cape Fear High School	Oct 2011	0	1	No
Carolina Forest High School	Oct 2009	1	0	No
Chardon High School	Feb 2012	3	3	No
Crossroads Charter High School	Jan 2008	0	1	No
Cummings Middle School	Jan 2012	1	0	No
Deer Creek Middle School	Feb 2010	0	2	No
Dillard High School	Nov 2008	1	0	No
Discovery Middle School	Feb 2010	1	0	No
E.O. Green Junior High School	Feb 2008	1	0	No
Episcopal School of Jacksonville	Mar 2012	1	0	Yes
Hamilton High School	Feb 2008	0	1	No
John Muir Elementary School	Feb 2009	0	0	No
Kelly Elementary School	Oct 2010	0	2	No
La Salle High School	Apr 2013	0	0	No
Larose-Cut Off Middle School	May 2009	0	0	Yes
LeFlore High School	Mar 2012	0	0	No
Louisiana Schnell Elementary School	Feb 2011	1	0	No
Marinette High School	Nov 2010	0	0	Yes
Martinsville West Middle	Mar 2011	0	1	No
Mary Scroggs Elementary School	May 2012	1	0	No
Mattituck Junior-Senior High School	Oct 2009	0	1	No
Millard South High School	Jan 2011	1	2	Yes
Mitchell High School	Feb 2008	0	1	No
Normal Community High School	Sep 2012	0	0	No
North Forest High School	Jan 2012	0	1	No
Notre Dame Elementary School	Feb 2008	0	1	Yes
Perry Hall High School	Aug 2012	0	1	No
Price Middle School	Jan 2013	0	1	No
Ribault High School	Mar 2009	0	0	No
Roosevelt High School	Apr 2008	1	0	No
Ross Elementary School	Apr 2011	0	2	No
Sandy Hook Elementary School	Dec 2012	26	2	Yes
Sheeler Charter High School	Apr 2011	0	1	No
Socastee High School	Sep 2010	0	1	No
South Gate High School	May 2010	0	1	No
Stamford Academy	Sep 2009	0	0	No
Sullivan Central High School	Aug 2010	1	0	No
Taft Union High School	Jan 2013	0	2	No
Virginia Randolph Community High School	Sep 2009	0	0	No
Walpole Elementary School	Feb 2012	0	0	No
Woodrow Wilson High School	Apr 2010	0	0	No

Notes: The above table lists the 44 shootings at U.S. primary and secondary schools included in our main analysis. We include schools that experienced their first shooting since April 1999 over our sample window (January 2008 to April 2013) and had at least one antidepressant prescription written by a provider within five miles of the school in each month in the two years surrounding the shooting. Source: *Washington Post* database on school shootings, downloaded on June 20, 2018.

Table S2: Effects of Fatal School Shootings on Youth Antidepressant Use by Grade Levels

	Baseline		High School Interaction	
	2-Year (1)	3-Year (2)	2-Year (3)	3-Year (4)
Treatment x Post	0.213***	0.245***	0.0659	0.0532
	(0.0643)	(0.0787)	(0.0773)	(0.115)
Treatment x Post x High School			0.249**	0.306**
			(0.109)	(0.138)
Observations	1,412	1,410	1,412	1,410

Notes: The above table reports output from estimation of augmented versions of our difference-in-difference specification. We regress log antidepressant prescriptions for individuals under age 20 at the school–area–month level on an indicator denoting months in or after a school shooting; an interaction between the post indicator and an indicator denoting treatment areas; interactions between the post indicator, the treatment indicator, and an indicator denoting high schools (grades 9–12 only); month-by-year fixed effects; and school-by-area fixed effects. All regressions only consider fatal school shootings and are weighted by school enrollment. Standard errors (reported in parentheses) are clustered at the school-by-area level. For ease of comparison, Columns (1) and (2) replicate our baseline results from Table 2. Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table S3: Effects of Fatal School Shootings on Youth Antidepressant Use by Density of Mental Health Care Providers

	Baseline	Tercile Interactions		
	(1)	(2)	(3)	(4)
Treatment x Post	0.213***	0.290***	0.426***	0.452***
	(0.064)	(0.103)	(0.085)	(0.090)
Prescriber Density				
x Tercile 2		-0.189		-0.129
		(0.113)		(0.097)
x Tercile 3		-0.142		0.067
		(0.151)		(0.149)
Non-Prescriber Density				
x Tercile 2			-0.328**	-0.332**
			(0.122)	(0.124)
x Tercile 3			-0.348***	-0.399***
			(0.114)	(0.140)
Observations	1,412	1,412	1,412	1,412

The above table reports output from estimation of augmented versions of our difference-in-difference specification that include interactions between the post indicator, the treatment indicator, and terciles of county-level measures of the density of child mental health providers. "Prescribers" include physicians in pediatrics, psychiatry, or family medicine; "Non-Prescribers" include psychologists and social workers. All regressions consider fatal school shootings and use a two-year follow-up window. Refer to *SI Appendix*, Fig. S3 for event study analogs, and *SI Appendix*, Fig. S4 for correlations between the density measures. * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table S4: Effects of Fatal School Shootings on Youth Antidepressant Use by Local Area Violent Crime Rates

	Baseline	Linear Interaction	Tercile Interactions
	(1)	(2)	(3)
Treatment x Post	0.213***	0.167**	0.123*
	(0.064)	(0.075)	(0.061)
x Crime Rate		0.019	
		(0.025)	
x Mid-Tercile Crime Rate			0.120
			(0.127)
x Top-Tercile Crime Rate			0.140
			(0.136)
Observations	1,412	1,412	1,412

Notes: The above table reports output from estimation of augmented versions of our difference-in-difference specification. We regress log antidepressant prescriptions for individuals under age 20 at the school–area–month level on an indicator denoting months in or after a school shooting; an interaction between the post indicator and an indicator denoting treatment areas; interactions between the post indicator, the treatment indicator, and county-level measures of violent crime; month-by-year fixed effects; and school-by-area fixed effects. We include crime rates either as a continuous variable (Column (2)) or as indicators denoting terciles across treatment counties (Column (3)). The treatment (reference) areas include providers practicing 0–5 (10–15) miles from an affected school. All regressions only consider fatal school shootings, use a two-year follow-up window, and are weighted by school enrollment. Standard errors (reported in parentheses) are clustered at the school-by-area level. For ease of comparison, Column (1) replicates our baseline results from Table 2. Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table S5: Effects of School Shootings on Youth Antidepressant Use: Wild Cluster Bootstrap

	Fatal Shootings		Non-Fatal Shootings	
	2-Year	3-Year	2-Year	3-Year
	(1)	(2)	(3)	(4)
A. Outcome: ln(Antidepressant Prescriptions)				
Treatment x Post	0.213**	0.245*	0.0187	0.0603
	[p=0.015]	[p=0.050]	[p=0.736]	[p=0.351]
B. Outcome: Antidepressant Prescription Rates per 1,000				
Treatment x Post	1.982**	2.645***	1.674	0.348
	[p=0.012]	[p=0.002]	[p=0.231]	[p=0.584]
Relative to Mean	0.206**	0.297***	0.163	0.037
Number of Schools	15	12	29	24
Observations	1412	1410	2601	2718

Notes: The above table reports output from estimation of our primary difference-in-difference specification. We regress measures of antidepressant prescriptions for individuals under age 20 at the school–area–month level on an indicator denoting months in or after a school shooting ("Post"), an interaction between the post indicator and an indicator denoting treatment areas, month-by-year fixed effects, and school-by-area fixed effects. We run separate regressions for fatal (Columns (1) and (2)) and non-fatal (Columns (3) and (4)) school shootings and include either a two-year (Columns (1) and (3)) or three-year (Columns (2) and (4)) follow-up window. In Panel A, the outcome is the log number of antidepressant prescriptions written to individuals under age 20; the reported coefficient in each column is therefore the percentage difference in the post-shooting change in the number of antidepressant prescriptions written to individuals under age 20 between the treatment and reference areas. In Panel B, the outcome is the antidepressant prescription rate per 1,000 individuals under age 20; the reported coefficient in each column is therefore the difference in the post-shooting change in the antidepressant prescription rate per 1,000 individuals under age 20 between the treatment and reference areas. The third row in Panel B reports the effect size as a proportion of the sample mean of the outcome. The treatment (reference) areas include providers practicing 0–5 (10–15) miles from an affected school. All regressions are weighted by school enrollment. We calculate $p-$values (reported in brackets) using a wild cluster bootstrap. Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table S6: Effects of Fatal School Shootings on Youth Antidepressant Use by Local Area Insurance Rates

	Baseline	Linear interactions			Tercile Interactions		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment x Post	0.213***	1.528*	0.343***	1.283	0.288***	0.426***	0.436***
	(0.064)	(0.797)	(0.090)	(0.760)	(0.104)	(0.085)	(0.093)
x Insurance Rate		-0.016*		-0.012			
		(0.009)		(0.009)			
x Non-Prescriber Density			-0.039**	-0.031**			
			(0.017)	(0.013)			
x Mid-Tercile Insurance Rate					-0.117		0.035
					(0.155)		(0.142)
x Top-Tercile Insurance Rate					-0.200*		-0.097
					(0.112)		(0.089)
x Mid-Tercile Non-Prescriber Density						-0.328**	-0.338**
						(0.122)	(0.128)
x Top-Tercile Non-Prescriber Density						-0.348***	-0.313***
						(0.114)	(0.102)
Observations	1,412	1,412	1,412	1,412	1,412	1,412	1,412

Notes: The above table reports output from estimation of augmented versions of our difference-in-difference specification. We regress log antidepressant prescriptions for individuals under age 20 at the school–area–month level on an indicator denoting months in or after a school shooting; an interaction between the post indicator and an indicator denoting treatment areas; interactions between the post indicator, the treatment indicator, and county-level measures of the share of the population under age 65 that has health insurance or county-level measures of the density of non-prescribing child mental health providers; month-by-year fixed effects; and school-by-area fixed effects. We include insurance rates and provider densities either as continuous variables (Column (2)-(4)) or as indicators denoting terciles across treatment counties (Column (5)-(7)). The treatment (reference) areas include providers practicing 0–5 (10–15) miles from an affected school. All regressions only consider fatal school shootings, use a two-year follow-up window, and are weighted by school enrollment. Standard errors (reported in parentheses) are clustered at the school-by-area level. For ease of comparison, Column (1) replicates our baseline results from Table 2. Significance levels: * p<0.1 ** p<0.05 *** p<0.01

C Alternative Reference Areas

Our baseline specification compares the number of antidepressant prescriptions written by providers practicing 0–5 miles from a school that experienced a shooting (treatment areas) to the number of antidepressant prescriptions written by providers practicing 10–15 miles away (reference areas). In this section, we examine the robustness of our results to using two alternative sets of reference areas.

The first set of alternative reference areas consists of antidepressant prescriptions written by providers practicing 0–5 miles from non-shooting schools that had the highest predicted probability of a shooting based on observable characteristics ("alternative reference areas A"). Specifically, we consider all primary and secondary schools in the United States and estimate a logistic regression of an indicator denoting whether each school experienced a shooting since 1999 on a range of school and district-level characteristics (see Table S7 below). We then select the 100 schools with the highest shooting propensities that are not in the same district as any school that experienced a shooting, separately for fatal and non-fatal shootings.

The second set of alternative reference areas consists of antidepressant prescriptions written by providers practicing 0–5 miles from non-shooting schools that were directly matched to schools that experienced a shooting based on observables ("alternative reference areas B"). In particular, we matched each shooting school to two non-shooting schools by doing an exact match on indicators for rural area, high school only, and private school. We then implemented a "nearest-neighbor" match procedure on the share of non-Hispanic white students, total enrollment, per-pupil expenditures, and the share of students receiving free or reduced-price lunch.

Table S8 below shows mean characteristics for both the treatment schools and for schools used to define alternative reference areas A and B. Alternative reference area B schools (Columns (3) and (6)) are more closely matched on observable characteristics to the shooting schools (Columns (1) and (4)) than alternative reference area A schools (Columns (2) and

(5)). The fact that alternative reference area A schools are less well matched suggests that shootings are relatively random events that are not well predicted by schools' observable characteristics. We believe that this provides further credibility to our research design that relies on variation in the exact timing of school shootings being exogenous to our outcomes of interest.

Panel C of Table 1 in the main paper shows mean antidepressant prescription rates across the treatment and reference areas. Out of all three reference areas, antidepressant use preceding a fatal school shooting in the treatments areas is most similar to antidepressant use in the primary reference areas.[1] This is a key reason why we prefer our primary reference areas and refer to this specification as the baseline model in the paper.

For analyses using these alternative reference areas, we estimate:

$$ln(RX_{st}) = \beta_0 + \beta_1 Post_t + \beta_2 Post_t \times Treat_s + \sigma_t + \gamma_s + \epsilon_{st} \tag{1}$$

where variables are defined analogously to Equation (1) in the Materials & Methods section of the main paper.[2] We cluster standard errors by school and weight the regressions by school enrollment.

Our results are robust to using either set of alternative reference areas. In particular, Fig. S9 below compares results from event-study specifications that use either our primary reference areas or one of the two alternative reference areas. Results are statistically indistinguishable across models. Furthermore, Table S9 below compares results from difference-in-difference regressions that use each of the three different reference areas. Again, the results are very similar.

[1] Note that we cannot distinguish pre- and post-shooting antidepressant rates for alternative reference area A. We collectively match all shooting schools to a set of non-shooting schools rather than matching shooting and non-shooting schools one-to-one, and thus there is no shooting date assigned to the non-shooting schools.

[2] As noted above, the method used to select non-shooting schools for alternative reference area A does not assign a shooting date to control schools. Since "Post" therefore equals zero for all observations for the control schools, "Post × Treat" is excluded from regressions using alternative reference areas A.

Table S7: Predicting School Shootings

Dep Var: Indicator Denoting School Shooting	Any		Fatal		Non-Fatal	
	(1)		(2)		(3)	
Suburban	-0.474**	(0.183)	-0.702**	(0.218)	0.177	(0.356)
Town	-0.291	(0.252)	-0.261	(0.284)	-0.332	(0.546)
Rural	-0.485*	(0.239)	-0.738*	(0.294)	0.145	(0.440)
Per Pupil Expenditures ($10,000s)	-0.022	(0.450)	-0.178	(0.521)	0.410	(0.870)
Per Pupil Instructor Expenditures ($10,000s)	-1.370	(0.884)	-1.125	(1.005)	-2.235	(1.804)
District Socioeconomic Status	-0.189	(0.129)	-0.191	(0.152)	-0.165	(0.245)
District 3rd-8th Academic Achievement (Mean)	0.231	(0.410)	0.537	(0.484)	-0.652	(0.770)
District 3rd-8th Academic Achievement (Slope)	-2.451	(1.844)	-2.030	(2.108)	-4.503	(3.699)
Total Enrollment (1000s)	0.816**	(0.085)	0.728**	(0.100)	0.982**	(0.127)
Private School	-0.898**	(0.322)	-1.229**	(0.430)	-0.305	(0.499)
Share White Students	-2.594**	(0.606)	-2.912**	(0.724)	-1.888*	(1.076)
Share Black Students	0.015	(0.567)	0.179	(0.670)	-0.664	(1.054)
Share Hispanic Students	-2.014**	(0.616)	-2.190**	(0.733)	-1.464	(1.093)
Share Asian Students	-6.292**	(1.840)	-5.213**	(1.924)	-10.488*	(4.707)
Share Free/Reduced Price Lunch	-1.391**	(0.430)	-1.223*	(0.503)	-2.110**	(0.815)
High School Only	1.424**	(0.159)	1.614**	(0.185)	0.962**	(0.297)
Constant	-3.581**	(0.678)	-3.749**	(0.796)	-5.099**	(1.272)
R-Squared	0.153		0.166		0.110	
Number Schools	117306		117250		117148	

Notes: The above table reports output from logistic regressions of an indicator denoting whether a school experienced a school shooting since 1999 on a range of school and district-level characteristics. The sample includes all primary and secondary schools in the United States. We run separate regressions for all (Columns (1)), fatal (Column (2)), and non-fatal (Columns (3)) school shootings. Note that the race/ethnicity categories are not mutually exclusive. Standard errors are reported in parentheses. Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table S8: Characteristics for Shooting Schools and Matched Non-Shooting Schools

	Fatal			Non-Fatal		
	Shooting Schools	Alternative Reference A Schools	Alternative Reference B Schools	Shooting Schools	Alternative Reference A Schools	Alternative Reference B Schools
	(1)	(2)	(3)	(4)	(5)	(6)
Total Enrollment (1000s)	1.22	3.38	1.21	0.90	2.27	0.9
Private School	0.13	0.00	0.13	0.07	0.00	0.07
High School	0.40	0.94	0.40	0.55	0.98	0.55
Share White	0.49	0.25	0.48	0.43	0.11	0.43
Share Black	0.17	0.24	0.26	0.37	0.71	0.28
Share Free/Reduced Lunch	0.43	0.56	0.44	0.51	0.62	0.51
Number of Schools	15	70	30	29	100	57

Notes: The above table reports average characteristics for shooting and non-shooting schools in our two alternative reference areas, separately for schools in the fatal and non-fatal shooting analyses. "Alternative Reference A Schools" are schools with the highest predicted probability of a shooting based on their school characteristics. "Alternative Reference B Schools" were matched to schools that experienced a shooting based on observable characteristics. See the text for more details.

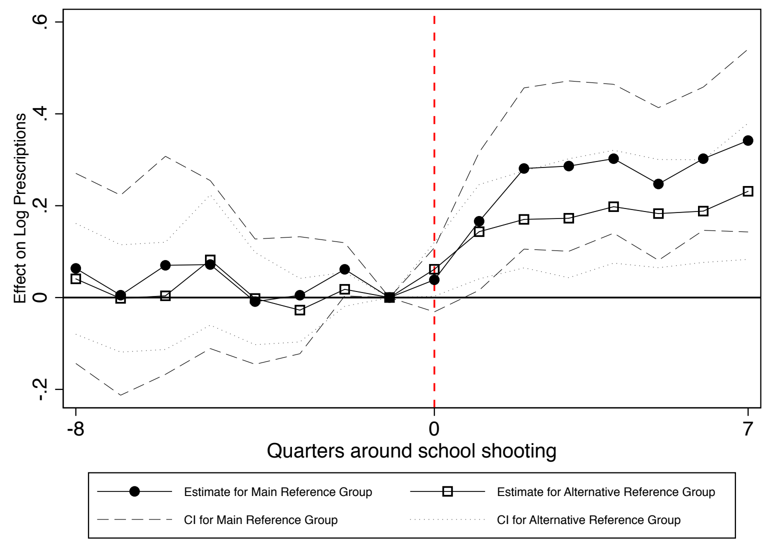
Table S9: Effects of School Shootings on Youth Antidepressant Use: Alternative Reference Areas

	Fatal Shootings		Non-Fatal Shootings	
	2-Year	3-Year	2-Year	3-Year
	(1)	(2)	(3)	(4)
A. Primary Reference Areas				
Treatment x Post	0.213***	0.245***	0.0187	0.0603
	(0.064)	(0.079)	(0.060)	(0.053)
Schools	15	12	29	24
Observations	1,412	1,410	2,601	2,718
B. Alternative Reference Areas A				
Treatment x Post	0.154***	0.165***	-0.0145	0.00192
	(0.0450)	(0.0449)	(0.0521)	(0.0461)
Schools	114	112	129	124
Observations	11,643	11,547	12,326	12,086
C. Alternative Reference Areas B				
Treatment x Post	0.156***	0.183***	0.0105	0.0558
	(0.0516)	(0.0563)	(0.0610)	(0.0625)
Schools	43	41	84	79
Observations	2,064	2,388	4,009	4,608

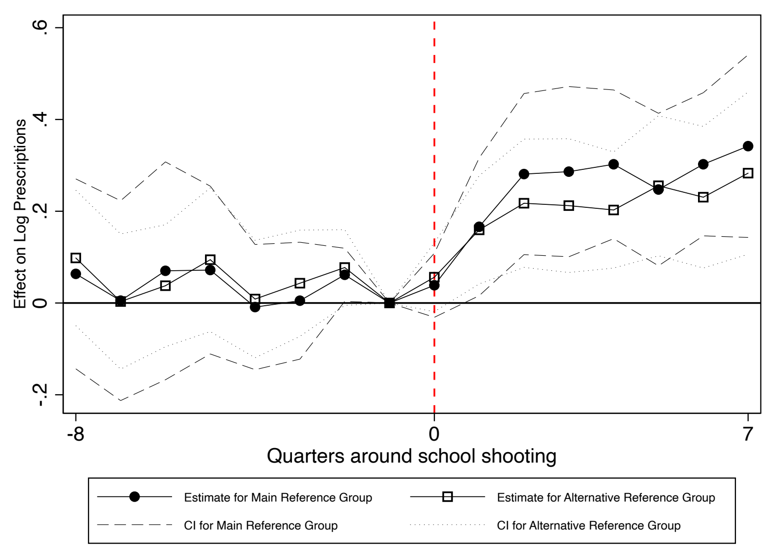
Notes: The above table reports output from the estimation of our primary difference-in-difference specification (Panel A) and difference-in-difference specifications using alternative reference areas (Panels B and C). In Panel B, the alternative reference group consists of providers practicing 0–5 miles from the 100 non-shooting schools that had the highest predicted probability of a shooting based on observable characteristics. In Panel C, the alternative reference group consists of providers practicing 0–5 miles from non-shooting schools that were directly matched to the shooting schools based on observable characteristics. Using these alternative reference areas, we regress log monthly antidepressant prescriptions written for individuals under age 20 by providers practicing 0–5 miles from a school on an indicator denoting months in or after a school shooting, month-by-year fixed effects, and school fixed effects. We run separate regressions for fatal (Columns (1) - (2)) and non-fatal (Columns (3) and (4)) school shootings and include either a two-year (Columns (1) and (3)) or three-year (Columns (2) and (4)) follow-up window. The reported coefficient in each column represents the percentage difference in the post-shooting change in the number of antidepressant prescriptions written to individuals under age 20 between the treatment and reference areas. All regressions are weighted by enrollment of the treatment schools. Standard errors (reported in parentheses) are clustered at the school-by-area level in Panel A and at the school level in Panels B and C. Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Figure S9: Sensitivity of Estimates to Using Alternative Reference Areas

(a) Fatal Shootings, Alternative Reference Areas A



(b) Fatal Shootings, Alternative Reference Areas B



Notes: The above figures present overlaid output from estimation of our primary event study specification and event studies using alternative reference areas. In subfigure (a), the alternative reference group consists of providers practicing 0–5 miles from the 100 non-shooting schools that had the highest predicted probability of a shooting based on observable characteristics. In subfigure (b), the alternative reference group consists of providers practicing 0–5 miles from non-shooting schools that were directly matched to the shooting schools based on observable characteristics. For each specification, we plot coefficients and 95% confidence intervals that represent the percentage difference in the post-shooting change in the number of antidepressant prescriptions written to individuals under age 20 between the treatment and reference areas. All regressions only consider fatal school shootings and are weighted by enrollment of the treatment schools. Standard errors are clustered at the school-by-area level in our main specification and at the school level when using the alternative reference areas.

D School Attendance Boundaries

We obtain data from the 2013–2014 School Attendance Boundary Survey to calculate average school attendance areas for schools included in our analysis. Out of the 44 schools included in our sample, 25 have valid school attendance boundary data (note that the district response rate to the survey was around 75 percent). For these 25 schools, the average school attendance area is approximately 80 square miles.

When we use a 5-mile radius to define treatment areas, we obtain an area size of $\pi * 5^2 = 78.5$ square miles. Thus, our treatment areas are likely to include most students who reside within the shooting schools' attendance boundaries (and who may therefore also see providers who are located in these areas). By contrast, our reference areas of 10–15 miles from schools that experienced a shooting are unlikely to include students who reside within the attendance boundaries of those schools.

E Data Availability

Prescription data

Access to the IQVIA Xponent database is restricted to researchers with data use agreements only. Interested researchers may contact IQVIA to inquire about purchasing the data at:
https://www.iqvia.com/solutions/real-world-evidence/real-world-data-and-insights

School shootings data

The *Washington Post* school shootings database can be downloaded at:
https://github.com/washingtonpost/data-school-shootings

Population data

Census block group–level population counts from the census can be downloaded at:
https://www.socialexplorer.com

School and district characteristics

Information on school and district characteristics from the Stanford Education Data Archive can be downloaded at:
https://exhibits.stanford.edu/data/catalog/db586ns4974

Mental health care resources

Data on county-level mental health care resources from the Centers for Disease Control and Prevention can be downloaded at:
https://www.cdc.gov/childrensmentalhealth/stateprofiles-providers.html

Crime rates

Data on county-level violent crime rates from the Uniform Crime Reporting Program can be downloaded at:
https://www.icpsr.umich.edu/icpsrweb/NACJD/studies/23780

Health insurance rates

Data on county-level health insurance coverage rates from the American Community Survey can be downloaded at:
https://www.socialexplorer.com